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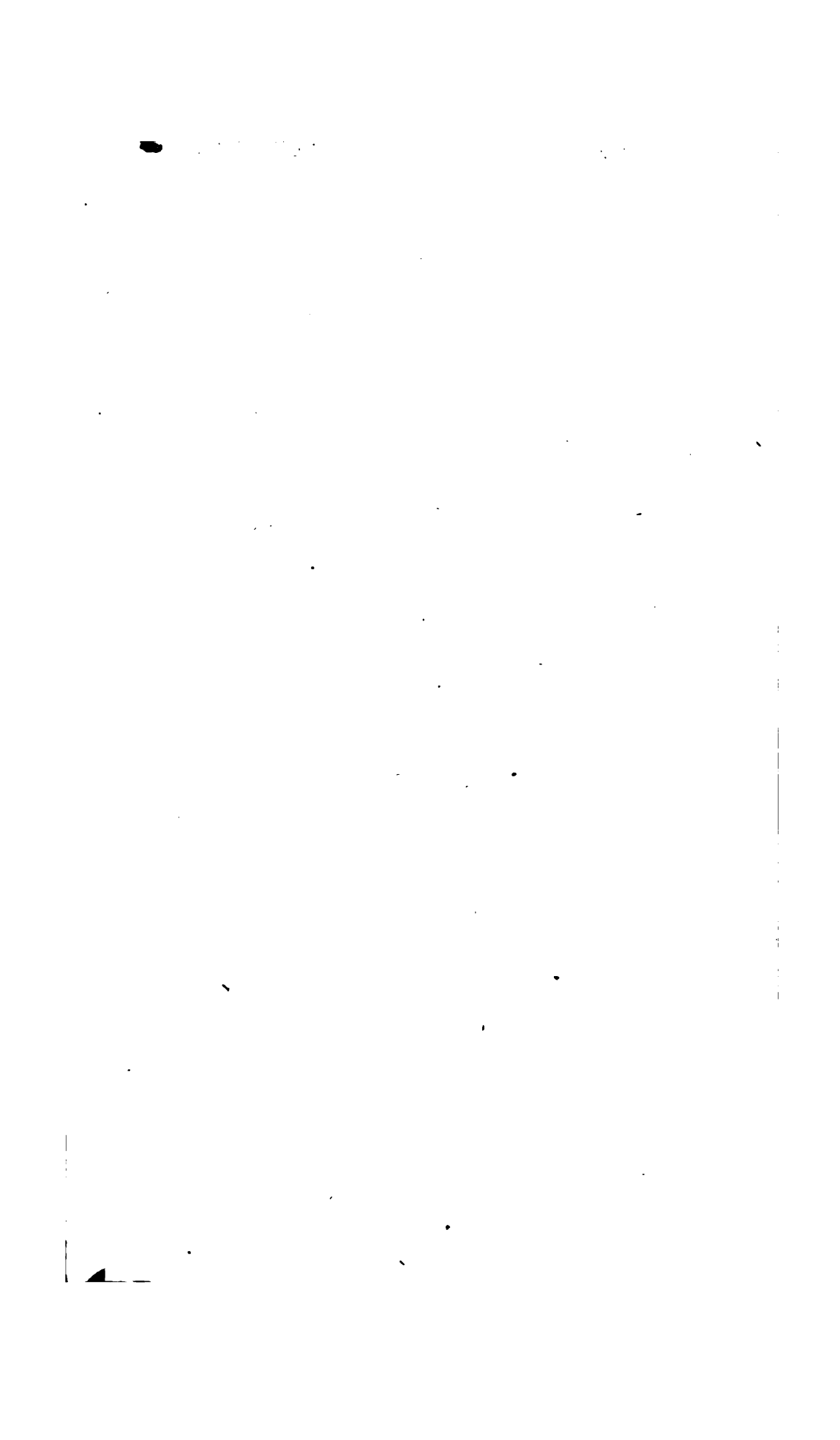
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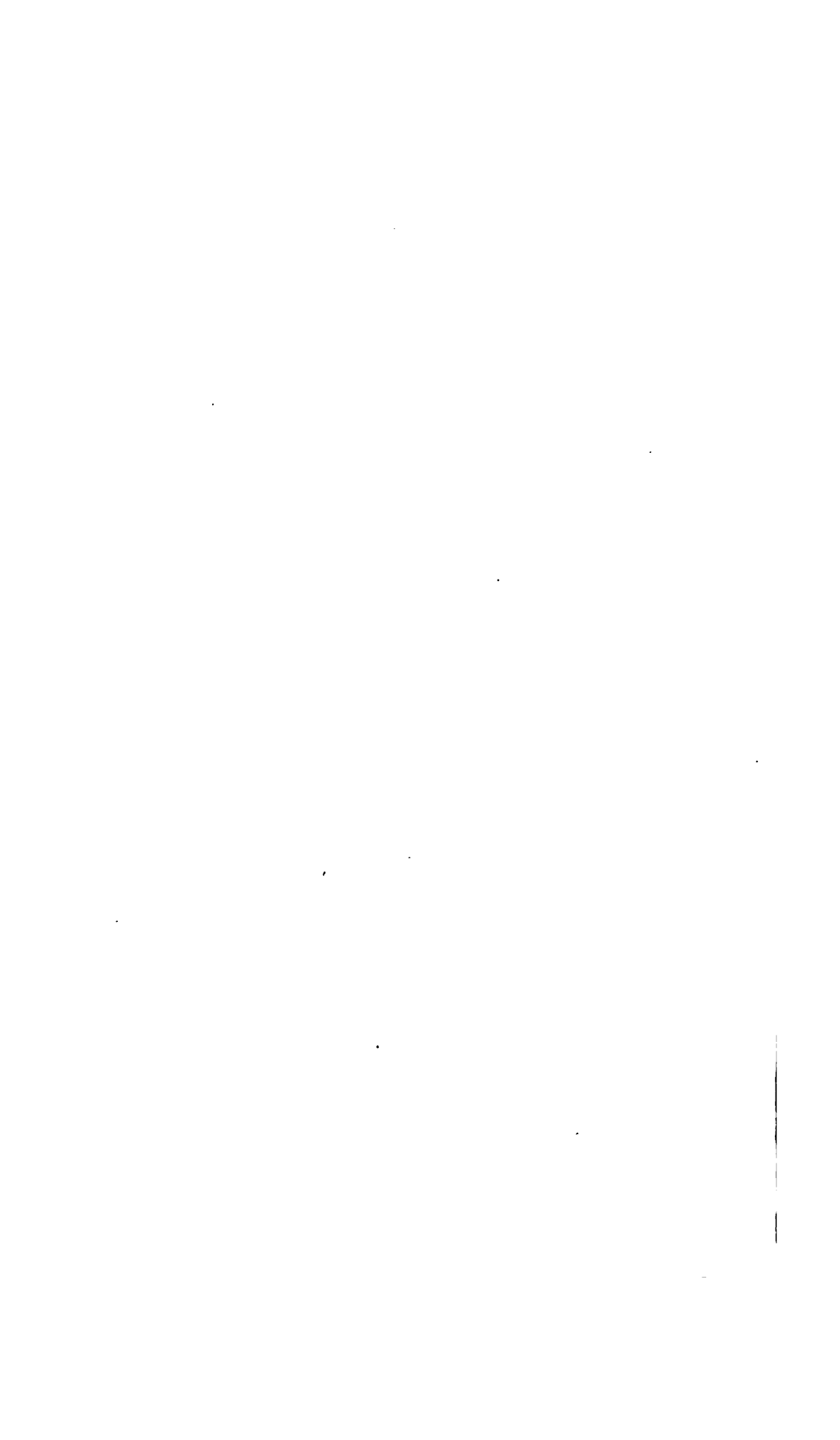




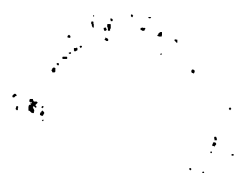












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JULY, 1850.

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PART FIRST.  
*Analytical and Critical Reviews.*

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ART. I.

1. *An Essay on the Principles of Circumstantial Evidence, illustrated by numerous Cases.* By WILLIAM WILLS, Esq. Third Edition.—London, 1850. 8vo., pp. 255.
2. *Report of the Trial of Professor John W. Webster, indicted for the Murder of Dr. George Parkman, before the Supreme Judicial Court of Massachusetts, holden at Boston, on Tuesday, March 19th, 1850.*—Boston, 1850. 8vo., pp. 314.

ON the 19th of March, 1850, a trial commenced before the Supreme Judicial Court of Massachusetts, which is probably, in many respects, without all precedent in the annals of criminal jurisprudence. The prisoner at the bar, Dr. John W. Webster, Professor of Chemistry in the University of Cambridge (New England), and in the Medical College of Boston, was charged with the wilful murder of Dr. George Parkman, a Physician resident in Boston; the murder having been committed (as was alleged) in the Laboratory of the Medical College on the 23d of November, 1849, and the murderer having subsequently attempted to destroy the body by fire and by chemical agencies, and having so far succeeded that the proof of identity would have been impossible, if it had not been for certain distinctive peculiarities in some of the parts which remained. To the community of which both parties were members, and in which they were both of them well known and respected, we can easily understand that every part of this proceeding must have been attended with a fearful interest. We know from private sources, that for some time after the first discovery of the murder, nothing else was talked of in the whole city. The horrors of a railroad accident which occurred within a few miles, and in which some lives were lost and several persons much injured, sank into insignificance whilst the circumstances of this dreadful tragedy were gradually being unfolded to the public view. The interest was kept alive by the non-publication of the evidence taken on the coroner's inquest;

the public mind having been thus left in that state of suspense in which every article of information is eagerly discussed, and every new rumour immediately caught up, canvassed, and transmitted. A letter which we have seen from a gentleman in Boston, dated February 18th (a month before the trial), says—"It will be a trial of intense interest; the mind of the community seems to have been already excited to the highest possible point about it; but if the excitement can and should increase, some people will go out of their wits." The trial lasted no less than *twelve days*; during its continuance, as may be easily imagined, it absorbed the attention of the entire community; and the interest which it excited was far from being limited to the city or state in which it took place, but was felt throughout the entire Union, and extended even to this side of the Atlantic. The investigation appears to have been conducted with all the dignity and solemnity, which the gravity of its nature and results required; and its conclusion was one which *must* (we think) command the acquiescence of every person who may go through the case step by step, without being misguided by preconceived opinions, or by the determining bias of personal feeling. That conclusion was a verdict of *Guilty* against Professor John W. Webster.

The circumstance that both the murderer and the murdered man were members of the Medical profession, and held no inconspicuous place in its ranks,—that the murder was committed within the walls of a distinguished Medical College, probably during the actual delivery of a lecture,—and that advantage was taken of the facilities afforded by its chemical laboratory for the disposal of the remains, and for the removal of all the indicia of the crime,—can scarcely fail to invest the history with an unusual interest in the estimation of the profession at large, and more especially in the minds of all such individuals as are directly or indirectly connected with Medical Schools or Colleges. Moreover, the trial itself is one well worth study as a specimen of juridical inquiry; and more especially as illustrative of the true nature and value of what is commonly termed "circumstantial evidence." And as it happens, that the appearance of a new edition of the first work whose title we have cited, had previously suggested to us the propriety of offering to our readers a few remarks on the general subject of evidence, preliminary to the inquiry into the foundations of medical knowledge through which we propose to conduct them, we shall first consider the nature of the various kinds of *proof*—direct and inferential, demonstrative and moral,—and shall then present them with such a condensed account of the trial of Dr. Webster, as may at once convey to their minds an idea of the leading features of that remarkable proceeding, and may serve as an illustration of the doctrines we have advanced.

The great object of all intellectual research is the discovery of TRUTH: which, in its highest sense, is equivalent to the attainment of a conviction that certain *ideas* have an actual conformity with the nature and reality of the events and things to which they relate.—There is also a truth of *words*, as correctly representing ideas; and as words constitute the form in which ideas are expressed, and by which they are communicated, truth of words is absolutely essential to the attainment of truth of ideas.

The intellectual recognition of truth may be a *direct* and *immediate* act of the judgment; this is the case with all those truths which are termed

necessary, the assent of the mind being given to them as soon as they are presented to it, because it is at once felt that a belief in anything that is contrary or even different is absolutely impossible. Such, for example, are the first truths or axioms of geometry or arithmetic; every mind that is capable of receiving the idea at all, is satisfied, without any attempt at demonstration, that things which are equal to the same thing are equal to one another,—that the whole is equal to the sum of all its parts, and greater than any one of them,—or that two and two make four. For our present purpose it is not essential to consider whether even these simple axiomatic statements, which command universal assent, are themselves generalizations of experience (as argued by Mr. John Mill), or whether they are *original* intuitions, dependent upon the constitution of the human mind, and existing in it prior to all experience. It is sufficient for our present purpose if it be admitted that there *are* truths, to which our minds yield unhesitating assent as positive or necessary, and which are directly perceived as such by every one whose mind is normally constituted.

But the intellectual recognition of truth may be the result of a more complex mental operation, involving the exercise of the reasoning processes, through which the asserted fact is tried by the test of those truths which we have already consented to believe in as necessary, and is shown to be either an unavoidable consequence of them, or at least to be in harmony with them. When the former is the case, we say that the proposition is *proved*; when the latter, that it is *probable*.

Of the most complete form of this *indirect* proof, in which the reasoning faculty necessarily takes part, we have a perfect example in a mathematical demonstration. This is nothing else than a series of *inferences* drawn by logical processes from the original acknowledged truths. Each of these inferences commands the unhesitating assent of the mind, which is not capable of even *conceiving* the contrary (or anything else) to be possible; and at every step in the process, therefore, we experience the same *positive certainty* in regard to its validity, that we do in regard to the fundamental truths or axioms; so that the closing *q. e. d.* comes upon us with a conviction of infallibility from which there is no escape. Every proposition thus proved may become the foundation on which another fabric of proof may be built up: and may be used as such, with as much security as the original axioms or first truths could afford. Hence it is that the highest propositions of the mathematician are asserted by him with no more doubt or hesitation, than the untrained mind experiences about the simple and direct assertions which come to it with all the force of intuitive necessity, and which no one in his senses ever deems of questioning. By a *demonstrative proof* of any proposition, then, we mean a reasoning process of which every step is positively certain, and in which, the foundation whereon it is erected being also perfectly secure, there is no room for possible doubt as to the result. But it must be observed, that the validity of the whole involves the perfection of every part; since each part of the proof is dependent upon every other part; and its value as bringing the *factum probandum* into the category of necessary truths entirely depends upon the impossibility of error, either in the fundamental assumption, or in the succession of inferences founded upon it. We may compare such a proof to a chain cable, holding a ship to its anchor; if the anchor have taken an immovable hold in the ground, and every link



in the chain be of adequate strength and perfect soundness, the ship may ride by it in perfect security; and no violence of the winds or waves can occasion the least apprehension, if a perfect and well-grounded conviction be entertained, that it is impossible either for the anchor to drag, or for any link in the chain to give way. Supposing such a conviction to have a warrantable existence, it is obvious that the feeling of security cannot be strengthened by the multiplication of cables and anchors;—what is already *quite certain* cannot be made *more* so by any concurrence of proof.

But where the completeness of the proof depends upon a series of *connected inferences*, starting from a single assumption, it is obvious that any deficiency of certainty, either in the fundamental assumption, or in any one of the steps of the inferential process, proportionably invalidates the whole. Thus, to carry out our simile, if there should be any chance of the anchor's dragging, however strong the cable may be, the vessel cannot be considered as secure; and, in like manner, if any single link in the chain should be deficient in strength, however firm may be the hold of the anchor, and however sound may be the rest of the cable, the safety of the ship is compromised. To those who have not studied the doctrine of probabilities, it is somewhat startling to be informed of the degree in which any combination of such imperfections weakens the entire proof. Thus, if the occurrence of a particular event X, depend upon three antecedent events, A, B, and C, each of which is absolutely certain, then X is equally certain. But if *either* A, B, or C, instead of being certain, is only contingent or probable, then the occurrence of X is reduced to the same degree of probability with that of the contingency on which it is dependent; thus, if the probability of either A, B, or C be as 2 to 3, the other two events remaining certain, then the probability of X is also as 2 to 3. But if *any two* of the antecedent events be thus conditional (the third remaining certain), the probability of X will be as  $2 \times 2$  to  $3 \times 3$ , or as 4 to 9, or less than half. And if *all three* of the antecedents be contingent in the same degree, the probability of X will be reduced to the ratio of  $2 \times 2 \times 2$  to  $3 \times 3 \times 3$ , or of 8 to 27, or less than one third. Precisely the same is the case in regard to the validity of any inferential proof, of which the several steps are mutually dependent, and are all liable to a degree of uncertainty; the proof is weakened by the addition of every fresh link; and a very small amount of doubt in regard to each one most seriously impairs the validity of the conclusion. Thus, supposing that our chain of reasoning consists of six links, and that the strength of each link might be expressed by 9-10ths, which seems a near approach to absolute certainty,—the value of the conclusion will be  $\frac{9}{10} \times \frac{9}{10} \times \frac{9}{10} \times \frac{9}{10} \times \frac{9}{10} \times \frac{9}{10}$ , or  $\frac{531441}{1000000}$ , the probability being thus reduced to very little more than *one-half*.

But we have now to consider how a proof may be built up of a number of separate and independent probabilities, neither of which is sufficient in itself to establish the certainty of the thing to be proved, but which all concur to produce a belief in it. This belief has seldom the same foundation in the absolute necessity of the case, as that which rests upon demonstrative proof; and it may range from that low degree of assent which is the result of a mere balance of probabilities, and which might be upset by a very small amount of additional evidence, to that entire and unhesitating conviction which is commonly known under the conviction of *moral*

*certainty*. This designation appears to us far from appropriate; but as it is in common use, we shall take it as we find it, to imply that kind of assurance on which we act without doubt or hesitation in the ordinary affairs of life, and which can seldom be brought to rest on intuition or demonstrative proof. The sources of such assurance consist of supposed facts, every one of which may be attended with some degree of uncertainty; and of reasonings based on these facts, every step of which is in some respect open to question or to possibility of error;—the whole constituting what is commonly designated as *moral evidence*, probably because its principal application is to subjects connected with moral conduct or relations. Now we shall endeavour to show, that although *moral* certainty is attained in so different a mode, it *may* rank, when founded upon a concurrence of independent probabilities, scarcely less high than that mathematical certainty which we have instanced as an example of the conviction which rests upon demonstrative proof.

We have seen that the certainty of a demonstration depends upon the impossibility of our conceiving anything that shall be contrary to, or even different from, those propositions which constitute the foundation and the consecutive stages of the fabric of proof. Now where two or more independent probabilities concur to establish a certain *factum probandum*, although *each* of them separately may be deficient in probative force, as being consistent with some other hypothesis, yet their concurrence should give the force of absolute certainty, if no hypothesis can be conceived that shall be consistent with *both* of them, other than the one before us. Thus we will suppose two facts, A and B, tending to establish hypothesis X; but each of them having only a probative force of 1 to 2, since each is capable of receiving some other explanation,—fact A by hypothesis X, and fact B by hypothesis Z. Now although hypothesis Y may be quite as probable, *quoad* fact A alone, as hypothesis X may be,—whilst in like manner hypothesis Z may be quite as probable, *quoad* fact B alone, as hypothesis X may be,—yet, if hypotheses Y and Z are irreconcilable with each other, Y being inconsistent with B, and Z with A, so that X is the *only* hypothesis which we can conceive of as consistent with *both* facts (or sets of facts) A and B, it is entitled to our unhesitating assent, and may be as much relied on as if it had been demonstrated to follow either A or B as its necessary consequence. Indeed, the probative force of each separate fact may be reduced to a minimum, without impairing their conjoint power; for the combination of the two conditions has precisely the same force as the single condition in an ordinary demonstration, if any contrary or different supposition cannot be conceived of as compatible with both. —We shall again have recourse to a material illustration, which may help our readers to realize the abstract doctrine for which we are contending. Let us suppose that a certain strong box has two locks, each of which is capable of being opened by another key than the right one;—the chances are that a clever burglar would gain access to the treasure contained in it. But let it be imagined that the attempt to open *either* lock with a false key should bring down a catch, which (like the “detector” of a “Chubb” lock) should prevent the bolt of *the other* lock from being moved; and hence that, although either lock might be opened separately, the two could not be opened together;—it is obvious that, however easily either lock might be picked, the security of our strong box would be as great, as if it

were furnished with a single lock, so constructed as to defy all attempts to open it by any other means than the application of the right key.

Thus, then, from two *probabilities* only we may attain the *absolute certainty* of demonstration; but only under the most favorable circumstances. It is rare that, either in scientific investigations, or in juridical inquiries, or in the ordinary events of life, a case can be so stated, as to exclude altogether the feeling of the *possibility of some other inference* than that to which these probabilities point. But every addition to the number of inferential probabilities, founded upon separate and independent data, greatly strengthens the case; and their cumulative force comes at last to possess a cogency, which can scarcely be regarded as inferior to that of demonstrative evidence. These are not, as is sometimes represented, links in one chain, the perfection of which, as a whole, depends upon the perfection of every part; but they are so many distinct and separate cords, each comparatively weak in itself, yet serving, when united with others, to resist the utmost strain that can be put upon it. Reverting to our former comparison, we might say that a ship might ride at anchor just as securely when attached by four or five cables to as many separate anchors, neither one of which could separately hold it, as when moored by a single chain of adequate strength to some immovable *point d'appui*. The giving-way of any single probability, does not in the least degree weaken the probative force of the remainder, but only removes the support which it had individually given to the general conclusion; and this may still rest, with the most undoubted stability, upon the remaining supports.

It is not only, however, by the mere cumulative force of independent probabilities, that the validity of the general conclusion to which they all point, is established. For it frequently so happens, that facts of apparently the most trivial import in themselves—"trifles light as hair"—admitting of easy explanation on a variety of other hypotheses than the one in support of which they are adduced, are found to possess such a relation to others, as to acquire from them a probative force entirely new, and thus to become "confirmations strong as proofs of holy writ." And when it happens that *all* the evidentiary facts adduced in support of a particular position can be shown to have such a relation,—so that they can be arranged into a natural and self-consistent series, under the guidance of one hypothesis, but of no other,—their probative force is so greatly increased, that no room for reasonable doubt can then remain. Let us draw an example from our own art. Our diagnosis of diseases is a matter, in almost every case, of inferential evidence. In certain simple cases, we feel able to connect certain internal lesions with a certain objective manifestation (some definite sign or symptom), with such positiveness, through the combined assurance afforded by scientific principles and by universal experience, that the presence of the symptom leads us unhesitatingly to diagnose the lesion in question, and no other. Under such circumstances, the evidence approaches as nearly to demonstration, as in the nature of things it can do. But in another case, we may find an assemblage of symptoms, each having an independent value, and each separately explicable on the hypothesis of the existence of more than one disease; it is obvious that, if the symptoms are sufficiently numerous, the grouping or combination of them will point with almost positive certainty to some one out of the many possibilities, as the one which we know from experience

to be most consistent with *all* the phenomena. Let it be supposed, however, in the *last place*, that the combination of symptoms is such, that although they are in themselves so far independent as to be capable of being separately accounted for in other ways, they are found to be consistent, in their combination and sequence, with only one idea as to the nature of the internal lesion ; and that when viewed in relation to this idea, they are found to have a connexion and mutual dependence which is wholly wanting on any other hypothesis ;—it is evident that, in such a case, what would otherwise have been no more than a high probability, becomes invested with the force of moral certainty.

Just so it is in juridical inquiries. "The distinct and specific proving power of circumstantial evidence," says Mr. Wills, to whose valuable work we have been indebted for many hints in the outline view of the subject which we have here presented, "depends upon its incompatibility with, and incapability of explanation upon, any other reasonable hypothesis, consistent with the ordinary course of nature, than that of the truth of the principal fact in proof of which it is adduced ; so that, after the exhaustion of every other possible and admissible mode of solution, we must either conclude the accused has been guilty of the fact imputed, or renounce as illusory and deceptive all the results of consciousness and experience, and all the operations of the human mind. Conclusions thus formed are simple inferences of the understanding, aided and corrected by the application of those rules of evidence and those processes of reason, which sound and well-ripened experience has consecrated as the best methods of arriving at truth ; and they constitute that *moral certainty* upon which men securely act in all other great and important concerns, and upon which they may therefore safely rely for the truth and correctness of their conclusions, in regard to those events which fall within the province of criminal jurisprudence." (p. 210.)

It has been from the want of due care in this respect, that most of those wrong decisions have been given, which are occasionally cited in illustration of the fallacy of circumstantial evidence. They were cases in which the facts adduced *might* have been explained on some other hypothesis than the one in whose support they were tendered ; but in which the balance of probabilities seemed in its favour. And they cannot, therefore, be fairly urged against the propriety of our reliance on circumstantial or rather inferential proof, when there is but one possible means of accounting for a number of distinct and independent facts. In some other cases in which an erroneous decision has been given, this has been due, not to any misinterpretation of the evidentiary facts, but to a designed falsification of those facts on the part of those who deposed to them,—in fact to one of those fallacies of *testimony*, which is just as likely to give a wrong value to *direct* as to *inferential* evidence. For example, in the well-known case in which the landlord of an inn fastened upon one of his servants the guilt of a robbery committed by himself, the evidence partly consisted of "circumstances" which he had contrived for the deception of the witness who deposed to them, and partly of false assertions to which he himself deposed ; consequently, the deceptive effect was the same as if he had given direct but false testimony as to having himself been a witness to the crime. The erroneous decision in such cases, therefore, is not to be laid to the account of an undue reliance upon circumstantial or inferential evidence, but to the falsity of testimony.

The evidence of "testimony" is sometimes placed in contradistinction to the evidence of "circumstances;" and it has been argued by some that the latter is even more convincing than the former, since "circumstances cannot lie." The term "evidence of circumstances" must not be confounded with "circumstantial evidence;" the latter is really equivalent to our term inferential; whilst the former implies the production of facts which can be presented to the Court without any intermediate testimony,—as, for example, when the wadding found in a wound has been a piece of paper or calico exactly fitting to another piece found in the prisoner's possession. But, as Mr. Wills has very properly pointed out, the probative force of such "circumstances" entirely depends upon their being connected with the *corpus delicti* by adequate testimony; thus, in such a case as that now cited, it would be requisite to prove by credible witnesses that the wadding *was* found in the wound of the murdered person, and the corresponding piece in the possession of the prisoner. Upon this proof, when complete, a certain inference might be founded, which would have little probative force in itself (for the prisoner might easily give some other explanation of the possession of the article), but which might have much value in connexion with other facts.

Into the various fallacies of *testimony*, we cannot now enter at any length; but we must hint at some of those which are of the greatest practical importance. In the first place, what we are accustomed to call *the senses* of a witness may be deceived; so that he may be ready to make most positive affirmation of the occurrence of an event which never really took place. This is best illustrated by those extreme cases in which there is an actual and successful intention to deceive; as in that of a juggler who exerts his skill in presenting the semblance of actions which he does not really perform; or in that of a theatrical performer who feigns a passion which he does not feel. But in many of these cases, it is not the sensation which is false, but the interpretation which we put upon it; and the next and perhaps the most frequent source of this kind of error is the *wrong interpretation* of phenomena which were actually presented to the senses. One of the most common examples of this fallacy is an error as to personal identity;—we see an individual whose figure, countenance, gait, &c., convey to our minds the undoubting impression that a certain person is before us; we might be ready to give the most positive identification of him as having been there and then present; and yet we might afterwards discover that we had been in error in our determination, and might even, on being again confronted with the individual we had seen, and under a different aspect, feel surprised that we could have been led into a mistake. Now it very frequently happens that such wrong interpretations arise from one of two causes;—either an *insufficient opportunity* for making the observation, as when identification has taken place without adequate light;—or *the preconception of the mind* by a certain idea, which has given such a colour (so to speak) to the actual phenomena observed, as to cause them to present themselves to the mind in a light far different from their real one. There is not a more common fallacy amongst honest witnesses to any kind of truth, than this; they tell what they *think* they see or hear, instead of what they *actually* see or hear; and they often do this with such evident honesty of purpose, that it is impossible to doubt *their* complete convic-

tion of the truth of their assertions. Of this kind of fallacy, we have abundant examples among the upholders of all the quackeries of the day, almost every one of which can boast of *some* partizans who are neither fools nor knaves. The only way to get at the real truth in such cases, is carefully to analyse the assertions, to cross-examine the witnesses, and to separate the inferences which have been unconsciously engrafted upon the facts observed, from the phenomena themselves. A really honest witness, moreover, may give untrue evidence owing to the *imperfection of his memory*; which causes the events of a past period to present themselves erroneously to his mind, at the time when he is called upon to testify to them.—The existence of these, with other sources of fallacy which we could name, should be sufficient to show us how much caution is requisite in the reception of testimonial evidence, even when there is no motive nor intention to deceive. Where there is designed perversion of truth, we have no safeguard but in the improbability or inconsistency of the witness's story, or in the contradictions in which he may be sometimes involved by a dexterous cross-examination.

We now quit this part of the subject, with the intention of applying the principles here laid down to the investigation of the truths of Medical Science at the next opportunity; at present we shall employ them in our examination of the principal grounds on which Dr. Webster has been found guilty of the murder of Dr. G. Parkman.

Dr. George Parkman was "a well-known and highly respectable citizen of Boston," possessed of considerable wealth, remarkably precise and methodical in his habits, and (like most such men) somewhat exacting of a similar precision in others, though not wanting in liberality when his kindly feelings were appealed to. Of his generosity in promoting any object in which he felt interested, the fact that the Medical College was built on ground presented by him, is a sufficient indication. He possessed a good deal of house-property in Boston, and seems to have been in the habit of lending out his spare cash to such as needed accommodation. His figure was tall—5 feet 10½ inches in height,—and somewhat remarkable for the spareness of the chest and arms, and the fuller development of the lower extremities; his face, also, was characterised by a peculiarly prominent chin. Although sixty years of age, he was extremely vigorous and active in his habits, and seems to have been very fond of walking about the city, and to have been brought into contact, by the multitude of his business-transactions, with a great number of individuals; so that, from all these circumstances, there was perhaps no man in Boston who was more extensively known, or more generally recognised in his perambulations. This is a point of some importance, as will hereafter appear.

Of Dr. Webster we know less. He is spoken of as a man of sixty years of age, and is described by a number of witnesses of the highest respectability as a mild, amiable, and agreeable man, rather petulant at times, but not violently passionate, and deficient in strength of character. He seems to have had a good reputation for honesty and straight-forwardness in his general dealings; but circumstances which came out upon the trial indicate that this was not deserved. It is obvious that he had been for many years living beyond his income, and that in the endeavour to

keep up appearances he had become involved in debt.\* He was evidently one in whose estimation the opinion of "the world" was of great consequence; and he seems to have been especially desirous of maintaining his position on account of his family, which consisted of several daughters just arriving at maturity. It is worthy of note, that one of his daughters had been married not many months before the dreadful occurrence, and that the ceremony had been performed by the Rev. Dr. Francis Parkman, brother of the murdered man, of whose congregation Dr. Webster and his family were members. Although apparently much attached to his family, yet some circumstances which came out upon the trial, and some facts which we have heard through private sources, indicate that Dr. Webster manifested, subsequently to his arrest, a much greater solicitude for himself and for his "creature comforts," than for his wife and daughters; and we cannot help altogether considering him as a somewhat weak-minded, selfish man, in whom the sense of right and wrong was altogether subordinate to the gratification of his own desires.

The pecuniary relations of the two parties, previously to the alleged murder, constitute a most important feature in the case. In 1842, Dr. Parkman lent to Dr. Webster the sum of 400 dollars, and took his note for it. This debt seems to have been partly discharged; but in 1847, Dr. Webster's embarrassments had so much increased, that he applied to his friends for further assistance; and a sum of 2432 dollars was advanced to him, in which Dr. Parkman bore a share, the promissory note for the amount being in his hands. Arising out of or connected with these transactions, Dr. Parkman obtained from Dr. Webster a mortgage on all his personal property, including his household furniture and his cabinet of minerals, to secure what was then advanced, and also to secure the balance which was due from the note of 1842; that balance consisting of unliquidated principal and unpaid interest, to the amount of 342 dollars. In April, 1849, a friend of Dr. Webster's had an interview with Dr. Parkman, and the amount then settled as due from the former to the latter, on both accounts, was 456 dollars. The larger note for 2432 dollars included this sum; but all the remainder belonged to other parties, and Dr. Parkman could not, therefore, have given it up to Dr. Webster without their consent, except upon repayment of all the sums advanced. This fact will prove to be of considerable importance. Supposing Dr. Webster to have paid to Dr. Parkman the whole amount due to him, this would only have liquidated the smaller note and Dr. Parkman's own share of the larger note,—at the same time discharging the mortgage-bond, which pledged Dr. Webster's whole personal property for that sum only. In April, 1849, Dr. Webster's embarrassments having still further increased, he made an application to Mr. Robert G. Shaw for pecuniary assistance, offering to him for sale the very cabinet of minerals which was under mortgage to Dr. Parkman, and representing his necessity as so great that an officer was about to enter his house and to take his furniture for debt. Mr. Shaw commiserating his condition, and having no knowledge of the previous mortgage, agreed to advance him the sum of 1200 dollars, leaving the cabinet of minerals in Dr. Webster's posses-

\* We have learned from private sources that he had embarrassed himself by building a house far too large for either his wants or his pecuniary means.

sion, but taking a bill of sale upon them. Now this Mr. Shaw was the brother-in-law of Dr. Parkman; and, as any one but a most short-sighted man must have foreseen, this dishonest transaction soon came to Dr. Parkman's knowledge. "Subsequently," says Mr. Shaw, "I was walking with Dr. Parkman, and we met Dr. Webster passing. I asked Dr. Parkman 'what salary has Dr. Webster at Cambridge?' He answered, '1200 dollars.' I said that was not enough, for he has come to sell me his minerals. Said Dr. Parkman 'they are not his to sell;' and he took me to his house and showed me the mortgage. He then said he would see Dr. Webster and give him a piece of his mind." From that time it is evident that Dr. Parkman manifested an unfriendly feeling towards Dr. Webster, pursuing him as a creditor who felt that his confidence had been violated, and who regarded his debtor as a fraudulent and dishonest man. It appears from the evidence that he not only held these opinions, but that he had communicated them, shortly before his disappearance, in a message to Dr. Webster himself; so that the latter was in continual apprehension of an exposure of his dishonesty, which would greatly injure his position in society. It also appears that Dr. Webster had obtained further and further delay from Dr. Parkman, under a promise that he would pay him from the proceeds of his tickets for the lectures in the Medical College.

The Professors in the Medical College had made an arrangement with a Mr. Pettee, a clerk in one of the banks, to receive the fees from the students; and on November 12th, Dr. Parkman,—having called on Dr. Webster three days previously, and been informed by him that he had as yet received no money on account of his tickets,—called on Mr. Pettee to ascertain what money was in his hands on Dr. Webster's account. He was informed by Mr. Pettee that he had paid to Dr. Webster, three days previously, all the money which he had then received. On the 14th, Dr. Parkman called again, and made the same inquiry; and having received for answer that the further receipts had been just paid over to Dr. Webster, he seemed to blame Mr. Pettee for not retaining Dr. Webster's fees, and intimated that he should be obliged to distress Dr. Webster and his family. He afterwards made some remarks to the effect that Dr. Webster was not an honest or upright man, and desired Mr. Pettee to tell him so. On Monday evening, the 19th of November, Dr. Parkman called upon Dr. Webster at the Medical College, and said with anger that something must be done. Further communications took place between the parties on two subsequent days; and on the morning of Friday, November 23d, Dr. Webster called at the house of Dr. Parkman, and informed him that if he would come to the Medical College at *half-past one*, he would pay him the sum due to him. Early on the same morning, Mr. Pettee called upon Dr. Webster at the Medical College to pay him a further sum which he had received on his account; and after some conversation respecting Dr. Parkman's applications, Dr. Webster made use of this remarkable expression—"You will have no further trouble with Dr. Parkman, *for I have settled with him.*"

The sums received and paid by Mr. Pettee on Dr. Webster's account were as follows. On November 9th he received 510 dollars, out of which he paid a debt due from Dr. Webster to Dr. Bigelow, one of his colleagues, to the amount of 234 dollars, and handed the balance of 276 dollars to Dr. Webster. On the 16th he paid Dr. Webster 30 dollars more. And on



the morning of Dr. Parkman's disappearance, he brought Dr. Webster 90 dollars. It was proved in evidence by Dr. Webster's banking account, that the first of these sums was paid into his bank on the 10th of November; that a further sum of 100 dollars was paid in on the 15th; and that the sum of 90 dollars received by Dr. Webster on the morning of the 23d was paid in on the 24th. *These sums were entirely disposed of* by cheques drawn for the payment of accounts to various parties; so that it was clearly shown that Dr. Webster could not have appropriated any part of the receipts from the sale of tickets for his lectures, to the payment of his debt to Dr. Parkman. On the morning of the 23d, when he called on Dr. Parkman, his balance at the bank had been reduced to 139 dollars; and he had no knowledge that he should receive the further sum of 90 dollars that forenoon. The payment which he had to make to Dr. Parkman, according to his own showing, was 483 dollars. This is a point of fundamental importance; as indicating that the appointment to pay the debt at the Medical College was a mere pretence to get Dr. Parkman there. And we may as well say here, that no attempt was made on the side of the defence to show whence the money was obtained to make this payment. All that the counsel could urge was, that Dr. Webster had exerted himself extremely to make up the sum, and had been borrowing in small sums from any one who would lend them to him; but he did not produce a single witness, who would depose to having lent him money at this time. Dr. Webster himself asserted, that he had been accumulating money in a trunk in his own house for the payment of the debt to Dr. Parkman, and that he took out the sum on the morning of the 23d; but that he should have thus laid by such a sum apart from his banking account (which was reduced as low as four dollars at the beginning of November), and this when pressed by other claims, is altogether incredible. He had told Dr. Parkman only four days previously, that he should not be able to pay him until he had received a further sum on account of his class-fees; and of this sum, not a farthing can be shown to have been applied to the liquidation of this debt.

We have thought it better to follow this part of the investigation to its conclusion, in order that the discussion of these pecuniary matters may not be introduced into the narrative which we have now to give, of the circumstances attending the disappearance of Dr. Parkman and the subsequent discovery of his remains. Before proceeding to this, however, we must briefly describe the portion of the Medical College which was appropriated to Dr. Webster's use. This consisted, on the ground-floor, of a lecture-room, with an apartment behind it, which is spoken of as the "upper laboratory;" and from this apartment another small room opened off. From the upper laboratory a staircase descended to the "lower laboratory," a room in the basement story of the building, in which Dr. Webster's chemical operations were usually carried on, and in which were sundry sinks, furnaces, &c. This room communicated with a passage, on the other side of which was a cellar belonging to Littlefield the janitor of the building; but it was remarkably secluded from the remainder of the premises, the only direct access to it from the ground-floor being by the before-mentioned staircase. Underneath this staircase, and opening off from one corner of the lower laboratory, close to the passage, was a privy, the door of which was usually kept locked,—the key, however, generally

hanging on a nail in the neighbourhood. It was ascertained that loud noises might be made in the lower laboratory, without being heard in any of the rooms in ordinary use on the ground-floor; and it is obvious that the place altogether afforded peculiar facilities for the disposal of human remains, provided sufficient time should be allowed to get rid of them by combustion or chemical solution. It may be thought that facilities of another kind would have presented themselves in a medical school where dissections were being carried on, and that the remains might have been slipped-in piecemeal amongst the remnants of the "subjects;" but it appeared in evidence that Dr. Webster could not have gained access to the place where the latter were bestowed, without attracting observation; and further, that so accurate a record of these remnants was kept, that every part was fully accounted for, and that either surplus or deficiency would have been detected at once.

Dr. Webster's ordinary lecture-days were Tuesday, Wednesday, Thursday, and Friday; consequently from Fridays until Tuesdays there was an interval in which his presence at the Medical College was not required; and it appeared in evidence that it was very unusual for him to be there on these intervening days. Further, the last week in November was what is known as "thanksgiving week," Thursday, the 29th, being "thanksgiving day," on which the eating of turkeys (we mention the fact for a special reason) is even more universal than on Christmas day in this country. And during this week, Dr. Webster had only to lecture once, namely, on Tuesday the 27th. These circumstances also afforded unusual facilities for carrying on any private operations in his laboratory,—save that the very fact of his unaccustomed presence there was a suspicious circumstance.

On the morning of the fatal day, *Friday*, the 23d of November, Dr. George Parkman left his home at twelve o'clock, in good health and spirits, leaving word that he was going to keep an appointment, but should be at home by half-past two, his usual dinner-hour. He was seen and recognised by a number of individuals in the streets through which he passed, made some purchases in shops near the Medical College between one and two o'clock, and was seen by several persons in the neighbourhood of that building within a few minutes before two. He had an invalid daughter, to whom he showed great attachment; and one of his purchases consisted of some lettuce which he seems to have intended to take home with him to dinner, leaving it in a grocer's shop for a few minutes. His precision in regard to the hours of his meals was as great as in his pecuniary affairs; and if ever he was unavoidably absent, he was scrupulous in informing his family of his "whereabouts." These circumstances are mentioned, as showing the extreme improbability that his non-return could have been voluntary, or that he could have been wandering about the city for two or three hours after this time, as alleged on the defence.

His absence during the whole evening and night of course alarmed his family and friends; and on the following morning, *Saturday*, the services of the police were put in requisition, and a general search commenced. In the course of that day it became apparent that Dr. Parkman must either have met with foul play somewhere, or that he must have wandered away from his home in a state of mental aberration; and in the evening of *Saturday* notices were published, calling the attention of the entire

public to the fact of his disappearance, and requesting information as to the time when he was last seen. It was not then known to his family with *whom* the appointment had been; the servant who admitted Dr. Webster, when he called on the morning of the 23d, not being acquainted with his person. Rumours of Dr. Parkman's having been seen in various places on Friday, were rife the next day. These were traced in every instance, when brought to the knowledge of those who conducted the search, and were found to be fallacious. One of these rumours represented him to have been seen in Washington street as late as five o'clock on Friday afternoon; but on tracing this rumour, as all others were traced, to its source, promptly, and at the time, it was satisfactorily ascertained by those who had the greatest interest in following them up with assiduity, vigilance, and care, that the parties in every instance were either mistaken in respect to the time when Dr. Parkman was last seen, or in the identity of the person. It is important to bear this fact in mind, as being the conclusion at which these parties had arrived, before it was known that Dr. Parkman had been at the Medical College, or the least suspicion had rested on Dr. Webster. The latest definite and satisfactory information which they could then obtain, placed him in the neighbourhood of that building near the hour of two o'clock.

The search was continued during *Sunday*; and it was on the afternoon of that day that Dr. Webster first came forward, and informed Dr. Francis Parkman that his brother had been with him at the Medical College on Friday. The question naturally occurs, with respect to this communication, why he should not have made it earlier if he were innocent; or why he should have made it at all, if he were guilty. The reason assigned by Dr. Webster was, that he did not see the notice of Dr. Parkman's disappearance until Saturday evening; and that he did not communicate with Dr. Francis Parkman earlier on Sunday, because he supposed that the family would be at church. This looks rather strange, especially considering that he was a member of Dr. F. Parkman's congregation, and might naturally be supposed, therefore, to have felt an interest in anything that so nearly concerned his pastor; and the motive assigned for the delay was obviously insufficient. On the other hand, if Dr. Webster were guilty of the murder, why should he have made this communication? The answer is easy. The purpose of the crime could not be answered, unless it was made known that Dr. Webster had paid the debt and redeemed the mortgage; and owing to the public registry of mortgage-bonds existing in New England, it would be necessary to get this mortgage formally discharged at the proper office, in order that Dr. Webster might be freed from the claim which Dr. Parkman's executors would still have over his property. His great object, in fact, seemed to be to establish that he had paid Dr. George Parkman the sum of 483 dollars. The account which Dr. Webster gave of the interview was this:—that Dr. G. Parkman had dashed his pen across the note, and gone away suddenly and hastily, saying, that he would see to the cancelling of the mortgage. Other motives may not improbably have operated to lead Dr. Webster to make this communication to Dr. F. Parkman. He knew by the statement in the newspapers, that Dr. G. Parkman's family were aware of his having gone to keep an appointment with a gentleman who had called that morning to make it; he could not be sure but that Dr. Parkman's servant might hereafter recognise him as the party in question; and the fact would then tell most strongly against him, should it thus

become known. Further, Dr. Webster had very likely heard in the course of that day (having been in the city, and even visited the Medical College, as it appears, on the Sunday morning), that Dr. G. Parkman had been last seen in the neighbourhood of the Medical College; and may have thought that some further information might turn up, which might show that he had entered that building. On the other hand, his spontaneous communication as to the interview would naturally tend to prevent suspicion from resting on *him*, and would give it another direction. It obviously did have that effect in the mind of Dr. Parkman's friends; for the whole course of the search on the two succeeding days indicated their belief, that he had left the Medical College with papers and probably with bank-notes in his hand, and that he had been decoyed into some low dwellings in its neighbourhood, and there robbed and murdered.

According, however, to the statement of Littlefield, the janitor of the Medical School, it appears that *he* began to feel suspicious of Dr. Webster as early as *Sunday evening*. The statements of this witness must be received with a certain degree of hesitation, because on the hypothesis of Dr. Webster's innocence, Littlefield must have been guilty, if not of the actual murder, at any rate of aiding to conceal the remains, and of conspiring to fasten the accusation on Dr. Webster. We have most carefully analysed the evidence of this witness, however, and compared it with the other evidence adduced on both sides, without finding any serious discrepancy. He bore a most severe cross-examination extremely well; and his whole conduct in the matter appears to us not only extremely natural, but very creditable to him. We have further tried the effect of omitting his evidence altogether, as that of a witness who could not be relied on, having an interest to deceive; and we find that the main points of the case are not weakened thereby. The chief ground of the suspicion which he asserts that he thus early began to feel, was the observation that Dr. Webster had been occupied in his laboratory since his lecture on Friday in a very unusual degree, and with the doors locked. It was Dr. Webster's habit to leave the College very soon after his lecture was over, so as to be at home in time for dinner at three o'clock; but on *Friday* afternoon, when Littlefield went as usual to clear out the stove in the lower laboratory, he found every door fastened on the inside; and it was not until half-past five that Dr. Webster left the building. This last fact is confirmed by the testimony of other witnesses, and by the admission of Dr. Webster's own daughters as to the time he returned home on that evening. After Dr. Webster's departure, Littlefield again sought entrance to the laboratory through the door in the basement, and found it locked. With reference to this he states,—“I never knew Dr. Webster's door locked before, on the night of a lecture-day, since I have been in the College.”

The next morning, *Saturday*, Littlefield went through Dr. Webster's lecture-room into the upper laboratory behind it, but could not gain access to the lower laboratory that way, in consequence of the door at the top of the staircase being locked; of this lock, which had been put on to enable Dr. Webster to lock up the lower laboratory, when he went away for the summer, Littlefield had no key. Soon afterwards he saw Dr. Webster, who desired him to make a fire in the stove in the lower laboratory, which he did; he heard Dr. Webster moving about there during

part of the morning, but could not tell how long. There is no contradiction to this on the part of Dr. Webster's family, none of whom depose to having seen him before one o'clock on that day. Saturday was the usual "cleaning-day" at the College, and it was most unusual, if not unprecedented, for Dr. Webster to be there. It is somewhat remarkable that Dr. Webster's time at home is not accounted for between one o'clock on Saturday morning, when his daughters saw him on their return from a party (at which he also had been, but from which he had returned earlier), until one o'clock on Saturday afternoon, no proof having been given by either his daughters or domestics that he had even breakfasted at home on Saturday morning. A set of skeleton keys were found in Dr. Webster's laboratory, which had been filed to fit the outer door of the College, as well as other doors; and a circumstance mentioned by Littlefield excites the suspicion that Dr. Webster had been there on the Friday night. This, however, is *merely* a suspicion, and cannot be admitted as evidence; but it is important to mention that Dr. Webster could give no other account of these keys, than that he had picked them up in the street!

It was on Saturday afternoon that Littlefield first heard of Dr. Parkman's disappearance; and on Sunday afternoon he met Dr. Webster in the street, returning from his interview with Dr. F. Parkman. After finding from Littlefield that he had seen Dr. G. Parkman coming towards the College on Friday afternoon, Dr. Webster gave him the same account of the interview which he had just before communicated to Dr. Parkman's brother. From the strangeness of the whole proceeding, the unusual presence of Dr. Webster in his laboratory on Friday afternoon and Saturday morning, the unaccustomed fastening of the doors, some knowledge of Dr. Webster's relations to Dr. Parkman, and Dr. Webster's peculiar manner in the relation of his interview, the suspicion seems then to have entered his mind that Dr. Parkman had been murdered by Dr. Webster; and on returning home, he communicated this suspicion to his wife. Her reply is perfectly natural under the circumstances;—"For mercy's sake, what makes you think so? Don't you ever mention such a thing again. If the Professor should hear of it, it will make a trouble for you." It is obvious to us that Littlefield's whole subsequent proceedings, as given in evidence, not only by himself but by the other witnesses, were dictated by the fear of giving offence by suggestions which might prove altogether groundless; whilst he kept a careful watch on Dr. Webster's proceedings, in order to gather any additional evidence which they might furnish, either to corroborate or to dispel his suspicions.

Allusion is made, in the speeches of the prosecuting counsel, to a visit paid by Dr. Webster to the Medical College on Sunday; but we do not perceive that this is borne out by evidence. He was seen near the building; but he was not proved, either by Littlefield or his wife, to have entered it.

On *Monday* morning, the search was recommenced more rigorously than before, and a large reward (3000 dollars) was offered for "information which will lead to the discovery of Dr. Parkman if alive; or for the detection and conviction of the perpetrators, if any injury may have been done him. A suitable reward will be paid (it was added) for the discovery of his body." The search was begun at the Medical College; but in a mere formal manner. No suspicion on the part of the police at that time

attached either to Dr. Webster or to any one connected with the College ; and the police-officers, when they visited the building, stated their object to be merely the satisfaction of the neighbours, who would have less objection to the search in their houses when they knew that the Medical College had been first examined. On Monday they seem to have merely looked through the rooms in the most cursory manner. But it was remarked, that when they wished to enter the lower laboratory where Dr. Webster then was, they found the door fastened, and were kept waiting two minutes or more before it was opened. On that day, a carrier brought in, by direction of Dr. Webster, from his house at Cambridge, two bundles of faggots made from the cuttings of grape-vines, an empty box about a foot and a half square, and a bag of tan ; he left these articles in Littlefield's cellar, instead of bringing them into the laboratory (as he had been used to do on former occasions, whether Dr. Webster was there or not), having been expressly enjoined so to do by Dr. Webster. He tried the laboratory-door, however, and found it fastened, and observed that the keys usually hanging up in Littlefield's kitchen were not there.

The Medical College was searched by the police more particularly on *Tuesday*. On this occasion, Littlefield's apartments appear to have been carefully examined ; and the officers then went into Dr. Webster's rooms, to which they gained access more readily than on the previous day. Their search there was still of a formal kind. They told Dr. Webster that they had no suspicions—had to go where they were sent—had come to search his apartments with the rest. Dr. Webster very politely said that they could look, but expressed a wish that nothing should be turned over. They walked through the lecture-room into the back-room or upper laboratory, and one of the officers made a motion to enter the small private room behind. On this Dr. Webster said, "That is the room in which I keep my valuable and dangerous articles." The officer put his head in, and then drew it back, remarking, "I will not go in to get blown up ;" and the room was not entered by any one. The police then went down into the lower laboratory, where they noticed a bright fire in the *furnace*, also a tea-chest with tan and minerals in it, filled up full. The officers took up some of the minerals and looked at them, but made no remark. A question was asked about the door under the staircase ; Littlefield answered that it was Dr. Webster's privy, and that Dr. Webster had the key. Dr. Webster immediately diverted the attention of the police to something else, and the privy was not examined. It is not fair to charge Littlefield with this oversight. He had readily replied to the question which was put to him ; the search was not being conducted under *his* direction ; and if the police did not think it worth their while to ask Dr. Webster to open the door, it was not for him to expose himself to the maledictions of Dr. Webster if it were opened upon *his* suggestion, and nothing found therein.

The presence of a fire in the *furnace* was remarkable on two accounts ; Dr. Webster having usually required only one in the *stove* ; and Littlefield having asked him expressly on that morning whether he should light one, was ordered not to do so. In the evening of that day (on which Dr. Webster lectured to his class), Littlefield saw Dr. Webster in his room behind the lecture-room, where he was reading the paper ; and the following conversation took place, according to Littlefield's evidence. "Dr.

Webster asked me if I had bought my Thanksgiving turkey. I said no; I had thought of going out [to dine out, we presume] to Thanksgiving. He added, 'Take that order, and get you a nice turkey. I am in the habit of giving away two or three every year, and perhaps I shall want you to do some odd job for me.' I thanked him, and told him I should be most happy to do anything for him. It was the first time he ever gave me anything. The idea of his giving a cent's worth was remarkable." This statement was confirmed by the evidence of the provision-dealer to whom the order was directed. The circumstance in itself does not stand for much; but it has a marked bearing on the two hypotheses, the guilt of Prof. Webster, or that of Littlefield. If Dr. Webster were desirous of blinding Littlefield, by an act of unusual civility, to any suspicious circumstance he might chance to notice, this was a very good opportunity. If, on the other hand, he had the least suspicion that Littlefield was in any way inculpated in the disappearance of Dr. Parkman, such as he afterwards pretended to have felt, it is about the last thing he would have done. On Dr. Webster's departure that evening, Littlefield asked him whether he should want any fires in his room that week, as there would be no more lectures; to which Dr. Webster replied in the negative.

The search in the city on Monday and Tuesday was very carefully conducted. The river was dredged. The yards, the outbuildings, the dwelling-houses in the west part of the city, where Dr. Parkman was known to have a large property, were thoroughly examined. All the rumours which pointed to any later intelligence of Dr. Parkman were traced to their source, and turned out to be unfounded. By this time public attention was now universally aroused to the event; and the strong feeling on the subject is thus alluded to by the prisoner's counsel: "By no means are we to forget, gentlemen, or are we to suppose that you have forgotten, the great excitement which existed in this city, when it was first bruited abroad that George Parkman was murdered. Do we now forget that men then quitted their avocations,—that they clustered together in the corners, in the doors, in the stores, the houses, and the churches, and that their conversation was upon this one point, and upon no other?"

On *Wednesday* morning, Dr. Webster came again at the College at an early hour; Littlefield heard him moving about, and was under the impression that he was making a fire; but he was himself absent from the building from nine o'clock until one. About three o'clock, in passing up-stairs, he felt a strong heat from the flue of Dr. Webster's laboratory furnace; and his suspicion being strongly excited by this circumstance (as he had not made a fire in that furnace), he tried to get into the laboratory. The lower doors were bolted on the inside; and of the upper one Dr. Webster alone had the key. He then made an entrance by the window, and examined the interior of the laboratory. There did not seem to be then much fire in the furnace; and the whole top of the range was covered with pots of minerals, which he did not move. He noticed, however, that a large amount of water had been drawn off; and that a great quantity of pitch-pine "kindlings," which he had left there on Friday (subsequently to which he had not himself made any fire in the laboratory), had disappeared. On the stairs between the upper and lower laboratories, he saw spots of liquid such as he had not seen before; he tasted one of them and found it acid.

Littlefield's suspicions were now strongly fixed on the vault under the laboratory, to which Dr. Webster's privy afforded the only access, as the place where Dr. Parkman's remains would be found; and he now began to take means to gain access to this vault from the adjoining vault beneath his own cellar. He says, "I could not go outside of the building without everybody being at me, saying that Dr. Parkman would be found in the Medical College, if he was found anywhere." On the afternoon of *Thursday* (which was Thanksgiving-day, and on which Dr. Webster does not appear to have come to the College), Littlefield began to endeavour to pierce the partition-wall; but having only a hatchet and mortising chisel, he could not make much progress. On that night he went to a ball of the "Sons of Temperance," and did not return home until four o'clock. About nine on *Friday* morning, Dr. Webster again came to the College. Soon afterwards, Littlefield communicated his suspicions to Dr. H. J. Bigelow and Dr. J. B. S. Jackson, two of the Professors of the Medical College, who happened to be on the premises; they both directed him to make his way through the wall that afternoon, and to communicate with them immediately, if he should make any discoveries. It is obvious that, by this time, *their* suspicions also were directed against Dr. Webster, probably through the facts they had learned from Littlefield. In the afternoon, Littlefield borrowed a crowbar, and with this he made his way through the wall. As soon as he was able to put a light into the privy-vault, he saw the pelvis and two pieces of legs of a human body. This discovery, according to the testimony of his wife, most violently agitated him; but he immediately proceeded to communicate with Drs. Jackson and Bigelow, according to the directions he had received. In the meantime a police officer, who had before been at the College and had been apprized of the intended search, returned and was admitted to the spot by Mrs. Littlefield, who had been set to watch in case Dr. Webster, who had left the College, should return.—Now the whole of this conduct of Littlefield's appears perfectly natural and consistent under the circumstances; it being especially to be borne in mind that he was the dependant of Dr. Webster as well as of the other Professors, and that it was obviously for the interest of everybody connected with the College, that not even a shadow of groundless suspicion should rest on Dr. Webster, but that the investigation should be conducted secretly, so as not needlessly to implicate him in this dreadful transaction.

A party of police immediately proceeded to Dr. Webster's house, for the purpose of arresting him. When he was informed that they had now given over searching for Dr. Parkman, since they had found his body, he inquired, "Did they find the whole of it?" a question of remarkable significance. He subsequently averred that the remains were not those of Dr. Parkman any more than they were his own; and yet almost in the same breath he charged Littlefield with the murder of Dr. Parkman, or with having been accessory in the crime by aiding in the concealment of the remains. On the evening of his arrest, Dr. Webster was conducted to the Medical College, where it was intended to make a more rigorous search through his apartments. The evidence of the police and others, as to his violent agitation, while this was being carried on, is very striking; but we do not think that much weight should be attached to it. A man of not very vigorous mind, already depressed by pecuniary difficulty, and



conscious that there were many circumstances that tended to inculpate him, might naturally feel violently agitated at being dragged from his home at night, leaving his wife and daughters in a state of the greatest alarm, to answer a charge of murder of which he might be entirely innocent. Still the extreme prostration which he manifested is quite consistent with the idea of his guilt, and particularly tallies with the supposition that, being what is commonly considered a very 'nervous' man, he had been making an immense effort during the preceding week to keep up an appearance of calmness.

The search which was made on that evening, and on the following days, was attended with the following results:—In the privy vault were found a pelvis, a right thigh (from the hip to the knee) a left leg (from the knee to the ankle), and certain towels with Dr. Webster's initial, similar to those used in his laboratory. In the tea-chest, before mentioned, were found, imbedded in tan and covered with minerals, the thorax, the left thigh, a hunting-knife, which Dr. Webster had been seen to use, and a piece of twine of a peculiar kind, similar to that which Dr. Webster had been accustomed to employ in his laboratory. In the furnace were found fragments of various bones in a state of calcination, with certain blocks of mineral teeth, and several portions of gold, which had been melted, also some pieces of melted pewter; all these substances being imbedded in the slag and cinders of the furnace. The fragments of bones were most carefully examined by Dr. Wyman, the accomplished Professor of Anatomy in Harvard University; and the greater part were sufficiently preserved to be determined by him as parts of the cranium, and of the bones of the face and neck, of those of the fore-arms, hands, right-leg below the knee, and feet. There were also some fragments supposed to belong to a humerus, but not of sufficient size or character to enable them to be certainly identified. On putting together the well-preserved parts found in the privy-vault and tea-chest, it was obvious that they belonged to the same individual; and the parts deficient were the head and neck, the arms and hands, the right leg from the knee to the ankle, and both feet. These, as we have just seen, were the very parts to whose bones the fragments found in the furnace were referable; there was not a single part in the one set of remains, which duplicated any part found in the other; and the correspondence in form and proportions was such, that there could be no reasonable doubt that they belonged to the same body. Of this it was very important that clear proof should be given, since it might have been otherwise alleged that these were remains of different 'subjects' brought into Dr. Webster's premises for the purpose of inculpating him.

The next point was the identification of these remains; and this appears to have been most satisfactorily accomplished. Considering that these remains were found in the place where Dr. Parkman was known to have last been, the presumption was extremely strong that they were his. It is obvious that, by whomsoever the body was disarticulated, the attempt was made in the first instance to get rid of the head, hands, and feet, as the parts by which the remains would be most easily identified; and if nothing in the rest of the person was disaccordant with Dr. Parkman's known height and figure, the presumption would receive strong negative confirmation. But the evidence of identity by no means rests here. Dr. Wyman made a calculation of the total height, from the length of the trunk and

limbs; and found that it would reach 5 ft. 10½ inches, which was proved to have been exactly Dr. Parkman's height. Mr. Shaw, Dr. Parkman's brother-in-law, spoke to the remarkable hairiness of the chest and leg, as having been very characteristic. The most important evidence on this point, however, was furnished by the conformation of the lower jaw, and by that of the mineral teeth. On the former point Dr. Wyman spoke positively, being able to make out the peculiarities of this bone by the apposition of its fragments. There was an absence of teeth on the right side, from the coronoid process to the posterior bicuspid; and on the left side was a peculiar depression, such as Dr. Wyman had seen in no other jaw: he concluded also, from the curves of the fragments, that the chin would be unusually prominent. At these conclusions he had arrived, so as even to furnish a sketch of the complete jaw, before he knew that these peculiarities in the conformation of Dr. Parkman's jaw could be positively sworn to by Dr. Keep, whose moulds gave unequivocal evidence of them. The evidence of that gentleman is extremely remarkable. He states that he had been Dr. Parkman's dentist from the year 1825; and that all the peculiarities of his dental conformation were strongly impressed on his mind. He had made Dr. Parkman a set of artificial teeth in 1846, under circumstances which caused him to take particular note of the workmanship. Dr. Parkman had required to have them completed by a fixed day, —the opening of the Medical College; there was barely time to do this, more especially as the peculiarities of Dr. Parkman's mouth rendered it more difficult than usual to make a good fit. One of the teeth in the front block was injured by an accident, shortly before its completion; and the loss of time was only made up by both Dr. Keep and his assistant working at the set during nearly the whole of the night preceding the day on which Dr. Parkman wanted them. They were finished just thirty minutes before the opening of the Medical College; and afterwards, Dr. Parkman having complained that he had not room for his tongue, a portion of the block of the lower jaw was ground away with a very small wheel. Dr. Parkman had called on Dr. Keep not above a fortnight previously to his disappearance, in order to have a slight repair done to his teeth; and Dr. Keep's memory was refreshed as to the peculiarities of the set, whilst he was thus enabled to testify that it was in use by Dr. Parkman within this short period. When the pieces found in the furnace were shown to him, he at once recognised them as Dr. Parkman's; and upon comparing them with the moulds, the correspondence was found to be exact. It is stated in the report of the trial, that during the progress of Dr. Keep's testimony, the Court and witnesses were affected even to tears, Dr. Keep especially being overcome with emotion. It was obviously felt that this positive identification was the most important point in the whole case. Dr. Noble, who had been Dr. Keep's assistant at the time the set of teeth was made for Dr. Parkman, also spoke with great confidence as to the pieces found in the furnace being part of this set; his impression being founded upon what he calls the general *shade* of the block, and upon the removal of part of its inner surface by grinding. An attempt was made, on the defence, to invalidate the testimony of these gentlemen; Dr. Morton,\* also a dentist,

\* It is rather a singular coincidence, that Drs. Jackson and Morton, the rival claimants for the discovery of the anæsthetic properties of ether, should have been brought forward as witnesses on opposite sides in this case: Dr. Jackson having been called to give evidence for the prosecution on certain chemical points.

being called to testify that the marks sworn to by Dr. Keep were insufficient. But even he was obliged to admit, on his cross-examination, that if he had a jaw to operate on, and had taken particular notice of it from any peculiarity of conformation, he should probably be able to identify it, if too long a period had not elapsed. And several other dentists were afterwards called on the side of the prosecution, who stated their conviction that Dr. Keep was fully justified, by the peculiarities he had noted, and by the adaptation of the blocks to his moulds, in the confidence of his identification. We have made some inquiry amongst eminent dentists in this country, and meet with no hesitation amongst them as to this point; all concurring in the assertion that a positive recognition was quite justifiable under the circumstances stated. One further point in Dr. Keep's evidence is worthy of note, namely, his conviction that the teeth must have been put into the furnace whilst yet in the head; if they had been suddenly exposed to a strong heat, he states that they would have flown in pieces.

No positive indication could be obtained, from the examination of the remains, as to the mode in which Dr. Parkman came by his death. The thorax exhibited a perforation in the region of the heart, such as might have been made by the knife found with it in the tea-chest; but it was obvious that the edges of the wound had been affected by alkaline applications, so as to obliterate any characters by which it might have been affirmed that the stab was made during life. An attempt was made to show that some of the fragments of bones found in the furnace had been broken before their calcination; but this allegation could not be sustained. The indictment was laid in four counts; the first specifying a stab with a knife as the cause of death; the second, a blow with a sledge-hammer which was known to be in the laboratory for the purpose of breaking up coal; the third, an assault with the prisoner's hands and feet; whilst the fourth left the means and instruments of death altogether unspecified. Objection was taken on the part of the defence to the last count; but the Court held that it was legally valid; and the counsel for the prosecution wisely fixed the attention of the jury upon it, leaving them free, however, to find a verdict upon either of the other counts, if they should think such verdict borne out by the evidence.

The medical witnesses called on the part of the prosecution all agreed that the dismemberment had been effected with a certain amount of skill, and that it displayed some anatomical knowledge; in particular, the separation of the sterno-clavicular articulation was spoken of as not done by a mere bungler. This point of evidence, however, does not implicate Dr. Webster to the exclusion of Littlefield.—Proof was further given that the remains were not those of any "subject" brought to the College for dissection, not only by the record which was kept of the disposal of such remains, but also by the circumstance that, as was deposed to by chemical witnesses, they had not been injected with the solution which was employed in every case for the preservation of the bodies.

The careful search which was at last made of Dr. Webster's apartments in the Medical College, brought to light other objects than the remains of the body supposed to be Dr. Parkman's. In the small room opening off the upper laboratory, were found a pair of trousers and a pair of slippers; these had red spots upon them, some of which, when examined chemically

and microscopically, proved to be blood. In the lower laboratory was found a sort of grapple made of fish-hooks, attached to a stick by twine; these fish-hooks were proved to have been purchased by Dr. Webster on Tuesday; and the twine with which they were fastened was of the same peculiar kind as that which was tied around the thigh in the privy-vault, and as the ball in use in the laboratory. The purpose of this grapple would seem to have been, to fish up the remains from the vault, piece by piece, as they could be disposed of in the furnace. Further, on the Friday on which he was arrested, Dr. Webster went to a tinman, and ordered a large and strong tin box, which he directed to have so made that he could solder it up himself; he stated that this box was to hold books; but he did not explain why he wished to adopt such an unusual proceeding as enclosing books in an air-tight case. When he ordered this box, as if with the view of completely putting the tinman off the scent, he made statements as to Dr. Parkman's having been seen at Cambridge after leaving the Medical College; and also told a story, that through certain mesmeric operations it had been discovered that Dr. Parkman's body was found, and that it had been carried off in a cab upon which there was some blood.

Some other points in Dr. Webster's conduct may be noticed, as having a certain significance. When he was brought to the Medical College after his arrest, it was proposed to enter the privy from the lower laboratory. Littlefield said that Dr. Webster kept the key; and on Dr. Webster being asked where the key was, he pointed to a key hanging up, and said "there it is." This key was tried and found not to fit the lock; the door was broken open, and the real key was afterwards found among those taken from Dr. Webster's pocket on his arrest. After his commitment, he wrote a note to a member of his family, desiring the concealment of a certain packet. This note was opened, according to the usages of the jail; and a suspicion being naturally excited that this packet contained something of consequence, a police-officer was despatched to Dr. Webster's house to obtain it. When it was opened, it was found to contain *both* the promissory notes already mentioned, with a memorandum in Dr. Webster's hand-writing of his transactions with Dr. Parkman, and of the amount of his debt to him. It appeared that this amount was wrongly calculated; and that the sum of 483 dollars, which he represented himself as having paid to Dr. Parkman, was not the precise amount of his debt. Neither of these notes was receipted; but the signatures of *both* of them were erased, and the face of the notes was crossed. Now supposing that Dr. Webster had really paid Dr. Parkman the sum due to him individually, it is in the highest degree improbable that a man of Dr. Parkman's methodical habits of business should have given up even the smaller note without properly receipting it; and it is quite impossible to suppose that he should have returned the larger note into Dr. Webster's hands, unless the sums due on it to other parties had been discharged, as well as the amount of the debt to himself. Of such a discharge, not even a pretence was made on Dr. Webster's side; and the possession of this note must be regarded as one of the most damning proofs of his guilt. On the same day that Dr. Webster ordered the tin case, and told the story of the mesmeric discovery, he called upon a lady with whom he was acquainted, and asked her (probably in consequence of something he had heard) at what time she

had last seen Dr. Parkman. She replied, on the afternoon of *Thursday*,—the day before his disappearance. He seemed anxious to make her think that it was on *Friday* that she had seen him, asking her if she was quite sure that it was not Friday; and he told her that a cloak and hat had been fished up from the river. A good deal was also said by him at different times, about certain bank notes which he averred that he had paid to Dr. Parkman, one of which he affirmed to have been passed by an Irishman who had offered it at the toll-gate.

The only evidence offered on the defence that threw any real doubt on the validity of the facts adduced on the side of the prosecution, or which in any way ran counter to the inference to which those facts seem necessarily to lead, was to the effect that Dr. Parkman was seen walking about Boston subsequently to the time at which he was supposed to have been murdered in Dr. Webster's laboratory. Five witnesses were produced, who deposed to having *seen* him,—one at twenty minutes after two,—another, between half-past two and half-past three,—another about twenty minutes after three,—and two others (mother and daughter walking together) at a little before five. The respectability of these witnesses is unimpeached, and they all affirmed that they were well acquainted with Dr. Parkman's person, having known him by sight for several years. Further, each of them had some special reason for remembering both the day and the hour. There is something about this testimony, on the face of it, which strongly leads to the suspicion that these witnesses were wrong in the identity, and that they saw some one whom they mistook for Dr. Parkman. Not one of them deposes to having *spoken* to him; and although the two ladies passed close to him and bowed, they did not see any returning salutation on his part. Now, according to this evidence, Dr. Parkman must have been wandering about the city for *nearly three hours*; the places in which he was represented to have been seen were remote from each other; there was probably not a man in the community whose person was more generally known; there had been no difficulty in tracing him, by the testimony of numerous persons who saw and recognised him, in his walk from his house to the Medical College in the morning; and yet he was only recognised on these four occasions, during the whole of his three hours' roaming about the city in the afternoon. Where could he have been, or what could he have been doing, during that time, so completely to seclude himself from observation? Moreover, it is to be remembered with respect to the testimony of the two ladies, that the sun set on the 23d of November at half-past four, and it was proved to have been a cloudy day; is their recognition of Dr. Parkman's person from a quarter to half an hour after sunset, on a dull November afternoon, much to be relied on? We certainly think not. Moreover, both mother and daughter deposed that Dr. Parkman was walking with another gentleman at the time they passed him; yet no such person came forward to prove that he had been in company with Dr. Parkman, notwithstanding that every inducement was offered to bring out any such information. Putting aside their statement, we find that the latest time deposed to by the other witnesses was half-past three: and there is nothing really inconsistent in their statements, even supposing them to be correct, with the hypothesis that Dr. Parkman was murdered by Dr. Webster on that afternoon. For it can easily be imagined that Dr. Webster, not having his preparations

complete, or feeling his determination not sufficiently strong, dismissed Dr. Parkman on his first visit with the plea that he was not yet quite ready with his payment, and requested him to call again at half-past three ; and as Dr. Parkman entered the building the first time without being met by any one on the premises, so might he have done on the second occasion.

This explanation may serve for those who think it improbable that the first three witnesses should have been mistaken in their identification, it being broad daylight when they believed that they saw Dr. Parkman. For ourselves, however, we have no doubt that they *were* so mistaken, and that Dr. Parkman never left the Medical College after he first entered it on his fatal errand. For it must be within the experience of every one of our readers, that he has occasionally recognised (as he believed) a friend or acquaintance in the street, and has afterwards found that he was wrong. It was proposed, on the side of the prosecution, to prove by the testimony of other witnesses, that there was in Boston, at the time of Dr. Parkman's disappearance, a person who strongly resembled him in person and gait, and who had been previously mistaken for him ; but this evidence was not received by the Court. Under the peculiar circumstances of the case, however, it appears to us that—leaving this proposed proof altogether out of view—the *negative* value of the fact that no one could be brought forward to depose to having *spoken with* Dr. Parkman subsequently to his first visit to the Medical College, and that there were so few to testify to his even having been seen, whilst he is represented as having been walking for at least an hour and a half through streets where he would be likely to meet numbers of friends and acquaintances, is sufficient greatly to weaken, if it does not altogether destroy, the value of the evidence on the other side. This was evidently the conviction of the presiding judge, who thus alludes to the point in his charge to the jury :

“ On Monday morning Dr. Parkman's disappearance was universally known in the city. Then thousands were put upon their recollection to say whether they had seen Dr. Parkman, where, when, and under what circumstances. Now, he was a person very well known ;—perhaps no man of his age and situation was better known here, in person, than himself. Now, notwithstanding this proof, the question is, whether he would have been likely to have been seen by many persons if he had been moving through the streets in the manner indicated by this testimony. Judge for yourselves. Would there not have been hundreds or thousands of persons who would have seen him, and have testified to it ? This, however, is negative evidence. But if anything happens, and persons do not see it, if they were placed where they might have seen it, this, though negative, leads to an affirmative result. That is one of the modes to lead you to a view of the truth. If you are satisfied that there were a great number of persons along the streets where he was said to have been seen, would there, or would there not, have been a great variety of persons who would have confirmed that statement ?” (p. 291.)

In considering the charges against Dr. Webster, and the evidence brought on each side in relation to them, we shall follow the course adopted by the jury, who took into consideration the following issues :

*First.* Are the remains of a human body, found in the Medical College on the 30th November, 1849, those of the late Dr. George Parkman ?

*Second.* Did Dr. George Parkman come to his death by the hands of Dr. John W. Webster, in the Medical College, on the 23d of November, 1849 ?

*Third.* Is Dr. John W. Webster guilty, as set forth in the indictment, of the Wilful Murder of Dr. George Parkman?

On the first of these points, we cannot suppose that our readers can have any doubt. Dr. George Parkman disappears, without any assignable motive. A careful and rigorous search is made. Remains of a body are found in the very place in which he was last certainly known to have been. Of these remains no other account can be given. They correspond in dimensions and configuration with the known characters of Dr. Parkman's person. The peculiar hairiness of the chest and legs; the conformation of the lower jaw; and the special characters of the mineral teeth, so distinctly recognised by the dentist and his assistant, who had made them under such peculiar circumstances, must leave, we should think, no room for doubt in the mind of any unprejudiced judge. Every circumstance here points in one direction, and not a single adverse proof is given on the other side. Not even Dr. Morton would affirm that these teeth were more likely to have belonged to any one else; he merely attempted to throw doubt on Dr. Keep's positive recognition of them as Dr. Parkman's. The jury do not appear to have entertained the slightest hesitation on the point; and it is worthy of note, although it could not be stated in evidence, that the proof of Dr. Parkman's death had been fully admitted elsewhere, as authorizing the administration of his large property in the regular mode. The fact that property of Dr. Parkman's was found in Dr. Webster's possession, and that the latter could give no sufficient account as to how it came into his hands (we refer especially to the *larger* note, which there was no pretence of Dr. Webster's having paid), was a strong confirmatory indication, if any were needed, that the body found on Dr. Webster's premises was really that of Dr. Parkman.

Let us now examine, in the *second* place, the circumstances which indicate that Dr. Parkman came by his death by the hands of Dr. Webster. In the first place, we have a strong *motive* alleged, and not disproved, arising out of the prisoner's relations with Dr. Parkman. This has been very strongly, but not, we think, unfairly, stated by the counsel for the prosecution:

"Dr. Parkman is following up Dr. Webster continually.—For what? Dr. Webster has no money to meet him! What is his condition? Here is this creditor, inexorable, as he calls him, and as his counsel echoes; *inflexible*, I think, would have been more just. The cloud over him is broadening and blackening, day by day. What can he do?—What is he exposed to? The disclosure to the world of his false but fair character!—The exposure of his fraud! But more, and that which comes nearest home to the bosom of such a man—for, I grant you, he has strong domestic affections, and warm attachments—that which comes nearest home to the bosom of such a man is, that all his effects were liable to be seized at any moment, and his home stripped of that which stood security for his debts. His household furniture was all that was left: the minerals, as you will see when you examine the mortgage, were already disposed of, and money raised on them to pay his debts. That had all been exhausted. You will see by the papers we put in, that his friends' benevolence and beneficence had been exhausted, and that he had no resources left. He was left stripped and bare to receive the shock coming upon him from this creditor, whose just indignation he had reason to dread. What was involved in this impending blow, which he thus feared was about to be struck *home* upon him? The loss of caste!—the loss of reputation! For he could not stand an hour with that reputation assailed and exposed.

"Now, gentlemen, when you come to motive, I undertake to say, that no poor

illiterate outcast from the dregs of social life, who prowls out from his hiding-place to steal the bread for himself, starving, or for his starving wife and children, ever had a motive which addressed itself with more force to him, than was thus addressed to the prisoner by circumstances like these, to get rid, in some way—in any way—of this tremendous cloud that was darkening all around him, and deepening every hour." (p. 257.)

In this point of view, the case is very similar to that of Tawell, who, as our readers will doubtless remember, murdered the woman with whom he had cohabited, merely to prevent the exposure of his transgression, and his consequent loss of caste with the Quaker community, in which he was desirous of regaining his place. And it will also be in the recollection of some of our readers, that a murder was committed some years ago at Leicester, under circumstances very similar to those of the Boston tragedy, except that both the parties were in a lower rank of life. A bookbinder owed money to a man who had supplied him with goods, and, after repeated applications for payment, made an appointment with his creditor for the latter to come to his workshop on a particular day, in order to receive his money. Instead of paying him, he murdered him, cut up his body, and attempted to burn the members in his stove. The smell, however, soon attracted attention, and the crime was thus discovered early enough to prevent its *indicia* from being obliterated. Such facts have their value, as showing that the crime charged against Dr. Webster is by no means (as was urged by his counsel) beyond the pale of human experience; but they have, of course, no probative force of their own. Dr. Webster called evidence to character, but this could be of but little avail in respect to such a charge. And it is to be recollected that all the facts which came out upon the trial with regard to his pecuniary affairs for some years past, indicated that, in order to keep up appearances and indulge expensive tastes, he had been living beyond an income which was sufficient for the reputable maintenance of his family; and that he had not only heavily taxed the generosity of his friends for assistance, but had been guilty of an actual fraud, of the exposure of which, as well as of his whole involved position, he was in daily dread.

The next important point is, that Dr. Parkman came to the Medical College by Dr. Webster's own appointment, and this at a time when Dr. Webster would have peculiarly uninterrupted possession of the laboratory for several days. Dr. Webster was the last man with whom Dr. Parkman was proved to have had any communication. The body of Dr. Webster was found on his own premises, under his own lock and key, and he could give no explanation of its presence there. Dr. Parkman's property was found in his possession, after an attempt on his part to conceal it, and he can give no sufficient account of the mode in which it came into his hands. With the body are found towels marked with his name, a knife which he had been seen to use, and tan which he had sent for subsequently to the murder. On the thigh found in the privy-vault, was twine taken from his laboratory-ball. On his trousers and slippers, found in his room, were spots of blood. In his laboratory was found a grappling composed of fish-hooks, which he had himself ordered, secured with his own twine. Of the keys found in his rooms, which had evidently been adapted to open the college doors, he admitted knowledge, by saying that he had picked them up in the street. The ordering of the tin case, at the junc-



ture when it was found that it would take some time longer to dispose of the body, and when, from the still-increasing excitement about the Medical College, it was likely that a more rigorous search would be made there, is another inculpatory circumstance; the purpose alleged being altogether inconsistent with the mode in which the cover was directed to be constructed. All these facts are quite independent of the testimony of Littlefield; and they stand as facts to be accounted for on any hypothesis of *his* guilt in the matter.

Then we have Dr. Webster's unusual continuance in the laboratory, to nearly six o'clock on Friday evening, as proved independently of Littlefield; his visit to the laboratory on Saturday morning, Littlefield's statement of which is not contradicted by any proof that he was elsewhere; his presence there on Monday, where he was seen by other witnesses; his presence there on Tuesday, where he was seen by the police; his presence there on Wednesday, as deposed to by Littlefield and not contradicted; and his presence there on Friday. That he was locked in on various occasions, and that his laboratory was not as accessible as usual, was proved not merely by Littlefield, but also by various persons who had sought admission; and that he had himself carried away the keys of the privy and laboratory, which usually hung up on the premises, was indicated by their being found at his house. That the fire in the furnace, by which the head and hands, and probably the clothes, had been consumed, was burning there on Tuesday, was proved by the police; and it is obviously impossible to suppose that its existence was without his knowledge. Besides, if Littlefield had lighted it for such a purpose, would he have done so on the morning of the day on which Dr. Webster lectured?

Dr. Webster's unusual presence at the laboratory, whilst it is a strong *inculpatory* circumstance as regards *him*, is an equally *exculpatory* circumstance as regards Littlefield; who could scarcely have effected the partial destruction of the body by the agencies furnished by Dr. Webster's laboratory, without some knowledge or suspicion on Dr. Webster's part. No proof of any such suspicious circumstances was elicited either by the cross-examination of Littlefield, or by direct evidence produced in Dr. Webster's defence.

All the evidence given of Dr. Webster's behaviour and expressions with regard to this transaction, is perfectly compatible with the idea of his guilt, and some of it points strongly towards this. We have already seen how easily his spontaneous mention of his interview with Dr. Parkman is accounted for; and another circumstance urged in his defence, namely, that he himself went to the Register Office to ascertain whether Dr. Parkman had discharged the mortgage,—is at once explained, when it is considered as a "blind" to divert suspicion from him. His unfortunate family, and some of his intimate friends, we understand, rely much on his cool and easy behaviour on the evening of the murder, on which, after remaining some little time at home, he accompanied his wife and daughters to an evening party. But the annals of crime are filled with such examples. The latest that occurs to us is that of the chicken-hearted Mr. Manning, who went out into the garden because he could not bear to remain in the house with the dead body of his victim; and whilst there, coolly smoked his pipe, and conversed pleasantly with his neighbour over the wall. The remark to Mr. Pettee on the morning of the 23d, that he

would have no more trouble with Dr. Parkman, is just such a one as has been made in hundreds of similar cases of intended murder; as if the criminal was so full of his design, that he could not altogether keep it to himself. His charge against Littlefield, as a conspirator who had fixed on him the guilt of Dr. Parkman's disappearance, was quite inconsistent with the unlooked-for mark of confidence and favour which he had shown him two days before. The question, whether the whole body of Dr. Parkman had been found, would be most strange and unnatural, supposing him to be ignorant of its dismemberment. And his extreme agitation, when conveyed by the police to the Medical College, and afterwards committed to jail, is just what might be expected under such circumstances, when he found all his plans and hopes of escape to have been shattered to fragments.

After a careful examination of the whole case, we do not find any single circumstance incompatible with the idea of Dr. Webster's guilt; and of those which tend to fix the crime on him, there are some which no other hypothesis will explain. We have carefully inquired into the value of every one of the hypotheses suggested by his defenders, each of which involves the guilt of Littlefield, either as a principal or an accomplice; and we find ourselves at once met by an array of incompatibilities and absurdities, which would be sufficient to negative inculpatory facts of a much stronger kind. But neither adequate motive, nor any other direct inculpatory circumstance whatever, can be assigned as indicating that the murder was more probably committed by Littlefield than by Dr. Webster; so that there is not really one single evidence of the guilt of the former and the innocence of the latter, which is not at once borne down by the overwhelming weight of facts adduced in proof of Dr. Webster's guilt. Dr. Webster's defenders seem to have laid their account with suggesting a possible explanation of each of the principal facts which inculpated him, and to have wished the jury to believe, that if they could show that not one of them necessarily involved the presumption of his guilt, no such necessary presumption arose out of their concurrence. They talk about the weakening of the *chain* of evidence, as if the impairment of the value of any one fact, either by counter-testimony, or by an explanation of it consistently with Dr. Webster's innocence, rendered all the rest less cogent. We have already pointed out to our readers the fallacy of this line of argument, and have shown the difference in the bearing of *dependent* and *independent* facts and reasonings on the *factum probandum*. It is the perfect independence of the facts of this case, considered in themselves,—the remarkable way in which they are linked together and shown to be in natural succession as one series of occurrences, when considered on the hypothesis of Dr. Webster's guilt,—and their inconsistency and unmeaningness, when viewed in any other light,—which impress us with the fullest conviction that but one solution is possible.

The only apparent inconsistency, is that which it was attempted to establish by means of the "Parkman *alibi*," as it was called in the course of the proceedings. Into the validity of this objection we have already inquired. The jury appear to have felt no doubt upon the matter, but to have completely made up their minds before leaving the box, that Dr. Parkman had come by his death by the hands of Dr. Webster.

The *third* and last question for consideration was, whether the charge against Dr. Webster amounted to *wilful murder*; or whether the fatal result might be accounted for on the supposition, that there was no pre-determined design on the part of Dr. Webster, but that in the meeting between the creditor and debtor a dispute arose, blows were struck, and Dr. Parkman killed in the heat of passion. Now, the law upon this point stands thus:—If *homicide* be proved, the law presumes it to have been done with *malice prepense*, so as to constitute it wilful murder. The proof of provocation, whereby the charge may be mitigated to manslaughter, must be adduced by the prisoner. No such proof was offered by him, for the simple reason, that his line of defence consisted in denying the act altogether. His counsel, however, advanced the hypothesis in his behalf; and there would have been, we doubt not, a strong desire, on the part of all concerned in the trial, to take advantage of it, if the least colour of plausibility could have been given to it. But not only was there an utter absence of any such evidence, but a strong counter-proof that the murder had been deliberately planned. For why else should Dr. Webster have enticed Dr. Parkman to the Medical College, under pretence of paying him money which he had it not in his power to hand over, and of settling a business transaction which might have been just as well accomplished (if Dr. Webster had it in his power to do so) at Dr. Parkman's own house?—the additional fees received by Dr. Webster that morning not having been employed for that purpose. So, again, if Dr. Webster had killed Dr. Parkman in the heat of passion, why should he have taken into his possession, and done his best to cancel, not merely the note which he said that he had paid, but also the other for a much larger sum, which there was no pretence of his having liquidated? Both the previous and all the subsequent transactions appear to us not merely to bear out, but even to require, the idea of predetermination on Dr. Webster's part; and of this, too, the jury appears to have been completely satisfied. The jury was absent between two and three hours, but this was not from any hesitation as to their verdict. The court had been adjourned for that interval, in order (we presume) to give them time for the full consideration of their verdict, and to avoid even the appearance of unseemly haste.

Of what took place in the jury-room, a most interesting account has been given by one of that body, in a letter to a Boston newspaper, which we shall quote in full. There never was a case, perhaps, in which the sense of responsibility was more strongly felt, or in which more earnest efforts were made to arrive at a just decision :

"The jury were composed of twelve men, from as many different branches of the mechanical and mercantile 'professions;' they were from four different religious denominations, and their ages varied from 28 to 66 years. They were men whom I should designate as possessing good sound common sense,—men capable of judging, of discerning, of appreciating evidence, and estimating its importance. The jurors after they had become better acquainted with each other, and as the evidence began to bear with crushing weight upon the prisoner, and the 'network of complicated circumstances' seemed to encircle him, felt strongly the need of 'that wisdom which cometh from above,' to guide and direct their minds aright, in their most momentous and responsible situation.

"It was then that our worthy foreman (whom we all must highly respect, and whom we shall ever remember with pleasure) proposed to the jury that they should

have religious services every evening. The proposition was most cheerfully responded to, and ever after that time, the voice of praise and prayer ascended, as we trust, from sincere hearts, to the throne of infinite Wisdom and Mercy. I need not say that the burden of every prayer was for wisdom to guide and direct unto a right decision, and for blessings most rich and precious to descend upon the prisoner and his afflicted family.

"I now come to the closing part of this momentous trial. When the witnesses for the defence had given in their testimony, and the counsel for the prisoner announced the evidence on their part closed, a feeling of pain and anguish must have come over the mind of every juror. 'What! can no more be said,—no more be done in behalf of the unhappy prisoner? Is that the evidence—the *only evidence* on which we are to place our verdict of *Not Guilty*?'"

"At that very time, with the light which the able charge of the Chief Justice afterwards gave us on several points of the 'law and the evidence,' I think I speak the sentiments of nearly if not quite all the jury, when I say, that they were as fully prepared for their verdict as they were when they retired to the jury-room, after listening to the most able and eloquent pleas of the prisoner's senior counsel and the Attorney-General, so strongly, so fully, had the evidence pointed to the prisoner as the guilty man, AND TO NO ONE ELSE. After the jury had gone to their room,—with the various evidences of guilt spread out on the table before them, and the door locked upon them, shut out, as it were, entirely from the world, with nothing but the eye of the omniscient God upon them,—so painful was the sense of responsibility, so unwilling were they to come to the result which *all* felt they must come to, that thirty or forty minutes were spent ere anything was done; when at last the voice of the Foreman was heard calling them to order, and reminding them of their duty, however painful. And, when they had all taken their seats around the table, then it was that one of the jurors arose and said, 'Mr. Foreman, before entering upon the further consideration and decision of this most important matter, I would propose that we seek for Divine wisdom and guidance.' The proposition met with a cordial response, and the Foreman called upon a juror to offer prayer. This was done, most feelingly and sincerely. We then proceeded to the most trying and painful part of our arduous duty. The various articles which were put into the case were examined by the jury, and particularly those things which seemed to bear most strongly against the prisoner."

The three questions into which the final decision was resolved, have been already quoted.

"When the vote on the first question was put, twelve hands arose immediately. Some little discussion then took place, when the second question was tested, and twelve hands at once arose. The third—the most important question of all—was next to be tried. Quite a pause ensued. One juror, in his sympathies of kindness for the prisoner (who was his personal acquaintance or friend) and his afflicted family, shrunk from the 'fiery ordeal.' 'Can't we stop here?—Can't the law be vindicated and justice be satisfied if we pause here? *Must* we take the *life* of the unhappy prisoner?' Some discussion ensued; the mind of the juror seemed more calm, and he expressed his readiness to vote on the *final* question, which was then put, and twelve hands arose. The die was cast, and John W. Webster was pronounced Guilty of Murder.

"Thus ended the closing scene in the jury-room. What afterwards transpired in the court-room is already known to the public. When our foreman there pronounced that awful word—Guilty! the jury, as well as the prisoner, trembled and grew faint. And what a relief it was to us when we were again allowed to 'go free,' and rejoin our families and friends, after so long and painful a separation! And there was not a juror's heart but would have leaped with joy could the prisoner have been allowed the same unspeakable blessing." (p. 314.)

## ART. II.

*Parallèle des Diverses Espèces de Taille.* Thèse par M. MALGAIGNE, Membre de l'Académie Nationale de Médecine, Chirurgien de l'Hôpital St. Louis.—Paris, 1850. 8vo., pp. 72.

*Comparison between the different kinds of Lithotomy.* A Thesis by M. MALGAIGNE. Paris, 1850.

M. MALGAIGNE was the successful candidate for the Chair of Operative Surgery in the Academy of Paris, after a *concours* unparalleled in severity, and shared in by some of the most distinguished of the French surgeons. The Essay before us is a portion of the fruits of this competition, which has only just been brought to a conclusion, after a continuance of not less than a full third of a year. As might be expected, it is very complete in its way, elaborate in its history, and elaborate in its argument; such parts of it, therefore, as are likely to prove of general interest to the profession in this country, we propose to bring under the notice of our readers. The author commences by insisting upon the necessity of defining the *species* of Lithotomy which are to form the subject of the comparison; for without some restriction, it would be impossible even to enumerate all the modifications in the operation, which the lapse of eighteen centuries has brought forth. Some of these proposals have never been put in practice at all upon the living subject; or, having been tried, have immediately fallen into disuse, or, at best, have obtained only an ephemeral notoriety, and have subsequently been altogether forgotten. As lithotomy is practised upon the female as well as the male, and as the differences in the structure of the two sexes require corresponding differences in the operation, it is necessary to consider them separately.

In the male, lithotomy may be, and often has been, practised in three different situations; below, by the perineum; above, by the abdomen; and behind, by the rectum. The perineal operation is so ancient, that its origin is lost in the obscurity of past ages; but as its subsequent history and progress are well known, we shall not do more than refer to the practical conclusions which M. Malgaigne draws, when comparing it with its less popular companions. The same considerations induce us to omit a more particular account of lithotomy through the abdomen, the "high operation" as it is generally called. The operation by the rectum, as less known, demands a more special detail. Its origin dates little more than half a century back; and it was not until the year 1815, that the recto-vesical operation, as we now understand it, became recognised in surgery. Sanson proposed it with a view of avoiding the dangerous bleeding, so common in the lateral operation; but it will be seen that his original proposal was so hazardous that it was abandoned even by himself. He began by cutting from the anterior border of the anus, as far forward as the bulb of the urethra, and upwards, so as to divide six or eight lines of the rectum. At this point he gave the operator the choice of two courses,—either to divide a part of the base of the bladder, by cutting along the groove of the staff, from behind forwards, and from above downwards; or, if preferred, to open the membranous portion of the urethra, and then, having carried the knife just into the bladder, to divide from behind the neck of the bladder and the prostate gland. These proposals, coldly received in France, and hardly

judged serious in England and Germany, found warmer partisans in Italy. Vacca, however, having often observed the escape of stercoraceous matter into the bladder to be a consequence of the first-named proceeding, gave the preference to the second; and after warmly defending the recto-vesical operation against Scarpa, all at once abandoned it for his new proposal of lithotomy in the mesial line. Afterwards Dupuytren, having ingeniously modified it, by applying the lateral incision to the prostate, in his turn quitted it for the bilateral operation. And finally, Sanson being dead, the whole proceeding was nearly dead too, until its recent resuscitation by M. Maisonneuve, who does away with any external incision, and divides the prostate with a bilateral incision.

*Comparison of the various modifications of the recto-vesical operation.*—The advantage of the first proposal of Sanson was, that it offered a large space for the extraction of the calculus through the superior incision; but the escape of the fæces into the bladder, the symptoms which followed this, and finally the large number of persons who recovered with a permanent fistulous passage, caused its abandonment even by the inventor himself. On the other hand, the second method, while it did not admit of fæcal extravasation into the bladder, yet gave a less free exit to a large stone than the incision in the lateral operation, because it divided the prostate from behind, and through its shortest axis; and thus Scarpa in the dead, and Textor in the living subject, found the utmost difficulty in the extraction of moderate-sized stones, through the incision recommended by Vacca. Besides this, the left ejaculatory duct, the vas deferens, or even the vesicula seminalis itself, was almost always cut; and in a case that occurred to Géri, the peritoneum, which was abnormally prolonged, was also wounded. Finally, the most deplorable result was the number of recto-urethral fistulæ which remained after recovery. Cittadini had 1 fistula in 2 patients cured; Uccelli, 2 in 3; Guidetti, 2 in 8; Vacca himself, 6 in 25, without including a case in which the fistula did not form, until the wound appeared to have entirely cicatrized. It must be observed, however, that Giorgi and Cavarra only enumerate between them 2 fistulæ in 37 cases.

There still remains the question of the comparative fatality of the operation. Guidetti had 6 deaths in 12 cases; Vacca, though more fortunate, had yet 5 deaths in 30 cases. In 1832, Sanson enumerated, in the last edition of his 'Operative Surgery,' 89 cases, which terminated thus:—15 died; 62 were completely cured; 10 recovered with fistulæ; 1 had a fistulous opening which gave exit to the seminal fluid during its ejaculation; and in 1 other a fistula probably existed, since it is not included in the 62 cures. In other cases the operation was yet more disastrous; and the modifications proposed by Dupuytren and Maisonneuve do not seem to have diminished its fatality in any considerable degree.

In regard to the operations performed by M. Maisonneuve himself, the result is thus reported:

"The first operation was done on the 2d July, 1847.

"Patient 80 years old, 8 calculi, died 4 days after, of inflammation of the bladder and kidneys.

"Patient 79 years old, 2 calculi, died 5 days after, of inflammation of the urinary passages.

"Patient 28 years old; the cure was complete.

"Patient 45 years old, cured, but with a small fistula, which still exists, 3 months after the operation."

This mortality, however, it must be remarked, could not have been guarded against by any other mode of lithotomy; the existence of a fistula in one of the two patients who recovered, is a more significant fact.

After concluding this chapter upon recto-vesical lithotomy, M. Malgaigne draws a parallel between all the methods previously spoken of.

These are:—1. Among the operations by the perineum, the lateral operation by the large and the small incision, the operation in the mesial line of the urethra, the bilateral, and the quadrilateral operations.

2. Among the recto-vesical operations, the methods of Dupuytren and of Maisonneuve.

3. The high operation, without any wound in the perineum.

His conclusions, which are preceded by some observations upon the difficulty of making the comparison, are much as follows:

1. *With regard to the difficulties of the operation.*—In this respect the high operation is by far the easiest. It has not the first difficulty, that of finding the groove of the staff, nor that of carrying the instrument along a narrow passage; and the size and dilatability of the incision always insures an easy passage to the forceps and calculus. On the other hand, all these difficulties are incident to the perineal and recto-vesical operations; and the latter, moreover, has the additional inconvenience which results from the want of tension in the boundaries of the rectum. But there is yet another difficulty, which has, on more than one occasion, compromised the success of the operation. In cases where there are several calculi, or where the stone has been broken in pieces, the search for and the extraction of them are painful, tedious, and often incomplete. The high operation, again, in facilitating these processes, offers some advantages not possessed by the others.

2. *The pain.*—The pain incident to the first incision is common to all the operations; but it is not this which is the most to be dreaded. Ledran, after thirty-two years practice, received almost unanimously this reply to his question as to what was the most painful part of the operation,—the soundings, and the extraction of a large stone through a small opening. In these respects, also, the superiority rests with the high operation; and of the others, the large lateral incision, or the bilateral and quadrilateral incisions, have a great superiority over the small incision.

3. *Hemorrhage.*—This has occurred in all kinds of lithotomy. The median incision only wounds the bulb, and hardly touches the prostatic venous plexus. The lateral operation in the first incision may wound the transverse and the inferior hemorrhoidal arteries; but, above all, in the deep incision, it is liable to cut the superficial perineal, or even the internal pudic arteries. The bilateral operation of Dupuytren endangers the bulb, may wound the inferior hemorrhoidal artery behind, and, in the deep incision, puts the same vessels in peril as the lateral operation. The quadrilateral incision is precisely similarly circumstanced; and the same dangers are to be feared with the deep incision of the lateral or bilateral modification of the recto-vesical operation. But with the high operation, hemorrhage is an event so rare as to be inexplicable when it does occur.

4. *False passages*.—Here, again, the high operation is superior. If, says M. Malgaigne, some operators have too freely divided the cellular tissue which separates the bladder from the pubis and abdominal walls, such an error is so easy to be avoided, that it need not be enumerated in making the comparison. When once the finger is in the bladder, the instruments have a guide which effectually prevents them from going wrong. It is not so with any other mode of lithotomy. It is easy to get out of the groove of the staff, either with the knife, or the lithotome, or the gorget. After the incision is made, the forceps have to enter by an opening which recedes as they advance; often they injure the prostate or tear the rectum; and these accidents, though not occurring to an experienced hand, are yet the results of dangers which always exist.

5. *Tearing the soft parts*.—This is common to all the operations, except the high one.

6. *Wounds inflicted on neighbouring organs*.—These are, the rectum, the vesiculæ seminales and ejaculatory ducts, the bladder, and lastly, the peritoneum. Upon the latter of these dangers, the author makes the following observations:

“The wound of the bladder is generally considered as a source of danger in the operations below the pubes; it forms the basis, on the contrary, of the high operation, and at the same time, its greatest danger. It is certain that with a large opening for the escape of the urine, infiltration is generally avoided; but the danger is always there; and as it is that which has made surgeons dread the extra-prostatic incisions in the sub-pubic operations, where the urine escapes by means of a depending and direct opening, it is easy to understand that the fear is still greater in the high operation, in which the urine stagnates in the wound, and to find an exit, must rise to a level much higher than the floor of the bladder. Here, then, the large sub-pubic incisions must yield the superiority to the multiple incisions; and here the high operation has a fatal inferiority.

“This inferiority is further increased by the danger of wounding the peritoneum, which belongs exclusively, among all kinds of lithotomy, to the high operation.” (p. 57.)

7. *Rapidity of the cure*.—Making due allowance for the difficulty of obtaining accurate data to go upon, it seems tolerably certain that lithotomy by the perineum affords the most rapid cures. By this operation, cures have been obtained in three or four days: the most rapid cases after the “high operation,” have required twelve days; and a still longer period is required after the recto-vesical operation.

8. *Consecutive accidents*.—These are principally fistulæ, incontinence of urine, and injuries to the virile powers, which may amount to complete impotence. As regards fistulæ, the recto-vesical operation is the most unfavorable; and then the high operation. According to Scarpa, the lateral operation, well executed, should not afford more than two fistulæ in a hundred operations; but this is probably an unduly-favorable estimate. Incontinence of urine may follow the perineal and recto-vesical operations, however well performed; and it has been attributed by turns to the dilatation and to a large incision in the neck of the bladder. The high operation alone affords security against any injury to the procreative functions, or to consecutive affections of the testicle.

9. *Accidents which retard the cure or even occasion death*.—Arterial bleedings belong entirely to the sub-pubic operations; venous hemorrhage, also, is more common in them; and the other sources of bleeding,



as from the surface of the bladder, or from the kidneys, or even from the track of the incision, are alike incident to all.

Infiltration of urine on a large scale has been met with in the high operation, and in the lateral with a large incision; and on a small scale, terminating in circumscribed abscesses, has followed the small perineal incision as frequently as any other form. The recto-vesical operation, moreover, endangers the occurrence of inflammation of the rectum; and the high operation is liable to give rise to inflammation of the peritoneum, when this membrane happens to be injured. But, setting this aside, the chief causes of death after lithotomy, are inflammation of the bladder, inflammation of the kidneys engrafted on some chronic affection, inflammation of the gastro-intestinal membranes, often in connexion with the presence of worms, phlebitis, and the purulent diathesis. Pouteau, who performed lithotomy by the "*apparatus minor*," considered the condition of the lower belly as the index of the probable success, or the reverse, of the operation. Dupuytren, in 1834, stated that the causes of death among stone patients in Paris ranked in the following order:— 1st. Inflammation of the pelvic cellular tissue. 2d. Inflammation of the urinary passages. 3d. Peritonitis. 4th. Gastro-enteritis. Frère Côme frequently noticed the presence of worms in the autopsies; and at Naples, this complication is considered as of a most serious character. Finally, Morand has noticed, in lithotomy by the *apparatus major*, the occurrence of metastatic abscesses.

"Now," says M. Malgaigne, "Is there any species of lithotomy which excludes these essential causes of death? None. This affords us the key to two enigmas. The first is, the near approach that each kind of lithotomy makes to the rest in its fatality; and the second, the indifference that there is among surgeons, after so much dispute, as to arriving at any agreement in the choice of operations." (p. 59.)

10. *The mortality*.—This is not an easy question to decide. The proportion of deaths after lithotomy varies from year to year; and is especially affected by the ages of the patients, so that it would be easy enough to show an enormous disproportion between the different methods, by comparing the results of one operation among children with those of another among adults. Speaking generally, lithotomy is least serious in the first years of life; from 5 to 15 years of age, it affords the most fortunate results; above all, it is hazardous above the 50th year; and the care which M. Malgaigne has taken to ascertain the fact, enables him to state with certainty that, from the 70th to the 80th year, it is not more hazardous than in the 20 years preceding. After some further observations, indicative of the difficulty and even impossibility of making any dependable comparison of the relative mortality in the various kinds of lithotomy, the author concludes:—

1. That the incisions should be no larger than may be necessary to prevent tearing the parts; general reaction and purulent absorption being least to be feared with a small wound.

2. That as the pain of the operation must exhaust the patient, it should be abridged as much as possible; care being taken not to do unnecessary violence to the parts, or to substitute for a prolonged pain a more acute one.

3. That hemorrhage should be avoided by keeping clear of such vessels as are known to be in the way.

4. That the chances of infiltration of urine should be diminished by cutting the body of the bladder in as few cases as possible.

5. And, finally, that every care should be taken against such injuries of other organs, as might lead to fistulæ or incontinence of urine, or interfere with the integrity of the generative function. These things being premised, M. Malgaigne observes that there is no one method which is capable of fulfilling all these necessary conditions; and, therefore, there is no one method which can be said to be superior in all cases. For very small calculi, the median incision first, and then the lateral operation, keeping within the limits of the prostate, is to be recommended. For moderate-sized calculi, the lateral or bilateral operation; and for very large stones, the high operation. These, it seems to us, are just the conclusions which must naturally be drawn by a clear head from the premises which have been stated.

So much for the theory; but in practice it is not always possible to determine beforehand the size of the stone; and it is, therefore, desirable to be on the right side, and to judge it to be somewhat larger than we might suppose it to be before beginning to operate. M. Malgaigne inclines to the opinion, therefore, that those surgeons are right who commence by the lateral operation, which is easy to be completed in a few incisions; and who, if the stone then appears to exceed the medium size, immediately proceed to extract it by the high operation. This proposition we leave to the judgment of the reader without expressing any opinion upon it; our object being to record the views of M. Malgaigne, not our own, upon the important subject of lithotomy.

The rest of this Thesis we must pass rapidly over. The Second Part of it concerns Lithotomy in the Female; and, as in the case of the other sex, is preceded by an historical sketch of the operation. This, though very interesting, our space forbids us to dwell upon. We shall at once pass to the comparison which is drawn between the different kinds of lithotomy in the female. These are three in number, viz.: the sub-pubic method; the high operation, as in the male; and the vesico-vaginal operation.

1. *The sub-pubic operations.* Among these, though not exactly varieties of lithotomy, M. Malgaigne considers the extraction of calculi by dilatation; and to this question we shall confine ourselves. All the varieties of lithotomy in the female are very liable to be followed by incontinence of urine. It was Sir A. Cooper's opinion, that simple dilatation of the urethra gave the best chances of escaping this sad termination; but the whole question is one of great obscurity. Dionis, in whose time the practice of dilatation was much followed, stated that not more than one fourth of the women so treated recovered without incontinence of urine. Deschamps affirms that daily experience, and the experience of all ages, have proved that the extraction of a moderately-large, or even a small stone from the female bladder, is almost certainly followed by incontinence; and, therefore, except in the case of a very small stone, he preferred the high operation.

2. *The high operation.*—Little is stated on this head, that is not also referable to the same operation in the male; but the following table from Frère Côme shows that in his hands the mortality was less among females.

In 41 cases in which he gives the ages, the results are :

16 operations, from 3 to 17 years old, 2 deaths.
(These two deaths were at the ages of 5 and 9 years, respectively),
16 operations, from 20 to 50 years old, 5 deaths.
8       "       "       50 to 70       "       successful.
1       "       "       72       "       "

It is not surprising that, finding incontinence of urine to be an almost inevitable result of one method, and the proportion of deaths in the other to be as high as one sixth, surgeons should have endeavoured to devise some more successful plan.

3. *The vesico-vaginal operation*.—This operation has been performed in various ways; thus, Bussière and Gooch cut upon the stone; J. J. Rigal upon a catheter; M. Clénot rested the catheter upon a gorget introduced into the rectum, so as more certainly to guard its posterior part; Giorgi introduced into the urethra a *bistouri caché*, in a silver tube, the point of which protruded at the neck of the bladder, and was received upon a spatula which guarded the back part of the vagina, so as in this manner to divide the neck of the bladder and the vagina, from above downwards, and from within outwards. Vacca injected the bladder, and then plunged into it, through the vagina, either the "*bistouri-trocart*" of Thomas, or a common knife. The disadvantage of the vesico-vaginal operation, practised in any way, is that it endangers the formation of a fistulous communication between the two organs; but it is, in other respects, by far the easiest to perform. Velpeau estimates the chances of a fistula as, at least, one in four. It is, therefore, very difficult to determine the question, as to the admissibility of any of these operations in the female. The dangers of the high operation are such as to exclude it, except in very rare instances. The operation by the urethra has also its dangers; and moreover exposes the patient to the inconveniences of incontinence of urine. Dilatation endangers incontinence, without any other danger. Lithotomy through the vagina exposes the patient to great risk of a fistula. M. Malgaigne thus sums up:

"The inconvenience of a fistula and of incontinence of urine is about the same; while, thanks to the operation 'par glissement' of M. Jobert, a vesico-vaginal fistula is remediable, while incontinence is not so. I therefore think, with M. Velpeau, that the surgeon may practice dilatation when the stone is not larger than from five to six lines; but, notwithstanding this great authority, for all others, I prefer the vesico-vaginal operation." (p. 68.)

The Third Part treats of certain methods, applicable to both sexes. These are, lithotomy combined with lithotripsy, when the stone is too large; and lithotomy in two distinct operations,—that is, after the incisions are made, deferring the extraction of the stone until a less or greater interval has elapsed.

Want of space prevents our entering into these subjects; but the remarks of M. Malgaigne are well worthy of the reader's attention.

The whole Essay is indeed a very complete examination of the subject of Lithotomy, and we cordially commend it to the notice of the profession in this country.

Before concluding, it may be interesting to know the names of the competitors for the chair which has been awarded to M. Malgaigne, and the names of the Judges who decided in his favour.

The names of the Judges were,—on the part of the Faculty, MM. Roux (President), Andral, Bérard, J. Cloquet, Cruveilhier, Denonvilliers, P. Dubois, Laugier, Moreau, and Velpeau;—on the part of the Academy, MM. Baffos, Bégin, Gimelle, Huguier, and Jobert.

The names of the Candidates were, MM. Chassaignac, Gosselin, Jarjavay, Lenoir, Maisonneuve, Malgaigne, Nélaton, Richet, Robert, and A. Sanson.

### ART. III.

1. *Compendium de Médecine Pratique, ou Exposé Analytique et Raisonné des travaux contenus dans les principaux Traités de Pathologie Interne.* Par M. LOUIS DE LA BERGE, Docteur en Médecine, Agrégé à la Faculté de Médecine de Paris, Chef de Clinique Médicale à la même Faculté; M. ED. MONNERET, Agrégé à la Faculté de Médecine de Paris, Médecin du Bureau Central des Hôpitaux; et M. LOUIS FLEURY, Agrégé à la Faculté de Médecine de Paris, Membre Correspondant de l'Académie Royale de Médecine de Belgique. Ouvrage Autorisé par le Conseil Royal de l'Instruction Publique et par le Conseil de Santé des Armées de Terre.—Paris, 1836-46. 8 vols. 8vo, pp. 698, 638, 642, 636, 639, 634, 615, 496.
2. *Guide du Médecin Praticien, ou Résumé Général de Pathologie Interne et de Thérapeutique Appliquées.* Par F. L. I. VALLEIX, Médecin des Hôpitaux de Paris, Membre Titulaire de la Société Médicale d'Observation et de la Société Anatomique, Auteur de la 'Clinique des Maladies des Enfants Nouveau-nés,' du 'Traité des Neuralgies,' &c.—Paris, 1842-47. 10 vols. 8vo, pp. 576, 600, 627, 559, 632, 608, 586, 599, 847, 1006.
3. *Handbuch der medicinischen Klinik.* Verfasst von Dr. CARL CANSTATT, königlich-bayerischem Gerichtsärzte und Mitgliebre mehrerer gelehrter Gesellschaften. Zweite vermehrte Auflage.—Erlangen, 1843-47. 4 vols. 8vo, pp. 382, 1102, 919, 1109. Also published with the second title of *Die specielle Pathologie und Therapie vom klinischen Standpunkte aus bearbeitet von Dr. CARL CANSTATT, &c.*
4. *Handbuch der Pathologie und Therapie.* Von Dr. C. A. WUNDERLICH, Professor der Medicin, Vorstand der medicinischen Klinik zu Tübingen. Dritter Band.—Stuttgart. 8vo, pp. 1238.

THE titles which we have here recorded, pertain to four of the most extensive systems of medicine which have, during the last few years, appeared in France and Germany. Each work is well deserving of a separate review in our Journal; but we think that we shall be doing our readers a better service by considering them collectively in three or four continuous articles. In this manner, while we glean from them all that seems practically valuable, and, at the same time, presents any claims to novelty in Great Britain, we shall also be enabled to compare the state of medicine in those two countries, and to contrast French and German medicine with our own.

We must commence with a few introductory remarks regarding the style and nature of these works, and the conditions under which they were published.

The 'Compendium de Médecine' was commenced in 1836, by MM. De la Berge and Monneret; on the death of M. De la Berge, in 1838, M. Fleury became M. Monneret's colleague; and, to the united labours of these three physicians, we are indebted for a closely-printed, double-columned work of 5000 pages. The following remarks, extracted from the preface to the first volume, will afford the best idea of the plan which the authors had in view:

"It is the special object of the 'Compendium' to enumerate the received opinions on the nature, the seat, and the treatment of diseases, to discuss them conscientiously and impartially, and so to place the facts connected with each malady before the reader, that he may be enabled to apply them in his own practice. We have ever borne in mind that, as the cure of diseases is the great point to which all our efforts are directed, it is especially the practical side of medical questions to which our attention should be directed." (p. ii.)

They differ from the other writers, to whom we shall shortly direct our readers' attention, in adopting the alphabetical arrangement. All the articles are carefully written, and some are almost perfect monographs. In the articles on subjects of any importance, we find the following mode of arrangement:—etymology, modern synonyms, synonyms arranged chronologically, definition, pathological changes, symptomatology, progress or course of the disease, its duration, terminations, convalescence, consecutive phenomena, relapses, description of its species and varieties, essential or accidental combinations, diagnosis, prognosis, etiology, treatment, its nature and classification according to nosological systems, and, lastly, its history and bibliography.

The 'Guide du Médecin Praticien' is the work of M. Valleix alone. The first volume was published in 1842, and the tenth and last in 1847. The 6000 pages he has here contributed to our medical literature are inferior both in quantity and (in some respects) in quality to the 5000 double-columns contained in the 'Compendium.' They are not so complete in those departments which belong to the literature of medicine; they do not exhibit so general a knowledge of English and German authors; but, for all that, we are not by any means sure that they do not constitute the better guide to practice—the point which our author seems chiefly to have held in view.

An abstract of the mode of arrangement of this and the two following works will be not altogether uninteresting, as elucidating the different nosological systems of these writers, and indicating the errors into which they fall.

The first two volumes of Valleix's 'Guide,' constituting the *first book*, treat of *Diseases of the Respiratory Passages*, which are considered in the following order:

- I. *Diseases of the nasal fossæ.*
- II. *Diseases of the larynx.*
- III. *Diseases of the trachea.*
- IV. *Diseases of the bronchi.*
- V. *Diseases of the parenchyma of the lung.*
- VI. *Diseases of the pleura.*
- VII. *Thoracic diseases of undetermined seat, namely, asthma and pertussis.*

The third volume constitutes the *second book*, and treats of *Diseases of the Organs of Circulation*, which are thus arranged :

**FIRST SECTION.—DISEASES OF THE HEART AND PERICARDIUM.**

- I. *Succinct description of the normal state of the heart*, giving an account of its situation, volume, movements, and sounds.
- II. *Diseases of the endocardium.*
- III. *Diseases of the muscular texture of the heart.*
- IV. *Diseases of the pericardium.*

**SECOND SECTION.—DISEASES OF THE ARTERIES.**

- I. *Diseases of the aorta.*
- II. *Diseases of secondary arteries.*

**THIRD SECTION.—DISEASES OF THE VEINS.**

- I. *Diseases of the pulmonary artery.*
- II. *Diseases of the venous system generally.*

**FOURTH SECTION.—DISEASES OF THE BLOOD.**

- I. *Diseases originally dependent on an abnormal state of the blood.*  
Under this head he places plethora, anæmia, chlorosis, and scurvy.
- II. *Diseases in which the organs of circulation and some other system are conjointly affected* ; namely, syncope and asphyxia.

There cannot, we think, be a question that this is a most unsatisfactory, clumsy, and unphilosophical arrangement. The “diseases of the blood” to which Vallex alludes—namely, plethora, anæmia, chlorosis, and scurvy—have no more intrinsic connexion with “the diseases of the organs of circulation,” than have dysentery or typhus fever; whilst the morbid condition of the blood, dependent on diseased kidney, which does tend to excite inflammation of the membranes of the heart, is altogether unnoticed. Dr. Taylor's Memoirs, it is true, were published subsequently to the appearance of this volume of Vallex; but we much doubt whether our French brethren are inclined to accord to him the priority of the discovery of the intimate connexion between *uræmia* and heart-inflammations.

All these attempts at a perfect nosological arrangement break down sooner or later. But, to return to our analysis of the second book, there is a **FIFTH SECTION**, on **DISEASES OF THE LYMPHATIC VESSELS**, which is devoted for the most part to the consideration of scrofula.

The *third book* extends over the fourth, fifth, and part of the sixth volumes, and embraces the consideration of the *Diseases of the Digestive Canal*. It treats of—

- I. *Diseases of the mouth.*
- II. *Diseases of the pharynx.*
- III. *Diseases of the œsophagus.*
- IV. *Diseases of the stomach.*
- V. *Diseases simultaneously affecting the stomach and the intestines.*
- VI. *Diseases of the intestines.*

Here the nosologist is again at fault. Being desirous to get rid of the piles, he appends them as a tail-piece to the diseases of the intestinal canal. We think they would have found a more natural position in the chapter on the diseases of the veins. An article on intestinal worms concludes the third book.

*Diseases of the Appendages to the Digestive Canal*—namely, of the liver, spleen, pancreas, and peritoneum, occupy the *fourth book*.

The *fifth book* treats of the *Diseases of the Genito-urinary Organs*, and extends over the seventh and eighth volumes. It is arranged in the following manner :

- I. *Diseases of the kidneys.*
- II. *Diseases of the urinary canals*, (under which are included the subjects of gravel, calculi, and *diabetes* !)
- III. *Diseases of the bladder.*
- IV. *Diseases of the prostate and the seminal vesicles.*
- V. *Diseases common to both sexes* (including an article on *balanitis* !).
- VI. *Diseases peculiar to women.*
  - a. *Diseases of the vulva.*
  - β. *Diseases of the vagina.*
  - γ. *Diseases of the uterus.*
  - δ. *Diseases of the ovaries* ; with an appendix on inflammatory, pelvic, and iliac tumours.

The *sixth book*, occupying the ninth volume, is devoted to the consideration of the *Diseases of the Nervous System*, which are thus divided :

- I. *Diseases of the nerves*, embracing neuritis and neuralgia, both considered generally and in reference to particular nerves.
- II. *Diseases of the encephalon.*
  - a. *Diseases of the membranes.*
  - β. *Diseases of the cerebrum, cerebellum, and pons.*
- III. *Diseases of the cerebro-spinal membranes.*
- IV. *Diseases of the spinal cord and its membranes.*
  - v. *The neuroses*, (embracing the consideration of various forms of paralysis, of convulsions, epilepsy, tetanus, chorea, hysteria, insanity, angina pectoris, &c. &c.)

The *seventh book*, which commences the last volume, treats of *Diseases of the Areolar Tissue and Organs of Locomotion* ; the *eighth book* is devoted nominally to *Diseases of the Organs of the Senses*, but is almost occupied by skin diseases ; the *ninth book* treats of *Cutaneous Febrile Diseases* ; the *tenth book*, of *Fevers generally* ; the *eleventh book*, of *Diseases communicated to man from the lower animals*, with an appendix on parasitic animals ; and the *twelfth book*, of what the French term “ Intoxications,” and poisoning.

Valleix does not make so many subdivisions as the authors of the ‘Compendium,’ and yet he gives us sufficient for all practical purposes. We usually have—

*The General History of the Disease, from the earliest Time of its Recognition.*

- I. *Definition ; synonyms ; frequency.*
- II. *Causes.*—a. *Predisposing causes* ; β. *Occasional causes.*
- III. *Symptoms.*
- IV. *Progress ; duration ; termination of the disease.*
  - v. *Anatomical lesions.*
- VI. *Diagnosis ; prognosis.*
- VII. *Treatment.*

His observations on diagnosis and treatment are invariably sound and deserving of attention.

Canstatt's four volumes next claim our attention. Some of our readers will doubtless be prepared for the tone and general character of the work, when we inform them that it is dedicated "to his immortal master," Dr. Schönlein, "with a feeling of unbounded gratitude and of the most profound reverence." Schönlein's views are only known (out of Berlin) by his '*Allgemeine und specielle Pathologie und Therapie*,' a work which, although roughly concocted from the aggregate notes of several anonymous pupils, and containing, according to his own statements, many erroneous expressions of his views, passed through no less than four large editions in the course of seven years; and by his '*Klinische Vorträge in dem Charité-Krankenhaus zu Berlin*,' which were edited by Dr. Güterbock, and reached a third edition in two years. We think it extremely probable that the avidity with which Schönlein's '*Sayings and Doings*' are swallowed up by our German friends, may, in some measure, account for the fact of the very early call for a second edition of the work we are now considering. We are far from regarding Canstatt's work as being any the worse for being somewhat deeply imbued with the views of his "immortal master," who is doubtless one of the best practical physicians of the present age, although a little apt to ride his favorite hobbies too hard. We shall have occasion to notice this fault in a future part of this series of articles.

The first volume is entirely devoted to the morphological portion of clinical medicine, or, in other words, to the elementary forms of disease; as, for instance, to hypertrophy, atrophy, plethora, anæmia, congestion, inflammation, blood-diseases, &c. &c.

Specific morbid processes constitute the subject-matter of the second volume. He divides them into two great classes:—

- I. Acute cosmical diseases; and
- II. Constitutional dyscrasie.

The first class is subdivided into six orders, which we shall now enumerate:

"1. *Specific exanthematous morbid processes.*

Variola.

Modified variola (morb. variol.)

Varicella.

(Vaccinia, although strictly belonging to the diseases produced by animal poison, naturally finds a place here, in consequence of its protective powers).

Scarlatina.

Morbilli.

Miliaria.

Erysipelas.

Erythema.

Zoster.

Urticaria.

Rubeola. [We need hardly observe, that the Germans make a distinction, which is not recognised in this country, between *Masern* (Morbilli) and *Rötheln* (Rubeola).]



2. *Malaria-contagiosi* (morbid processes excited by specific telluro-atmospheric miasms).  
Intermittent fever.  
Yellow fever.  
Cholera.  
Plague.  
Dysentery.
3. *Typhus* (morbid processes induced by a specific animal miasm).  
Petechial typhus.  
Ileo-typhus.  
Colo-typhus, or dysenteric typhus.
4. *Atmospheric contagions* (morbid processes induced by atmospheric agents which sometimes take the form of miasms).  
a. Diseases produced by exposure to cold.  
a. Rheumatoses.  
b. Catarrh; influenza; pertussis.  
b. Diseases arising from heat.  
Choloses.
5. *Animal-poison contagions* (morbid processes produced by specific morbid poisons of animals).  
Equinia.  
Anthrax.  
Hydrophobia.  
Vaccinia.
6. *Chronic contagions*, produced by specific contagions or specific endemic relations  
Syphilis.  
Lepra.  
Plica Polonica." (vol. ii, pp. 11-12.)

As an appendix to the fifth order, he considers the *Toxicoses*, and the *Diseases connected with development*. His remarks on the former subject are for the most part confined to the symptoms, anatomical characters, and treatment connected with the ordinary poisons, -namely, arsenic, corrosive sublimate, tartar emetic, copper, concentrated acids, caustic alkalies, liver of sulphur, acrid animal and vegetable poisons, opium, belladonna, prussic acid, nux vomica and strychnine, poisonous fungi, and poisonous sausages (of very common occurrence in Germany); concluding the section with more lengthened considerations regarding the morbid conditions induced by ergot, mercury, lead, and iodine. Under the latter head, after briefly alluding to difficult dentition, and to the peculiarities that often exhibit themselves about the period of incipient puberty, he enters with considerable fulness into the subject of *puerperal fever*!

While the "acute cosmical diseases" occupy 975 pages, the "constitutional dyscrasias," which constitute his second class of specific morbid processes, occupy little more than 100. In this class he places scurvy, purpura, the dyschymoses of Schönlein (embracing icterus and urodialysis, Schönlein's term for ischuria), gout, hæmorrhoids, rachitis, and osteomalacia. No classification of this sort can be perfect. Tuberculosis, acrophulosis, (if, indeed, there are any differences between the two processes,) and cancer, are unquestionably "constitutional dyscrasias," and our author has to apologise for their absence on the plea of their pre-

engagement to "the elementary forms of disease," treated of in the first volume.

The two remaining volumes, constituting the greater bulk of the work, are devoted to "special local pathology;" the diseases being classed topographically.

The arrangement adopted in the consideration of each individual disease is very similar to that of Valleix. Commencing with the synonyms, we then have the bibliography which is usually very perfect, the anatomical characters, the symptoms, the diagnosis, the causes, the course and terminations, the prognosis, and the treatment.

We have only further to add, that there is an excellent index, extending over 54 double-columned pages, which adds very much to the practical value of this work.

Wunderlich's '*Handbuch der Pathologie und Therapie*' will, when completed, form three volumes. It is intended by its learned author, as we learn from his prospectus, to present a comprehensive view of collective (general and special) Pathology and Therapeutics, brought up to the existing state of German scientific medicine, that is to say, based on rational experimental physiology, and on the enlarged knowledge of animal chemistry and pathological anatomy of the present day.

The first volume, which is not yet completed, will contain the general principles of pathology, etiology, and therapeutics, the theory of morbid crases, and affections of compound tissues.

The second volume will contain (according to the prospectus):

1. *Affections of the central organs of the nervous system.*
2. *Affections of the locomotive apparatus* (bones, muscles, joints).
3. *Affections of the general investments of the body.*
4. *Affections of the male generative organs.*
5. *Affections of the female generative organs.*

The third volume, which is the only perfect one, contains—

1. *Affections of the organs of circulation.*
2. *Affections of the organ of respiration.*
3. *Affections of the digestive organs.*
4. *Affections of the urinary organs.*

We believe that Wunderlich's '*Handbuch*,' when completed, will be the most scientific and the most valuable book of the four whose titles stand at the head of this article. While nearly all extraneous matters are excluded, everything of real value is retained; and his writings (we refer not only to the present volume, but to his '*Pathologie des Blutes*') evince his extensive knowledge of physiology, organic chemistry, and pathology. His arrangement of each subject is simple. After a bibliographical history of the disease, he usually gives distinct paragraphs merely to the etiology, the pathological anatomy, the symptoms, and the treatment. If we were called upon to adduce any objections to his work, we might perhaps say that we thought he sometimes was unnecessarily minute in his subdivisions of disease, and that his therapeutic sections were too concise. In this latter respect he contrasts strongly with Canstatt, who usually mentions every remedy that has ever been suggested, and almost every mode in which it has been exhibited.

With all these stores before us, it is difficult to know where to begin. As, however, it is expedient that we should select some department on which Wunderlich's 'Handbuch' is complete, we shall commence with the *Diseases of the Digestive System*, to which we give a preference, inasmuch as no works on this subject have lately been reviewed in our pages.

Having already expressed our predilection in favour of Wunderlich, we shall adopt his arrangement and draw pretty freely from him, selecting occasional remarks from the other works when it seems expedient to do so.

Commencing with the affections of the mouth and throat, we shall proceed to the general consideration of the diseases of the intestinal canal, and from thence to that of the special morbid changes to which its various parts are liable, concluding this department of our subject with a notice of the diseases of the liver, the spleen, the pancreas, and the peritoneum.

The length to which these introductory remarks have extended will, however, prevent us from getting over much of this ground in the present number; indeed, we shall not attempt to extend this article to the consideration of any of the special forms of disease.

We cannot convey to our readers a clearer idea of the German *exhaustiveness* possessed by this writer; than by presenting them with his tabular view of the *diseases of the mouth and throat*. After a physiological proem on the mouth and throat: 1, as a part of the digestive organs; 2, as connected with a special sense (that of taste); 3, as a portion of the organs of voice; and 4, in connexion with respiration; he takes a general view of the history, etiology, pathology, phenomenology (to use one of his favorite words), diagnosis, and general treatment of these affections. The diseases are classed under *nervous affections*, and *anatomical disturbances*.

Under the *nervous affections* there are placed:—

- A. Anomalies of the sense of taste.
- B. Disturbances in the functions of the fifth pair of nerves.
- C. Motor disturbances.

While under the *anatomical disturbances* we find:—

I. Those occurring on the mucous membrane and submucous cellular tissue of the mouth and throat.

- A. Anæmia.
- B. Extravasations. Hæmorrhage.
- C. Hyperæmia. Inflammatory exudations.
  - 1. Simple hyperæmia, and mucous catarrh of the mouth and throat.
    - a. Acute form.
    - b. Chronic form.
  - 2. Vesicular inflammations.
  - 3. Pseudomembranous exudations.
    - a. Simple plastic stomatitis.
    - b. Croupous stomatopharyngitis.
    - c. Diphtheritic stomatopharyngitis.
    - d. Aphthous stomatopharyngitis.
  - 4. Pustular stomatopharyngitis.
  - 5. Submucous infiltration and phlegmonous stomatopharyngitis.

- D. Cancerous deposits.
- E. Benignant tumours.
- F. Processes connected with mortification.
  - 1. Ulcerations.
    - a. Simple ulcers.
    - b. Scorbutic affections of the mouth.
    - c. Mercurial affection of the mucous membrane of the mouth.
    - d. Syphilitic affections of mouth and throat.
  - 2. Softening and gangrene.
    - a. Gangrenous stomatitis. Noma.
    - b. Gangrenous ulitis or gengivitis.
    - c. Pharyngitis gangrenosa.
- G. Parasites.
- H. Inorganic deposits.
- II. Those occurring in the tongue.
  - A. Anomalies in formation, nutrition, and position.
  - B. Hyperæmia and inflammation.
  - C. Cancerous degeneration.
  - D. Benignant tumours.
  - E. Gangrene.
- III. Those occurring in the tonsils.
  - A. Hyperæmia and inflammation.
  - B. Cancerous and other tumours.
  - C. Mortification.
- IV. Those affecting the salivary glands, their ducts, and the saliva.
- V. Those occurring in the jaws.
  - A. Alterations in the osseous substance and in the periosteum.
    - 1. Inflammatory affections.
    - 2. Depositions, tumours, &c.
    - 3. Rachitis and osteomalacia.
    - 4. Mortification.
  - B. Affections of the antrum.
  - C. Affections of the joint.

This tabular sketch will afford our readers an idea of the minute subdivisions in which Wunderlich delights to indulge. The subject-matter extends over 126 closely printed pages. Valleix devotes upwards of 400 pages to the diseases of the mouth and throat, and Canstatt 80 to those of the mouth alone.

We select the following remarks from Wunderlich on "the general treatment of affections of the mouth and throat."

In speaking of local bloodletting, he observes, that it is seldom required unless where there is severe hyperæmia. Although it often moderates the acuteness of the disease, it frequently prolongs its course, and without being able to check suppuration, makes the process slow and tedious. Local bloodletting is of little use unless applied *directly* to the diseased spot; if applied merely to adjacent parts (as, for instance, to the neck, in affections of the tongue or tonsils), its effects are hardly perceptible.

There is nothing in his remarks on cataplasms deserving of special notice. When merely a superficial effect is required, he recommends dry

heat, as, for instance, small bags of hot bran, chamomile or elder flowers, aromatic labiatae, &c. Vapour possesses advantages over poultices in cases when the latter cannot be applied directly to the diseased part, or in affections of the throat. The vapour-douche, moreover, has a far more energetic action than the local applications to which we have previously alluded. There is nothing else in his remarks on local applications to which we need advert. In reference to general and internal remedies, he expresses himself as strongly opposed to emetics, in consequence of their giving rise to cerebral congestion, a view adopted by several German physicians, but we believe a theoretical rather than an actual objection. In the early stage of inflammation of the tonsils, they are often of the highest service.

The first special morbid changes to which we shall advert, are those termed in Wunderlich's table *pseudomembranous exudations*.

The first variety, *simple plastic stomatitis*, is the *muguet* of French writers. It is a disease regarding which there has been considerable confusion of names; and in adopting the word stomatitis, we by no means intend to express our belief that the exudations are confined to the mucous membrane of the mouth. Valleix, who has devoted much attention to the diseases of early infancy, observes that this affection is by far the most common during the first two months of life, although it may occur at any period of infancy or even in adults. Among the predisposing hygienic conditions, we must especially rank improper food. Valleix has always found, on investigating cases of this affection, whether in town or country, that the children were being fed on farinaceous food, and he has never known an instance in which a child brought up exclusively at the breast for the first few months suffered from it. He has, moreover, observed that more than half the cases occur in the three hottest months of the year. Wunderlich suggests that this may, in part, be owing to the more rapid decomposition which the food given to the infant, or the fragments of food adhering to the mouth, undergo in hot weather, and in part to the general frequency of intestinal inflammation in summer.

The opinions of Valleix, that the diseased condition of the mucous membrane of the mouth is merely "the expression of a general intestinal disease," is perhaps too general, although most commonly true. His observations lead him to conclude, that in the great majority of cases the pseudo-membranous exudation of the mouth is preceded for some days by erythema of the nates and thighs, which in some instances is certainly independent of irritation caused by the contact of fæces or urine. After the erythema, we usually have diarrhoea, more or less febrile disturbance, and then the buccal exudation.

It is chiefly in the continental Foundling Hospitals that this disease has been studied. Indeed it is better known by the French word *muguet* than by any other, for our word *thrush* is correctly applied only to the smaller patches of exudation—the *aphthæ*.

We have condensed the following sketch of the symptoms from Valleix's description.

The first stage is almost invariably attended by reddening of the mucous membrane of the mouth, and especially of the tongue. On removing (when the disease is more advanced) a portion of exudation, the surface presents a blood-red, glistening appearance; and not unfrequently drops

of blood actually escape. The papillæ become swollen and prominent, either simultaneously with the above-named reddening, or very shortly afterwards.

The pseudo-membranous exudation appears two or three days after the redness, and first shows itself on the enlarged papillæ, and from the tongue extends in small masses over the inner surface of the lips, the cheeks, and the palate. In the course of three or four days, these patches augment in size and thickness, and form a layer which covers the whole or a considerable portion of the tongue and mouth. These larger masses of exudation have a white or somewhat yellowish colour, are soft, break down under the fingers like fresh cheese, present no trace of organization to the naked eye,\* are not attached by any kind of filament to the subjacent tissue, and soon reappear after they have been renewed. Their presence causes great irritation, and we observe the infant continuously moving the tongue and jaw to get free from the annoyance. Indeed the degree of this motion often serves as a measure of the extent of the disease. Ulcers frequently present themselves on the mesial line of the palate, and at the frænum of the tongue.

In addition to these local symptoms, we frequently observe in *muguet* obstinate vomiting, owing doubtless to the extension of the local symptoms to the œsophagus and stomach, tenderness and a tympanitic state of the abdomen, and diarrhœa; the stools at first being abundant, liquid, and of the ordinary yellow colour, but subsequently becoming green. Valleix has never succeeded in detecting the peculiar exudation in the stools, although he regards its presence as possible.

The erythema, to which we have already alluded, often spreads over a great extent, becomes very intense, and is frequently followed by ulceration. Moreover pustules, papulæ, and even bullæ, may appear on different parts of the body, but these cannot be regarded as peculiar to this disease. There is considerable fever preceding and accompanying the disorder, and pneumonia is a not unfrequent complication.

The second stage may be very briefly described. It is the stage of collapse. The pulse diminishes in tone and in frequency; the extremities and afterwards the trunk become cold; the face becomes pinched, the emaciation extreme, and the infant resembles a miniature old man. The diarrhœa diminishes, and the stools again assume more of their normal colour; the vomiting ceases, the exudation becomes less abundant, and the child expires.

We have given this description of what Valleix very appropriately terms *muguet grave*, as he has witnessed it at the Hôpital des Enfants-Trouvés, because we believe and trust that it is a disease almost unknown in this country in anything like such intensity. In the cases observed by that physician, the duration varied from seven to thirty-seven days. The mortality is very high. In 140 cases in the wards of M. Baron, in the above-named hospital, the number of recoveries was only 29; and of 22 cases under the care of Valleix, only 2 recovered.

The fourth variety of the pseudo-membranous exudations, *aphthous stomato-pharyngitis*, includes cachectic, secondary aphthæ, those exudations

\* We do not advert to the microscopical appearances, because the subject was fully entered into in a review of Dr. Berg's work, 'Om Torsk hos Barn,' in the twenty-fourth volume of the 'British and Foreign Medical Review.'—R.V.

which occur so frequently in the mouth during the height and towards the conclusion of severe illnesses. We extract from Wunderlich the following remarks on the etiology of this affection :

"Aphthæ occur at every period of life, but most commonly in childhood and old age, probably because those are the periods of life at which diseases giving rise to aphthæ most commonly occur. Amongst these we may especially mention those inducing marasmus, especially the severe diseases of the intestines in which reparation of the effete tissues is prevented, as gastro-enteritis, cholera, and softening of the stomach in children; and cancer of the stomach, stricture of the œsophagus, and gastric catarrh in aged persons. Moreover they occur in all severe diseases in which there is much change in the blood; as (among the acute) in very severe cases of pneumonia and typhus, and (among the chronic) in cases of tuberculosis and cancer, as well as in the marasmus which accompanies long-continued, general dropsy. These are, however, only the most common illustrations of the etiology of aphthæ, and these exudations may occur in any primary disease, provided only that the patient be in a state of great constitutional disorder, and that marasmus ensue." (Wunderlich, vol. iii, p. 678.)

His therapeutic rules are almost identical with those adopted in British practice.

1. When the aphthæ occur during the height of an acute disease, their treatment must be a secondary matter. The most we can do is to keep the mouth cleansed, to remove the thickest patches of the pseudo-membrane, and to sprinkle the spots very lightly with a little borax or alum. The most important indication is the treatment of the primary disease.

2. In aphthæ occurring in the course of advanced, incurable, chronic diseases, we can afford the greatest palliation by strict attention to the cleansing of the mouth, and by pencilling it with honey, borax, citric, and other weak acids, alum, &c. In this way we may often get rid of them for a time, but they are certain in a short time to recur.

3. The treatment is most successful in aphthæ occurring in the course of curable chronic affections, or during convalescence. In addition to the local means already indicated, we may, if necessary, have recourse to nitrate of silver or concentrated mineral acids; while the system must be supported by nutritious and digestible food, and, if there are no contra-indications, tonics (iron, quinine, gentian, &c.) must be given.

*Post-pharyngeal abscess* is the next disease to which we shall advert—a rare affection on which little has been written, but which is accompanied with much danger.

In cases of abscess beneath the pharyngeal mucous membrane, the symptoms may vary considerably according to the course taken by the pus. Valleix observes that the tumour formed by the abscess may either remain fixed to the posterior wall of the pharynx, or the pus may find its way downwards towards the thorax, or it may burrow towards the upper part of the larynx, and compress the glottis. Cases illustrative of this last-named course of the pus are given by Ballot in the 'Archives Gen. de Méd.' Oct. 1841, and by Carmichael in the 'Edin. Med.-Chir. Trans.,' 1820.

"In the first case," says Valleix, "there is acute pain, difficulty of deglutition, a nasal voice, and an occasional obstruction to free respiration. In short, the symptoms are much the same as we observe in abscess of the tonsils not compressing the larynx.

"In the second, in addition to the above signs, there is great intensity of the general symptoms, and pain in the side of the thorax along which the pus is

burrowing, or the swelling and redness of the neck, and the fluctuation which may be detected externally sufficiently indicates the nature of the disease. [Valleix refers to cases recorded by Londe and Méandre Dassit as illustrating this form.]

"Finally, in the third, the patient is almost suffocated, the respiratory efforts are painful, there is whistling inspiration, and we have the ordinary phenomena of asphyxia; in short the signs of œdema of the glottis are so marked, that it is easy to understand how readily the two affections may be mistaken.

"In this case we must institute a very careful examination of the pharyngeal cavity. Having opened the mouth with a wedge of wood placed between the teeth, we must pass the finger deeply to the posterior and inferior wall of the pharynx, when, if the tumour can be reached, fluctuation may be readily detected. We must be especially careful to ascertain the state of the epiglottis and larynx, for if we can touch the opening of the glottis and ascertain its integrity, on pushing aside the tumour, the cause of the severe respiratory symptoms becomes sufficiently apparent." (Valleix, vol. iv, pp. 313-14.)

With due attention to the above remarks, any difficulty between the diagnosis of post-pharyngeal abscess and œdema of the glottis (the only affection with which it is likely to be confounded) may be readily avoided.

The essential treatment consists in the early opening of the abscess. In some cases the lancet and bistoury have been found insufficient, and it has been necessary to have recourse to the pharyngotome; and cases are recorded in which the symptoms have been so urgent that recourse has been had to tracheotomy.

Wunderlich's remarks on cancerous deposits and benignant tumours, and on the different forms of ulceration of the mouth, are very brief, and call for no remark; and we pass to the consideration of a disease, which, though long recognised, has only recently been carefully studied. We refer to *gangrenous stomatitis*, which, perhaps in slightly different forms, has been described by different authors as *cancer aquaticus*, *noma*, and *gangrene of the mouth*.

We condense the following remarks from Wunderlich's section on the etiology of this affection. It most commonly occurs between the ages of three and eight years, and is very rare after the twelfth year. It is apparently uninfluenced by sex. Although occasionally seen to attack robust healthy children, it is almost entirely limited to the poorer classes, whose constitutions have been deteriorated by depressing influences. It is more frequent in the northern than in the southern countries of Europe, and in large cities than in villages. Propinquity to the sea or to large rivers with low banks, seems to predispose to the affection, and it is most common in spring and autumn. Taupin,\* whose experience in this disease is very extensive, having observed 107 cases in the "Hôpital des Enfants" in the course of four years, believes that it is epidemic and contagious. Wunderlich is opposed to this view, and certainly as far as our limited experience in this affection goes, we fully concur with him. It is towards the close of, or during convalescence from other acute diseases, especially measles, scarlatina, pertussis, and pneumonia, that this usually manifests itself. In cases where the conditions favorable to the development of the disease are present, a burn or any local injury inflicted on the cheek may act as an exciting cause.

There are two distinct forms under which gangrenous stomatitis manifests itself. The following is Wunderlich's description of the patho-

\* See his Memoir, in the 'Journal des Conn. Médico-Chir,' Avril, 1839.



logical anatomy and symptoms of these two forms. We commence with the *local phenomena* :

"*First form.* The disease directly shows itself to be gangrene. This is *noma*,\* using the word in a limited sense, or *stomatite charbonneuse* of Taupin. One half of the face (usually the left) exhibits an indistinctly defined pale or violet marbled swelling, especially on the eyelids, with a peculiar oily appearance of the skin. An erysipelatous redness of the cuticle is also frequently observed. The inner surface of the cheek is livid and of a dark red colour. A small vesicle (which is often overlooked) now appears, generally on the outer surface of the cheek, near the mouth, but sometimes on the mucous membrane, and lying on a hard, dark red, and often livid ground; this vesicle shortly bursts and becomes converted either into a superficial erosion or into a deepish ulcer, which usually becomes soon covered with a slough of considerable size, measuring in diameter from several lines to an inch. It occasionally happens that there are several distinct points of origin of the morbid process, which either unite or remain isolated. The hardness and cedema of the surrounding parts increase until the whole face and occasionally the neck are swollen. An excess of saliva, often bloody or of a blackish colour, and of a fetid smell, dribbles from the mouth. Mastication is difficult if not impossible, while the voice becomes indistinct, and speaking difficult. The external sloughing goes on extending, while the parts beneath become so rapidly affected, that in a few days the cheek, a part of the lip, and the eyelid are reduced to a gangrenous, highly fetid pulp; and there is thus a lateral opening into the cavity of the mouth. The teeth, which may be observed through the opening, become loose or fall out after the destruction of the gum; and the adjacent portions of bone become to a greater or less extent exposed and destroyed. The whole neighbourhood of the gangrenous spot has a sodden, livid appearance. In this destructive process, which is almost always limited to one side of the face, and which extends much more widely on the inner surface of the cheek, and in the cavity of the mouth than externally, there is a perfect absence of pain, or at most only a dull sensitiveness. In the rare cases in which the gangrene is arrested, there is formed an inflammatory (hyperæmic) line of demarcation, suppuration commences in the circumference, and the gangrenous spot becomes converted into an ulcer, which gradually assumes a clean and healthy appearance, and, after cicatrizing for some months, becomes healed.

"Where there has been great destruction of tissue, cicatrization is always attended with considerable disfigurement; and the pre-existence of gangrene of the mouth may be recognised through life by the ugly, strongly-contracted cicatrix, puckering the eyelid, the ear, or the neck, uncovering the eye, and distorting the mouth, like the scars left on the face after deep burns.

"The *second form* appears to be incomparably the more frequent of the two. It is, however, less strongly marked, and owing to its greater affinity with other affections, and a deficiency in correct observation, there is much discrepancy in what has been written on it. It runs a slower course than the former, and does not so frequently break out with suddenness during convalescence from acute diseases. Instead of the gangrenous destruction with which the first form commences, we here have pseudo-membranes of unhealthy appearance on the inner surface of the cheek, and ulcers either there or on the outside of the cheek, and at the corners of the mouth. They either gradually or at once assume a very unhealthy character, emit a powerful and fetid odour, and become covered with sloughs or viscid masses, while the neighbouring parts become livid and cedematous, and the destructive process affects the deeper tissues. From this stage its further progress is similar to that of the first form, excepting, indeed, that the gangrene does not usually extend with the same extraordinary rapidity.

"The *remote and general symptoms* next claim our attention.

"The local destruction in gangrenous stomatitis is always accompanied by great general disturbance of the organism, which in the great majority of cases (eight or nine out of every ten) induces a fatal termination. The local affection is, moreover,

\* This term has been applied by some authors to *ulcerative stomatitis*, an incomparably less severe affection.—RAY.

often preceded by general disorder (premonitory symptoms), or is complicated with affections of some other organs.

"1. Premonitory symptoms are only observed when the affection appears immediately as gangrene, and is developed in the advanced stage of improvement or convalescence of a pre-existing disease (measles &c.); for where there is pseudo-membranous or ulcerative stomatitis, the gangrenous mortification is only announced by a gradual exacerbation of the symptoms, or at most by a shivering, an increased appearance of collapse, hæmorrhage, &c. When the gangrene supervenes at the height of some other disease, these premonitory symptoms are rarely observed. It even frequently happens that where the earlier disease has abated to a very great degree, and convalescence is considerably advanced, this fatal secondary affection will manifest itself unannounced by any premonitory symptoms, commencing unexpectedly and suddenly by local swelling. The premonitory symptoms, when they occur in these cases, are not very severe, as for instance lassitude, irritability, loss of appetite, disordered digestion, rigors, slight fever, and a somewhat suffering and collapsed appearance.

"2. More important disturbances are not manifested until the destruction has commenced or has even made considerable progress; and it would appear as if they depended on the gangrenous ichor being taken up into the circulating fluid. At first these disturbances are very moderate. The patient appears depressed and shaken, drowsy by day, and restless at night; the skin of the trunk is often very hot, while that of the extremities is cool; the pulse becomes rapidly more frequent and small, and towards the termination is so weak as hardly to be perceptible; there is great thirst, and not unfrequently a very tolerable appetite till the fatal termination; there is sometimes vomiting, and more frequently diarrhœa; sometimes hæmorrhages, œdema of the extremities, and rigors; and finally sopor and an irregular rattling respiration; and in this stage the patient expires. The more severe symptoms of fever and the manifest collapse only occur shortly before death. The patient is utterly beyond hope unless the gangrene can be arrested before the supervention of the graver general symptoms.

"3. In most cases, and in all those which prove fatal, noma of the mouth is complicated with anatomical lesions of other organs; most commonly with pneumonia, very often with enteritis and softening of the intestines, sometimes with gangrene of the generative organs, and in individual cases with other affections, as gangrene of the lungs, pleuritis, peritonitis, nephritis, meningitis, sub-arachnoid apoplexy, &c. The symptoms of these affections combine with those of gangrene of the mouth, by which they are generally more or less masked to the inattentive observer." (Wunderlich, vol. iii, p. 701-3.)

We regret to say that none of our authors have added much to our knowledge of the treatment of this disease. Wunderlich very truly remarks that everything depends on local treatment, and that the longer it is delayed, so much the more certain is the patient to be lost. The strongest caustics must be freely applied. After enumerating fuming hydrochloric acid, sulphuric acid, chloride of antimony, and chloride of zinc, he suggests that probably the hot iron is the most to be depended on. By way of subsequent treatment, decoction of cinchona, aromatic wine, camphor, or other substances of a stimulating nature, may be locally applied.

The tongue next claims our attention. Its most common serious affection is glossitis, a disease noticed by Hippocrates, Aretæus, and Galen; and even this disease is of comparatively rare occurrence. As few systematic British authors have alluded to this disease, we shall enter into its consideration at some length.

Amongst the predisposing causes, we find that compression of the jugular veins has been recorded, Stoll affirming that he has often seen it induced in soldiers by the tightness and stiffness of their collars. Cases are recorded by J. Frank, Hosack, and others, tending to show that

those who have at some previous time (perhaps two or three years) submitted to a mercurial treatment, are especially liable to this affection. Our knowledge of the occasional causes is much more precise than that of the above-named predisposing causes. First and foremost we must place wounds of the tongue, and especially lacerations and contusions, such, for instance, as are sometimes caused by the teeth in mastication, or in sudden and unexpected approximation of the jaws. It is, however, worthy of observation, that the wounds to which the tongue is subjected in epileptic attacks are seldom followed by intense inflammation. Burns are mentioned by several authors as frequently the cause of glossitis, but the inflammation they excite is usually comparatively superficial, and we believe that the cases of severe glossitis from this cause are very rare. A well-marked case is, however, recorded by J. Frank. Cases have been described in which severe inflammation has been induced by the application of caustics to the tongue; usually, however, the effect of these agents does not extend far beyond the spot at which they are applied. The most frequent cause with which we are acquainted is the action of acrid or acro-narcotic substances on the tongue. Such effects have been produced by the juice of the daphne mezereum, by tobacco leaves, and by the stings of wasps, bees, and other insects. J. Frank relates the case of a woman who was stung in the foot (he does not mention by what insect), and feeling the pain very intense, repeatedly applied her saliva with her fingers to the injured part. The poison was thus conveyed to the tongue, and an attack of glossitis was the result. Dupont relates the case of a young man who, for a wager, took two bites at a toad, and was speedily attacked with very severe glossitis. We think he quite deserved it. That toads had a very bad character for producing this disease, is obvious from a case recorded by Ambrose Paré,\* in which the use of an infusion of unwashed sage-leaves was followed by glossitis. A toad, he observed, had most probably touched the leaves!

We must not omit to mention that inflammation of the tongue sometimes occurs as a secondary affection in febrile diseases, and especially in variola.

The above remarks apply to parenchymatous glossitis, or to that form of the disease in which the deep tissues of the organ are involved.

Generally speaking, superficial glossitis is an affection of little importance; there is, however, a peculiar form of it which is often extremely difficult to cure, that, namely to which M. Requin has given the name of papillary glossitis.

The follicles at the base of the tongue become inflamed, and the papillæ are red, hard, prominent, and feel hot and pricking, a sensation which becomes almost insupportable when any pungent food is taken. This form of the disease is almost entirely confined to nervous, hysterical women.

For the following remarks on the morbid anatomy and symptoms of glossitis, we are chiefly indebted to Wunderlich. The disease, in the acute form, usually attacks the whole tissue of the tongue, or the greater part of it. The local disturbance proceeds with rapidity, and usually without premonitory symptoms, shortly after the action of the exciting cause, when any such cause can be recognised. Occasionally, however,

\* Œuvres Complètes, liv. xxi, chap. 31.

a general feeling of *malaise* or an attack of rigors ushers in the disease. There is a sensation of extreme heat on the tongue; any movement of it gives rise to pain; it swells, and soon attains a very considerable size, often becoming so enormous in a few days as to project for an inch or more beyond the lips, in the form of a livid or almost black mass. Speech soon becomes difficult, and at length impossible; and mastication and deglutition are similarly affected. Symptoms of more or less marked cerebral hyperæmia ensue, the face becomes dusky, red, or livid, and the eyes are injected; respiration is impeded and becomes laborious, and there are severe fits of dyspnoea, almost causing suffocation. When these latter symptoms occur there is great danger, such cases often terminating fatally from the swelling extending to the epiglottis and glottis. Inflammation of the tongue is usually accompanied with a certain degree of salivation; this is, however, sometimes only apparent, depending on the escape of the saliva from the lips, because its ordinary course to the œsophagus is impeded. The febrile symptoms in acute glossitis are not so much connected with great rapidity of the pulse and heat of the surface of the body, as with great debility, sleeplessness and restlessness, and disturbance of the cerebral functions. The inflammation terminates either in resolution with a gradual diminution of the swelling, and of the symptoms to which it gave rise; or in chronic induration (chronic glossitis); or in supuration; in the last case abscesses, containing fetid pus, are formed at the base of the tongue; after they are opened all the symptoms speedily yield, the tongue soon regains its natural size, and often there is not even a cicatrix left; or, finally, in gangrene. This last-named termination is fortunately very rare.

Chronic inflammation of the tongue is usually limited to one or more small spots of the size of a pea, or, at most, of a cherry. It may occur as the remains of an attack of acute glossitis, or it may be gradually developed from the irritation excited by a sharp angle of a tooth, a bit of rough tartar, &c. The disease usually occurs near the edge of the tongue, showing itself as a tolerably hard tumour, scarcely or not at all projecting above the surrounding surface, and only perceptible on pressure. There is usually no change of colour, but the mucous membrane covering it has a tense appearance. There is seldom acute pain, usually only a feeling of constriction, and of tenderness when the tongue is moved or the affected part touched, or a sensation as if a foreign body were on the affected part. There is often some difficulty in speaking and masticating. This induration may last for years, and may then either terminate by resolution, or by the formation of a small abscess.

With regard to treatment, Wunderlich observes, that when the symptoms come on with great severity, immediate recourse must be had to venesection, which often requires to be repeated two or three times. Local abstraction of blood by scarification of the tongue is often of much service, and may be repeated with advantage. In a case apparently of idiopathic glossitis, recorded in the 'Monthly Journal' of last year by Dr. Selkirk, almost immediate relief followed local scarification, two incisions being made parallel to the raphe. The introduction of ice into the mouth gives relief; all warm mouth-washes and injections must be avoided. As long as the patient can swallow he must occasionally take a laxative, and afterwards cooling enemata must be administered. The lowest diet must be prescribed;

but when after some days' constrained abstemiousness, the patient can again swallow, broths, and such-like fluid forms of nourishment, may be pretty freely given. When the symptoms of impending suffocation are very intense, it may be necessary to have recourse to tracheotomy. As soon as it is ascertained that an abscess is formed, a free passage must be at once formed for the pus.

In the chronic form of the disease we must rely on gentle, warm applications; warm mucilaginous mouth-washes, the continued use of mild laxatives, and the local and internal application of iodine. When there is great induration, superficial cauterization is sometimes of service.

We were surprised that, in the treatment of the acute form, we found no reference to emetics, which were highly commended in this affection by Dupont, Raggi, Wittengius, &c.; we presume that, in the somewhat congested condition of the brain which may be present in this disease, he deems them dangerous (see the remark on this subject in p. 48). Moreover, cod-liver oil deserves a place among the remedies for chronic induration of the tongue.

Diseases of the tonsils now claim our attention. Amygdalitis is so common and well-known a disease, that our remarks upon it will be very brief. It is generally allowed that, although this affection may occur at any age, it is most common during the period of adult life. This is demonstrated numerically by Louis, who found that, in forty-one cases that fell under his care at "La Charité," three only exceeded the age of forty years; and that the mean age of the others was twenty-five years and a half. Wunderlich remarks, that he has several times seen the disease induced by violent fits of sneezing, which may probably give rise to small hæmorrhages in the parenchymatous tissue of the tonsils.

The following is a summary of his views on the treatment of acute inflammation of the tonsils. General venesection seems (as shown by Louis) to lessen the intensity and slightly to shorten the duration of the affection. The mean duration of Bouillaud's cases, treated with "saignées coup sur coup," was eight days.

The application of leeches to the angle of the jaw is even of less service, apparently exerting no influence on the extent of the swelling, on the pain, or on the duration of the disease. Scarification of the tonsils is a far more useful measure; but it is a process to which patients usually offer strong objections, and which sometimes gives rise to troublesome hæmorrhage. There is probably no treatment, under which the swelling disappears so rapidly, or which is so likely to prevent suppuration, if it is adopted at an early period.

Next in importance to the abstraction of blood, he places strong purgatives. "Tartar-emetic, which was much praised by Stoll, appears," he observes, "frequently to overcome this affection rapidly. It is, however, a highly dangerous medicine; because, if there is much cerebral congestion, it is apt to be increased by the action of this drug." The results of our own practice tend to show, that, while emetics are most serviceable in the earliest stage of the affection, the cases must be very rare in which the danger, so frequently noticed by Wunderlich, is likely to occur. He alludes to Velpeau's mode of treatment, by applying powdered alum to the tonsils, without offering any opinion on its merits. Valleix, we may observe, thinks highly of it. Amongst other forms of the so-called

abortive treatment of amygdalitis, Wunderlich notices Tinct. Pimpernell. alb., recommended by Günther, in doses of 20—60 drops, on a lump of sugar, which is to melt gently in the mouth, as an almost infallible means of checking the inflammation; guaiacum powder, as recommended, in half-drachm doses, every four or six hours, by Mr. Bell of Barhead, and others; borax (to be applied in the same manner as alum, and probably acting in much the same manner—an ancient Chinese remedy in these cases); the various preparations of ammonia (the muriate in the form of gargles, the acetate internally, and the alkali externally); and pyrothionide, (which is nothing more than the oil yielded by the combustion of paper, rags, &c., and then purified and evaporated to dryness. It was extravagantly lauded by M. Ranque some years ago, who prescribed it in the following form: Pyrothionide, gr. iss.; cold barley-water, ʒj; honey, ʒij. Mix. Ten or twelve such gargles to be used during the day.) He concludes with the general directions, that the neck should be properly guarded from cold; that speaking should be prohibited; that little food should be taken, and that little of the mildest nature; that gargling should be prohibited, if it causes pain; and that the temperature should be properly regulated, which is best effected by keeping the patient to his bed.

When our early treatment has failed in reducing the size of the tonsils, or the disease first comes under our notice in an advanced stage, the treatment must be somewhat modified. In plethoric persons, and where there is much fever, Wunderlich considers that general bloodletting may still be had recourse to advantageously, that the enlarged glands may be occasionally scarified, and that cataplasms may be applied to the neck, emollient and narcotic vapours to the throat, and sinapisms to the legs. He regards the use of mercurial ointment in such cases as very doubtful. The bowels must be kept well open, the diet must be very low, and all attempts to speak or in any way to exert the adjacent parts must be prohibited. Finally, when the symptoms indicate that suppuration has taken place, the abscess must be opened, unless the effort of vomiting produced by an emetic succeed in breaking it.

We now turn to our author's remarks on chronic enlargement of the tonsils—a class of cases frequent in practice and often extremely obstinate. Although we find no new suggestions regarding the treatment of this affection, we meet with the following observation, to which we would most seriously call the attention of those of our readers who have an operative tendency. After remarking that extirpation of the tonsils should never be had recourse to rashly, or without the fullest consideration, he proceeds to state that:

“This operation has often produced a result directly opposite to that which was intended. It often leaves the parts in a state of irritability and sensitiveness which exceeds, beyond comparison, that which is produced by the chronic amygdalitis. The power of the voice is sometimes for ever lost by the operation, which not unfrequently leaves behind it a sensibility of the Eustachian tube, and even of the respiratory organs, and a tendency to catarrh of these parts, to which the patient was previously a stranger, which is in the highest degree annoying to him, and draws from him bitter lamentation over the worse than useless treatment to which he has been subjected.” (Wunderlich, vol. iii, p. 719.)

In a case of enlarged tonsils, occurring in a somewhat scrofulous lady about thirty years of age, to whom the late Dr. James Johnstone had pre-

scribed iodide of potassium internally, and tincture of iodine locally, without the slightest effect upon those glands, and who was strongly advised by Mr. Liston to have them at once removed, we witnessed an almost perfect cure by the administration of a scruple of ipecacuanha every second or third morning before breakfast. Decided benefit was perceptible in a fortnight, but the remedy was persisted in for upwards of two months.

There is nothing in the section on the anatomical changes in the salivary glands to call for remark. In the section on diseases of the jaw, there is an excellent summary of all that is at present known regarding the deleterious effect of phosphorus fumes; as, however, this subject was fully discussed in the first volume of this Journal, we will merely make the single observation, that the *Tinctura mezerei* (one drop three times a day) was found serviceable in a case alluded to by Wunderlich.

We now proceed to the general consideration of the diseases of the digestive canal.

Wunderlich, to whom we shall chiefly refer, and whose order of arrangement we shall almost invariably adopt, commences every fresh department with a few anatomical and physiological prolegomena. For a text-book, or for medical lectures, we regard this as an excellent arrangement, as without a clear conception of the relative positions of organs, and (in many cases) of the nerves and vessels with which different parts are supplied, it is needless to observe that neither student nor practitioner can duly interpret symptoms, nor can his practice be anything more than empiricism; and we fear that most teachers of medicine can confirm our sad experience of the small amount of anatomical and physiological knowledge possessed by the majority of medical students when commencing the study of the practice of physic. As Wunderlich is one of the most prominent of the so-called German school of "rational or physiological medicine," we think he has done well in the introduction of this feature into his work. Although it will not be compatible with the objects of this article to enter at any length into the consideration of any physiological questions, we may give our readers some little idea of the leading points of these prolegomena.

The relative position of each organ, in reference to the surrounding parts, its dimensions, the character of its epithelium, if it has any, the nerves and vessels supplying it, and its functions, are in most cases briefly noticed. A condensed review of his observations on the stomach (which extend over nearly six pages), will afford a fair illustration of the nature of his physiological remarks. Its position and size in infants, and the gradual modifications in these respects which it undergoes until the period of adult life, are first noticed; and the effects which the peculiar shape of the stomach in early life have in facilitating vomiting, are pointed out. The length of the stomach in adults is estimated by our author at about a foot, while its breadth and volume do not admit of being accurately determined, partly in consequence of the great individual differences that present themselves to our notice, and partly in consequence of the great extensibility of the walls of the stomach. He thinks, however, that the greatest breadth of the organ in its flaccid state is somewhat between three and a half and five inches, and that its volume, when moderately full, may be estimated at from three to six quarts. He observes, that in persons who from any cause

frequently or habitually vomit, the process becomes as easy as in early infancy, partly in consequence of the gradual dilatation of the cardia, by which the *saccus cæcus* has more of a funnel-like opening into the œsophagus, and partly because in certain lingering affections of the stomach, as, for instance, in cancer of the great curvature, the *saccus cæcus* gradually diminishes, and the stomach approximates to the form it possessed in early childhood. The position of the different parts of the stomach, both in its full and empty state, is given in considerable detail, because, as our author observes, "all these topical relations are of especial importance in a medical point of view, when either the stomach or a neighbouring organ undergoes an extraordinary enlargement of volume. In such a case, remote organs—as, for instance, the kidneys or the uterus—may be brought into topical relation with the stomach." We cannot follow him through his sketch of the metamorphoses the food undergoes in the stomach, and his description of the amount of resorption accomplished by that organ, nor through his notice of the gastric mucous membrane and muscular coat. "Rumination," he observes, "is in many cases noticed in persons who, in all other respects, are perfectly well, and who sometimes attain facility in the process by continual practice. According to Dr. Arnold (*über den Bau des Hirns*. 211), in three men who, during life, were in the habit of ruminating, there was found a saccular dilatation above the cardia, in fact just above the passage of the œsophagus through its diaphragmatic opening; and I have myself seen a preparation of this nature. That this peculiar state of the parts facilitates rumination is unquestionable, but whether the power of ruminating always accompanies this peculiar formation is a question to be determined by further experience."\*

The vascular arrangements of the stomach are fully described, and their connexion with the various forms of gastric hyperæmia clearly indicated. There is nothing to detain us in his physiological remarks on the small intestines. Of the cæcum he observes, that its anatomical relations predispose it to disease. The loose cellular tissue by which it is supported easily becomes the seat of inflammatory infiltration, and the angular fold of serous investment consequent on its short and imperfect mesentery readily becomes the seat of exudations. Moreover, the width of the cæcum, and the length of time during which it naturally retains its contents, give frequent occasion to an abnormal stoppage, and to excessive distension; to inflammations and ulcerations of the mucous coat. We think that our author is a little too practical, and exhibits an ignorance of philosophical generalization, in the following quotation on the comparative advantages and disadvantages of possessing an appendix vermiformis.

"The appendix vermiformis is of no physiological importance. Instead of being an advantage, it brings with it many disadvantages and dangers. Foreign bodies, small pieces of excrement, or aggregated crystals, may get entangled, and become fixed in it, and thus give rise to fatal inflammations and ulcerations. In typhus, and in tuberculosis, it is frequently attacked; and in consequence of the difficulty which is opposed to the escape of the morbid products from it, a fatal catastrophe not uncommonly ensues from this useless little part." (*Wunderlich*, vol. iii, p. 757.)

The size and direction of the colon, its local relations to other organs,

\* We do not make this extract for its novelty, for the opinion that there is a connexion between rumination and the œsophageal pouches occasionally met with, is one of considerable antiquity, but because, in cases of this nature, every authenticated fact is of value.—*Rev.*



and the varying character of its connexion with the peritoneum in the different parts of its course, are duly pointed out :—

“In the mode of attachment of the colon, and in the length of its mesentery, there are (he observes) frequent differences. Amongst the most striking are those in the transverse portion, which often has so long and lax a mesentery, as to permit this part of the bowel to descend into the lower portion of the abdominal cavity, and to form a horseshoe-like curvature. Although this peculiarity is sometimes observed in perfectly healthy individuals, it is far more frequent in cases of imbecility and melancholy, and has consequently been sometimes regarded as a cause of mental disease. The portion of intestine hanging in this way on a loose mesocolon, may sometimes, if it is filled with masses of *feces*, exert a dangerous pressure on other parts of the intestine. The deviations in the mode of attachment of the ascending and descending portion of the colon, are of little practical importance; but in the sigmoid flexure, a too prolonged mesenteric attachment, and a too free motion, may prove dangerous, by permitting the portion of gut to sink into the lower part of the pelvis. In a fatal case of typhus, accompanied with obstinate constipation, I found a tolerably long loop of the sigmoid flexure incarcerated with a double knot in the pelvis. The rectum, and a short piece of the lower portion of the colon, were empty. From the incarceration, which was situated about three finger-breadths above the upper limit of the rectum, the colon was thoroughly plugged, as far as the *cæcum*, with thick (and in the *cæcal* region, with pultaceous) *fecal* masses to the amount of nearly five quarts. Their progress along the intestine was impeded by the two incarcerations, and delicate recent pseudomembranes were formed on the adjacent serous surfaces of the depressed portion of the colon. The patient at the commencement of the typhus, and before he came under my care, had taken a powerful purgative, and his constipation dated from the action of that medicine.” (pp. 756-7.)

In paralysis of the circular fibres of the colon, Wunderlich observes, that he has seen some parts of this division of the intestine attain to so great a size, as to be half a foot in diameter.

The character of the mucous membrane, and of its glands, as well as of the *fecal* and gaseous contents of the colon, is then described, and the remarks conclude with a few words on its blood-vessels, lymphatics, and nerves.

In the three pages devoted to the anatomy and physiology of the rectum and anus, we have an excellent description of the anatomy of the rectum, and of its relations to the adjacent parts in both sexes. We extract the following remarks bearing on the process of defecation :

“The muscular layer of the rectum is thicker than that of the superior parts of the gut; the three bands of longitudinal fibres which we observe in the colon here unite, and longitudinal fibres surround the whole canal. The circular fibres become stronger towards the extremity of the canal, and finally form a strong muscular ring three or four lines broad, the sphincter ani internus, which in the ordinary state keeps the rectum closed. These muscles of the rectum are capable of bearing a considerable pressure, and they appear to contract independently of volition; they are only brought into action by impressions received by the mucous membrane; they are, however, not independent of the cerebro-spinal system; indeed, it seems that the will, or at least the instinct, is not altogether without influence on its action or suspension. Moreover, the lower part of the rectum and the anus possess two muscles perfectly subject to volition, one, the levator ani, which supports the lower part of the rectum, and the other, an elongated muscle, surrounding and closing the external orifice, the sphincter ani externus. They differ from the other muscular fibres of the rectum and intestine generally, in the latter consisting of non-striated fibres (organic muscles), while these present the striated primitive fibres of voluntary (or animal) muscle. In its state of rest, and when defecation is not going to

take place, the rectum, in its normal condition, is empty and contracted, and is so perfectly closed by the sphincter internus, that not only its contents, whether solid or fluid, but even gases are perfectly retained. The sphincter externus is a further support, since it is maintained in a state of moderate contraction. When the mucous membrane of the rectum is irritated by the descent of fæces, or by any other cause, the layer of muscles involuntarily contracts. If the sphincters can only present a slight degree of resistance, and at the same time the rectum can contract powerfully, and its contents are loose and easily moveable, they may be expelled by the action of these muscles alone. Ordinarily, however, defecation requires the assistance of voluntary or unconscious (but instinctive) abdominal pressure (contraction of the diaphragm and abdominal muscles). As long as the irritation is not too strong, the orifice may be kept closed by the voluntary contraction of the sphincter externus; and if in such a case the involuntary muscles of the rectum predominate, they can only force the contents of the rectum upwards towards the sigmoid flexure, if they do not finally overcome the resistance of the sphincter. During the expulsion of the fæces, the levator ani draws the lower part of the rectum upwards, and may act in enlarging the opening of the anus. A healthy person has one or two fæcal evacuations in the course of the day, or perhaps only one every second day; in infants the bowels are more frequently, and in aged persons less frequently, moved. The daily amount of fæces passed by a healthy adult varies from fifteen to thirty ounces, but much depends on the quantity and nature of the food.

"The prolonged retention of fæces in the rectum must always be regarded as a not altogether normal condition; and we are indebted to O'Beirne for remarking, that the opinion that fæces are usually found in the rectum is erroneous. In some persons, however, who do not present any other signs of disease, the rectum appears to be a receptacle for the fæces; at least, in applying the vaginal speculum, I have often found such large quantities of accumulated fæces in the rectum, as made the introduction of the speculum somewhat difficult, without there being present any tendency to immediate defecation. Although the muscles of the rectum are in a great measure independent of the influence of the will, we find them paralysed, and consequently incapable of propelling the fæces to the anus, in many diseases of the brain and spinal cord. In these cases we may sometimes observe, that the rectum and descending colon are enormously distended with fæcal matter, and notwithstanding the relaxed condition of the anus, no purgative or clyster can excite the slightest muscular contraction to expel the fæces, which must be mechanically removed, either by the finger, or with the aid of instruments. But a much more frequent affection, in diseases of the brain and spinal cord, is simple paralysis of the sphincter ani, and the consequent loss of power of retaining the contents of the rectum; in these cases, however, it frequently happens, that only a thin fluid escapes, while the more solid masses become impacted in the paralysed and distended sigmoid flexure and descending colon. In local affections of the adjacent mucous membrane (as in prolonged diarrhoeas, blennorrhœas, &c.), the levator ani is frequently reduced to a weak and half-paralysed state, in consequence of which it can no longer efficiently support and retain the rectum during straining efforts at defecation, and protrusion of the gut occurs." (pp. 759-60.)

Although there is nothing to which we can positively object in Wunderlich's description of the process of defecation, we think he might have more distinctly shown the reflex character of the act.

We proceed to his "general consideration of the affections of the digestive canal." In the historical sketch with which the article commences, he points out the obscurity that existed in the minds of the early physicians with reference to this class of cases. Cholera, ileus, and dysentery were the only acute, and colic, lientery and fluxus cæliacus almost the only chronic affections, to which special allusion was made. Amongst those

who led the way to a more correct knowledge of these diseases, he especially mentions Stahl, Boerhaave, Morgagni, Cullen, Brown, and Röderer and Wagler, whose 'Tractus de morbo mucoso' appeared in 1783, and contained many important facts relative to the morbid anatomy of intestinal diseases. The commencement of the present century forms an important epoch in the progress of our knowledge of acute intestinal affections, and of their connexion with certain febrile conditions. It was then that Pinel published his description of 'Febris meningogastrica'; that Prost, in his 'Médecine éclairée par l'observation et l'ouverture des corps,' (1804) demonstrated the association of certain anatomical changes in the intestines with the (so-called) mucous, gastric, ataxic, and adynamic fevers; that Peter Frank described, with more accuracy than any of his predecessors had done, not only gastritis and enteritis, but also the chronic diseases of the intestine; and that Broissais first promulgated his well known opinions. Since their time, the investigations of Billard, Andral, Gendrin, Louis, Bretonneau, Rigotard, Trousseau, and Cruveilhier, in France; of Pommer, Lesser, and Rokitansky, in Germany; of Pemberton, Howship, Armstrong, Abercrombie (the appearance of whose work constituted a positive epoch), Annesley, Munro, Parker, Prout, Chapman,\* and Alderson, in Great Britain, have contributed vastly to our knowledge of the diseases of the intestinal canal generally. If we except Folchi's 'Exercitationes Pathologicae,' the Italian school has aided but little in this inquiry.

We shall now endeavour to give our readers some idea of his observations on the etiology of these affections. He considers:

- A. Congenital defects and morbid tendencies of the intestinal canal.
- B. Tendencies connected with different ages.
- C. External noxious influences.
- D. Causes depending on accidental conditions of the digestive canal itself.
- E. Influence of other organs on the production of intestinal diseases.

Under the first head he notices the frequency of errors in form, and the extreme rarity of textural changes in the intestinal canal during foetal life; the original and congenital tendency to certain forms of disease, as shown in the common occurrence of that fatal affection, softening of the stomach, in the children of some families, hereditariness of ulceration of the stomach, cancer, hæmatemesia, tapeworm, and other affections; and finally, the unequal tendency to disease displayed by different parts of the canal; thus, while morbid changes are rare in the œsophagus, jejunum, and transverse arch of the colon, they are very frequent in the stomach, in the neighbourhood of the ileo-cæcal valve, and in the rectum.

Under the second head he observes that besides the slighter affections which scarcely any infant escapes, more severe disorders, especially follicular enteritis and colitis, cholera, and softening of the stomach and intestines, are by no means rare during the early period of life, although, from the rapidity with which they carry off their victims, they do not attain to a high degree of local pathological development. After noticing the most common intestinal affections during the periods of childhood

\* Chapman is, in reality, an American physician. As, however, with the exception of Bush, who has written the best work on the 'Diseases of the Rectum' existing in our language, no other American has contributed any important materials to this inquiry, we have deemed it unnecessary to remove him from the good company in which Wunderlich has inadvertently placed him.—*Rev.*

and puberty, and especially referring to the frequency of "the simple ulcer of the stomach" in the early period of adult life, he points out the special tendency that exists in declining life to cardialgia, chronic gastric catarrh, hæmorrhoids, and, in a lesser degree, to acquired errors in position, to ulcer of the stomach, to cancer of the stomach and rectum, and partial paralysis of the intestinal canal.

Under the third head—the external noxious influences—he alludes, in the first place, to those of a mechanical nature. They may act externally, as penetrating wounds, concussions, &c., in which case they fall within the domain of surgery, or they may enter the body either by the mouth or the rectum, and excite diseased action in the intestinal tube. The points at which foreign bodies are most likely to be arrested, are the œsophagus, the stomach (if they are too large to pass the pylorus), the ileo-cæcal valve, the appendix vermiformis or some other diverticulum, occasionally the cæcum and ascending colon (especially after swallowing fruit-stones), the rectum, and more particularly the neighbourhood of the anus, where pointed bodies are often detained and excite inflammation, after having passed without injury throughout the whole of the upper part of the canal. Substances possessing no noxious character, may, if swallowed in inordinate quantity, give rise to very dangerous symptoms. Our author observes that even water, taken in enormous quantity (in the so-called water cure), may, by its mechanical action, give rise to fatal consequences. Most of our readers will probably call to mind, in connexion with these mechanical influences, the singular plugging of the rectum, induced by the use of bad potatoes during the Irish famine.

The intensity of the local disturbance in these cases depends very much on the part in which the body is detained, although, of course, considerably on the nature of the offending substance itself. If wedged in the œsophagus, it gives rise to symptoms of impending suffocation; if detained in the stomach, merely in consequence of its bulk, it only gives rise to chronic irritation, with no very characteristic symptoms; if, however, it is sharp or acrid, it may excite hæmatemesis, inflammation, and suppuration; and finally even produce perforation of the walls. Foreign bodies are most fatal when they are entangled in the appendix vermiformis, or in some other intestinal diverticulum. Detained in the rectum, they frequently cause abscess, and give rise to fistula.

There is another kind of mechanical action to which our author alludes, and in which the whole body is affected, although the intestinal symptoms are the most prominent. Thus, in any unaccustomed form of motion, in swinging, rocking, and especially in a boat or ship on a rough sea, we frequently experience nausea and vomiting, great irritability of the stomach, and (in sea-sickness) often profuse diarrhœa. Our knowledge regarding the nature of the influence of passive motion on the intestines, and the manner in which such phenomena are induced, is still far from satisfactory. We know, however, on the other hand, that absence of bodily motion is unfavorable to the healthy condition of the intestines. A sedentary mode of life mechanically induces a stasis in the vessels of the rectum; and bed-ridden persons most commonly not only lose their appetite, but have a furred tongue and an impaired digestion.

Noxious influences of a chemical nature, and such as depend on the quality of the food, are then considered.

Although these latter causes are undoubtedly of a chemical nature, we are unable to indicate the actual chemical change which occurs. These influences commonly exert a direct influence on the intestinal canal; but even in those instances where our knowledge is most complete, as in the case of corrosive acids and poisonous metallic salts, much still remains to be elucidated, as, for instance, why some parts of the intestinal are specially acted on by certain poisons. The action of the corrosive acid is usually limited to the œsophagus and stomach, where they destroy the tissues in various ways, and consequently give rise to a more or less intense process of exudation; while the metallic poisons act less on the œsophagus than on the stomach and intestines beyond it; and are attended by ecchymoses, hyperæmia, plastic, purulent, or copious aqueous exudations, follicular ulcerations, erosions, and more or less destruction of tissue. Although these changes are in a great measure dependent on the production of chemical combinations, formed by the metal with the albumen and the other constituents of the blood and of the circulating fluid, and on the consequences arising from the stoppage of the circulation in the capillaries and small vessels, there is much in connexion with them which still remains unknown to us; and our ignorance is even greater regarding the chemical action of certain vegetable and animal substances, which excite hyperæmia, exudations, or even destruction of tissues, and to which we commonly apply the terms *acria*, *irritantia*, &c. There is no doubt that, in some of the cases pertaining to this class, as, for instance, in that of alcohol, a chemical combination ensues between the injected substance and some of the constituents of the tissue; but there are many instances in which it is impossible to obtain any experimental evidence of the nature of the ensuing action.

Substances taken as food sometimes exert a directly injurious action on the intestinal canal. If the use of insufficiently soluble food, or if, on the other hand, food of very easy solution in the gastric juice be persisted in for a prolonged period, the digestive powers are likely to suffer. A single error in diet (if a serious one) may interfere with the proper secretion of healthy gastric juice, and suffice to cause serious derangement of the stomach or intestines, giving rise to diarrhœa, or even some more severe affection; while a frequent repetition of such errors may lay the foundation of far greater evils. The use, or rather the abuse of condiments, and such articles of food as stimulate the gastric glands to increased action, is noticed by our author as a frequent cause of a disordered state of the intestinal canal. The excessive use of a stimulant capable of increasing the secretion of the gastric juice, may either excite acute inflammation of the stomach, or, which is more frequently the case, repeated excesses may give rise to chronic inflammation. Many articles of food exert a deleterious influence, altogether independent of their property of being easily or difficultly digested. We may mention the dangers resulting from the use of certain cold, watery fruits, as, for instance, cucumbers, water-melons, &c., as cases in point; and the injurious effects which the waters of many localities exert for a time on strangers.

Our author's section, 'On External Influences as Causes of Intestinal Disease,' terminates with a few remarks on the atmosphere, and especially on temperature, in reference to this class of diseases.

His fourth class of causes embraces those which depend on incidental conditions of the canal itself. He notices—

*a. An interruption of the functions of the intestinal canal*, either from an entire deficiency of food, or from an impediment in the course of the canal. The following are the most common points at which such stoppages occur:—

1. The œsophagus; if the stricture is complete, or nearly so, the consequences are the same as if no food were taken, and the food which the patient attempts to take is either rejected, or collects in a pouch of the œsophagus above the impediment.

2. The pylorus; when the occlusion is complete, which, however, rarely is the case, the consequences are identical with those arising from the entire absence of food, except that the stomach becomes extraordinarily distended, and everything which is taken is returned by vomiting.

3. The small intestine, especially in cases of incarceration. Death usually ensues so rapidly, from the inflammation arising from the constriction of the vessels, that the phenomena depending on the non-performance of the digestive functions are in this case very secondary. The large intestine is, however, found empty and contracted.

4. The large intestine, and especially the cæcum, descending colon, and rectum. In this case the most prominent symptoms are the distension of the gut above the impediment, and the obstinate constipation.

*β. An imperfect discharge of the functions of the intestinal canal.* Under this head he notices the bad effects of too sparing a diet; the danger of taking any quantity of food after prolonged abstinence; and the injurious effects that may arise from the retardation of the fæces in the intestine.

*γ. An irregular discharge of the functions of the intestinal canal.* Thus in young children, and in many persons of weakly constitution, constipation, diarrhœa, and other intestinal disturbances may arise from want of regularity in meal-times, or from slight deviations from the ordinary diet.

*δ. An excessive discharge of the functions of the stomach and small intestines*, through the too-frequent or too-abundant taking of food, or of the large intestines through the too-frequent use of purgatives and clysters, is productive of evil results; in the former case we have excessive distension and diseases of the stomach and duodenum, an abnormally great appetite, cardialgia, and chronic inflammation; in the latter, paralysis of the large intestines, or blenorrhœa and chronic inflammation of its mucous membrane.

*e. Anomalies in the position and dimensions of portions of the intestine.* Under this head are considered hernias, invaginations, abnormal dilations, &c.

2. *The progress of morbid processes from the originally diseased spot*, is often noticed in diseases of the intestinal canal. The course of this progress may be either superficial, extending along the same surface; or it may be deep, extending from one tissue to another. In the mucous and muscular layers it extends superficially; indeed, whenever any one spot of the mucous membrane is influenced by any noxious agent, the adjacent parts are always more or less affected, and the morbid process may gradually extend over the whole intestinal tract. The laws influencing and regulating this process of extension are as unknown to us, as are

those connecting certain general diseases with morbid conditions of certain portions of the intestinal canal; as for example in typhus and the solitary and Peyerian glands. The morbid process may extend either upwards or downwards from the spot originally affected; when it extends upwards its progress is usually slower than when it takes the opposite direction, except, however, in the case of the stomach, which, apparently from its peculiar predisposition to disease, often at once sympathises with the originally affected spot, while the intervening portion remains unacted on.

This upward extension is witnessed in cases of diarrhœa, in various inflammatory affections, &c.

The progress of morbid processes along the muscular layer is less rapid and striking than that along the mucous layer. Paralysis commencing at a certain point in the intestine, is often slow in extending along either the upward or downward course. Peristaltic and anti-peristaltic motion may extend from a given point along a greater or less extent of intestine, so that from some disturbing influence in the stomach we may have diarrhœa, and on the other hand, from obstinate vomiting we observe the successive evacuation of the contents of the stomach, the duodenum, the small intestines, and, in some very rare cases, of the large intestines.

The progress of disease from tissue to tissue—in a direction perpendicular to the superficies—is usually slower than that which we have been hitherto considering. It is observed, to a greater or less extent, in all cases of induration, softening, ulceration, cancerous infiltration, &c.

There can be little doubt that the secretions from a diseased portion of the intestinal canal exert a noxious influence on the surfaces with which they afterwards came in contact. Thus gastric hemorrhage may secondarily influence the small or large intestine; and typhous or tubercular affections of the small intestine, or cancer of the stomach, may excite the large intestine, and give rise to diarrhœa. It is by no means improbable that the pus from the lungs, in the last stage of phthisis, may increase the diarrhœa of those patients who are in the habit of swallowing their expectoration.

In his fifth and last class, he places the influence of other organs on diseases of the intestinal canal. He subdivides this class in the following manner:

*α.* Mechanical relations of adjacent parts,—their augmentation and diminution of volume, their unyieldingness, &c.,—may, by pressure, tension, &c., act injuriously on the intestines; for instance, the pylorus and duodenum may be compressed, and their caliber diminished, by the left lobe of the liver, the rectum by pelvic tumours, &c. &c.

*β.* Affections of remote organs may interfere with the due circulation of the blood through the intestines. If either the general or the portal circulation be affected, catarrhal, inflammatory, or hemorrhagic processes are likely to be established in the stomach and intestines.

*γ.* The local extension of morbid processes from neighbouring parts to the intestine is by no means rare.

Thus the œsophagus may become affected in its upper part by diseases of the mouth, throat, or larynx, and in its thoracic part by diseases of the great vessels (the aorta, and its immediate branches), the pleura, or the bronchial glands.

The stomach, and, indeed, the whole of the intestines lying below the diaphragm, are affected by diseases of the peritoneum. The former viscus

is also liable to be affected by diseases of the liver, spleen, and pancreas, and by aneurism of the descending aorta.

The duodenum is extremely liable to be involved in hepatic and pancreatic diseases.

Affections of the right *M. iliacus internus*, and its cellular tissue, and diseases of the pelvis and its contents, often extend to the cæcum; the right kidney and the liver may involve the ascending colon; the transverse colon may be implicated in diseases of the liver, spleen, and even of the thoracic organs; or, if it lies very low, in diseases of the pelvic organs, the descending colon in those of the spleen and left kidney; and the sigmoid flexure in those of the left *M. iliacus* and its cellular tissue, the female generative organs, and the bones of the pelvis.

The rectum is influenced by diseases of the bladder, the female generative organs, the seminal vesicles, and the prostate gland; and these diseases very frequently extend to and implicate it. And, finally, the anus is often affected by diseased conditions of the adjacent external parts.

δ. The influence of the nervous centres—the brain, spinal cord, and sympathetic system—on the intestinal canal, is universally recognised. There is nothing new in our author's remarks, either in the way of arrangement, or of facts.

ε. Diseases of the intestinal canal, arising from the influence of the blood, and the general relations of the constitution, are then considered.

An increase in the whole mass of the blood, or a relative augmentation of the blood-corpuscles, is liable to excite intestinal hyperæmia, and a distended condition of the veins of the rectum, and, as a secondary condition, hemorrhage or catarrh of the stomach and rectum.

A diminution of the mass of the blood, or of its solid contents—the *spanæmia* of Simon—up to a certain point, seem to exert no marked influence on the intestinal canal; beyond that point, it excites a tendency to slow, wasting catarrhs, and to a want of tone in the muscular layer of the intestine; while in its highest degree, it gives rise to attacks of acute catarrh, to hemorrhages, &c.

An increase in the amount of fibrin in the blood, by *hyperinosis*, is the change which, of all others, exerts the least influence on the intestine; whilst a diminution in the amount of that constituent, *hypinosis*, exerts well-marked changes, as severe hyperæmia, occasionally hemorrhage, very abundant secretion, ulceration, softening, probably infiltration of the solitary and augmented glands (typhous deposit); and, in its highest degree, causes paralysis of the muscular coats.

In *cholæmia* (when the constituents of the bile are retained in, or absorbed by, the blood), we have, besides a tendency to catarrh, a torpidity of the peristaltic action, and, consequently, constipation; in intense cases there are severe hyperæmia (often followed by mortification), and profuse exudations.

In *uræmia* (or retention of the urinary constituents in the blood) the coats of the stomach are almost always affected; and there are observed cardialgia, vomiting, profuse diarrhoea, catarrh, hemorrhage, inflammation and mortification, according to the degree and intensity of the urinary retention.

In *pyæmia* (or purulent infection of the blood), the stomach and intestines are almost always much affected; and these cases frequently ter-



minate in softening of the stomach. When this affection is very rapidly and intensely developed, as, for instance, in what is called *putrid infection*, there are observed very severe catarrh, the effusion of corrosively acid fluids into the stomach, obstinate vomiting, abundant and very fetid dejections, ulceration, and gangrene of the mucous membrane, and paralysis of the intestine.

In chronic poisoning by alcohol, it is impossible to decide to what extent the intestinal symptoms depend on the constitutional disease; it is probable, however, that they chiefly depend on the local action of the poison.

Passing over our author's remarks on the effects of the metallic intoxications, and the acute exanthemata on the intestinal canal, we come to his observations on the effects of the dyscrasias on that portion of system.

In scrofulous subjects (he observes), there is an extreme tendency to the development of acid in the stomach, to obstinate catarrh, and to thin watery evacuations. Chronic enteritis, ulcerations, and worms (especially the common, round worm) are of frequent occurrence, and a sub-paralytic weakness of the muscular coat, and a consequent abnormal distension of the intestine, are by no means rare.

In general tuberculosis, besides the frequent deposition of tubercle in the lower portion of the small, and the upper portion of the large intestine, and in the mesenteric glands, there is a tendency to thin discharges, to catarrhs, to submucous suppuration, and to alteration of the mucous membrane of the lower portions of the gut.

In the cancerous cachexia, besides the frequent growth of the morbid tissue in the stomach, the œsophagus, the duodenum, and the rectum, and the occasional deposition in the lower part of the colon, there are intestinal symptoms (at least in the advanced stages) depending on the diseased and non-natural state of the blood, and quite unconnected with the local depositions in the above-named parts.

This section concludes with a few remarks on the influence of arthritis, rachitis, and syphilis on the intestinal canal.

2. In this, his last subdivision, he places those influences which do not find a place in any of the preceding heads. The effects of impeded action of the skin, of severe injuries to the external surface (as, for instance, of extensive burns), and of diseases of the skin, kidneys, and bladder, or the intestinal canal, are briefly noticed; and the section concludes with a few remarks on the sympathies existing between the generative and digestive organs.

Wunderlich's observations on the general pathology of the diseases of the intestinal canal are very brief, and contain nothing worthy of notice.

In our next article we shall give an abstract of his 'Phenomenology' and 'General Therapeutics' of this class of affections, and then proceed to the consideration of some of the most important of the individual diseases of the digestive system.

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## ART. IV.

1. *Essays on Syphilis. Essay I: Syphilitic Sarcocoele.* By JOHN HAMILTON, Surgeon to the Richmond Hospital, Dublin.—*Dublin*, 1849. 8vo, pp. 40. With Two Plates.
2. *The Treatment of Secondary, Constitutional, and Confirmed Syphilis, by a Safe and Successful Method. With numerous Cases, and Clinical Observations, illustrating its Efficacy, and Mode of Application, in the more Obstinate and Complicated Forms of the Disease.* By LANGSTON PARKER, Surgeon to the Queen's Hospital, and Professor of Anatomy and Physiology in Queen's College, Birmingham, &c. &c.—*London*, 1850. 12mo, pp. 112.

It is a remarkable and not altogether creditable fact, that we should still find surgeons who deny the existence of venereal diseases of the testicle,—acute inflammation as a consequence of gonorrhœa alone accepted. Amongst these we are surprised to rank Mr. South, who thus declares himself in his translation of Chelius's surgery.

"Notwithstanding these high authorities, I must confess I have great doubt as to the swelling of the testicle depending upon a syphilitic cause." (South's Translation of Chelius, vol. i, p. 178.)

The remark we have quoted bears the initials J. F. S., and is appended as a sort of commentary to some passages extracted from the works of eminent surgeons who have written upon Syphilitic Diseases of the Testicle.

To all who share in Mr. South's incredulity, we commend Mr. Hamilton's Essay; and not to them only, but to the profession generally, as the best description of the venereal diseases of the testicle with which we are acquainted. Although these diseases have long been recognised, and were spoken of by Benjamin Bell, who described at any rate one form of them very closely, yet our knowledge is indefinite, especially of their exact nature and pathology. Mr. Hamilton speaks of the Simple Syphilitic Sarcocoele, and of the Tubercular Syphilitic Sarcocoele.

The *simple syphilitic sarcocoele* is the less dangerous, and more manageable affection. It occurs among individuals of healthy constitution, as a symptom of the general constitutional syphilitic infection; being generally associated with some form of skin disease, and very often with nodes and periosteal inflammation. It is not a painful disease,—in this respect contrasting in a marked manner with gonorrhœal orchitis; but such pain as does attend its progress has been said to suffer a nocturnal exacerbation. This circumstance, noticed by Sir A. Cooper and Mr. Curling, was never present in any of Mr. Hamilton's cases.

The external characters attending it are thus described by the latter author.

"In the simple syphilitic sarcocoele the testicle will be found enlarged to the size of a lemon or turkey-egg, of an ovoid or pyriform shape, sometimes flattened at the sides; either uniform on the surface, or with the epididymis distinguishable as an irregular ridge along the back; hard, particularly in the situation of the epididymis; heavy, with the integuments of the scrotum of a dusky red; generally neither tender nor painful, except that the hanging weight causes a feeling of uneasiness in the loins and inside of the thighs." (p. 7.)

Very commonly it is associated with effusion into the tunica vaginalis, or sometimes with a circumscribed hydrocele of the chord. It is consequently necessary to be careful in tapping small hydroceles, lest the enlarged testis suffer injury from the trocar. Such an accident has often occurred without any serious injury to the patient. Sir B. Brodie once injected a hydrocele of this description—with the effect of causing numerous abscesses in the testicle—which only got well under the influence of mercury. In this case, as often happens, when the testis was cured the hydrocele disappeared of itself.\*

The simple syphilitic sarcocoele is said to be occasionally met with among children who labour under hereditary symptoms.

As the disease is not a fatal one, we seldom have opportunities of examining these testicles after death. According to Mr. Hamilton, pale yellow lymph is deposited in the tubuli testis, and in the interstitial cellular tissue. This, on the one hand, prevents us from seeing the convoluted arrangement of the tubuli, and, on the other, gives to the section a firm fibrous appearance, by blocking up the mouths of the cut seminal tubes. According to some pathologists, the testicle becomes fibrous, cartilaginous, or even calcareous.†

From an extension of the inflammatory process, adhesion may take place between the serous covering of the testicle and the scrotum, which, by the effusion of water in the tunica vaginalis, occasions circumscribed hydrocele.

As regards the treatment of these cases, Mr. Hamilton is an advocate for the steady and protracted employment of mercury, as he has not found the various preparations of iodine serviceable. The local use of the strong mercurial ointment, or the unguentum iodini diluted with lard, is attended with benefit. Strapping the testicle is of no service.

In a little book we are presently going to notice, our readers will find several striking cases of the disease, which rapidly got well under the employment of the mercurial vapour bath.

The *tubercular syphilitic sarcocoele* is more common, and far more dangerous, than the simple form of which we have spoken. It is not, at first, a painful disease, and the patient may even be unaware of its existence, especially if the body of the gland is unaffected. It may be confined to one, or may affect both the testicles. In general there is not much enlargement, but a striking change in the outline of the organ; which, instead of a smooth, even surface, has an irregular hard, knotty feel, "with points into which the finger sinks, as if it were softened in parts." (Colles on the Venereal Disease.) Sometimes the epididymis alone is involved; and then Mr. Hamilton has found the upper globus the part earliest affected. The testicle is not always irregular in its outline from the first; it may be enlarged and hard, but quite even, until after a little time tubercles form and occasion the knots and irregularities.

"These tubercles are of a yellow colour, of a consistence rather less firm than that of coagulated lymph. Very small at first, they gradually enlarge, and, according to their duration, may vary in size from that of a hemp-seed or split pea to that of a

\* Vide London Medical Gazette, vol. xiii.

† We have lately seen a remarkable specimen of a ram's testicle, which has undergone the calcareous metamorphosis. Each tubulus, in a manner petrified, is as distinct as if it were carved in ivory.

chestnut, or even larger. They have a well-marked cyst, which can, by careful dissection, be separated from the yellow inorganic substance contained in them, and from the glandular substance of the testicle in which they are imbedded: the yellow substance within the cyst has sometimes a laminated arrangement. It is proved to be inorganic, by not receiving any injection when the testicle has been most successfully injected. The bright pink of the one offers a pretty contrast to the yellow colour of the other. In some of the preparations I have examined, the yellow tubercle has gradually so increased as to have caused absorption of the glandular structure of the testicle, and finally to have taken its place.

"When the progress of the yellow syphilitic tubercle is not stayed by treatment after having attained a certain size, it begins to soften in the centre, or at the side nearest the surface of the testicle, where it causes an inequality; adhesion takes place between the tunica vaginalis testis and scroti; and at last these structures, with the tunica albuginea, give way, and the surface of the tubercle becomes adherent to the skin of the scrotum. Suppuration of a slow, indolent character, ensues; the abscess bursts, and discharges very little matter and very thin, and terminates in fistula or lipoma, and total disorganization of the testicle: the same disorganization may ensue without any suppuration. The tubercles are absorbed, but at the same time the glandular structure of the testicle disappears; nothing remains in the place of the atrophied testicle but a hard, irregular, fibro-cartilaginous, contracted mass, in which ossific matter is at times deposited." (pp. 23-5.)

He thus also narrates the constitutional symptoms in this disease:

"The patient is sallow and broken down in appearance, and presents the worst form of secondary symptoms, or those which have been, from the late period of their occurrence, appropriately named tertiary symptoms by M. Ricord. Soft nodes, mostly on the cranium, with caries of the bones;—sore throat, either the large foul ulcer at the back of the pharynx, deep and more or less circular, or that form into which struma largely enters, and which I believe to be of a lupoid character, marked by irregular ulceration at the back of the pharynx, with copious secretion of yellow muco-purulent matter, extending to the arches of the palate, to the soft palate and uvula, and causing more or less destruction of these parts;—the subcutaneous tubercle, sometimes indolent, sometimes ulcerated, with the usual characters of lupoid ulceration;—the annular or serpiginous pustulo-crustaceous eruption, spreading in rings, or some less regular shape, of quickly scabbing superficial ulceration. I have found this eruption and the soft cranial node such constant attendants on the tubercular syphilitic testicle, that whenever a patient with them presents himself, I at once look for disease of the testicles, and often find it, although the patient himself, unaware of its existence, had made no complaint." (pp. 13-14.)

It cannot escape observation, that there is a large element of scrofula entering into this form of tubercular testis. A writer in the 'Dublin Quarterly Journal of Medical Science,' even expresses a wish to exclude the word "syphilitic" altogether from this description, as he has met with a precisely similar affection where there never could have been a syphilitic taint. He refers to the poisonous effect of mercury in bad constitutions, as the true cause of the disease.

Mr. Hamilton's description is illustrated with two excellent coloured lithographs; one represents a tubercular syphilitic testicle, the other a patch of the cutaneous tuberculo-crustaceous eruption, so often associated with it.

The treatment of this disease is much more complicated than that of the last described. A combination of iodine and mercury is the most successful.

"Fifteen grains of the hydriodate of potash are dissolved in a pint of the compound infusion or compound decoction of sarsaparilla; of this the patient takes a

third part in the morning and at mid-day, and ten grains of blue pill at bed-time. In a week or ten days the gums get a little sore, when the blue pill may be reduced to five grains at night. He should go about as usual, and be a good deal in the open air, and the diet should be generous. The dilute iodine ointment may be applied to the testicle." (pp. 31-2.)

We have great pleasure in expressing a very favorable opinion of this little work. It is written in a good spirit, and contains much that is valuable and instructive. It is deficient, however, in one important particular, that is, the question of virility in syphilitic disease of the testicle. Our readers may supply this by consulting the essay of M. Vidal, in the second volume of the 'Memoirs of the Society of Surgery of Paris;' or may refer to the analysis of his paper in the last number of this Review. We add a word as to the form of Mr. Hamilton's publication; because he promises "should the profession receive this essay favorably," to follow it up by others upon the subject of syphilis. We hope, by this expression, he does not mean that it shall have a large remunerative circulation. Isolated essays and pamphlets, particularly on professional topics, seldom sell well; and we should be sorry if this circumstance were to prevent Mr. Hamilton from continuing the series, which he intimates we may expect. We are sure he has much to tell that is worth knowing; and we shall be very glad to meet him again.

The second work, the name of which stands at the head of this article, is from the pen of a writer who is already very favorably known, as the author of an able little work on syphilitic diseases, which has lately reached a second edition. The high estimate we had formed of it, made us very glad to hear of Mr. Parker again, especially as he now comes forward to offer an addition to our means of treating the secondary forms of syphilis; and we are bound to say, that he has impressed us very favorably in behalf of his plan. It were useless here to lament the uncertainty of the best existing means of treating syphilis. In hospital practice it is quite heart-breaking to find the same patient for months remaining on the books, and sent away improved, or even for the time cured, only to return within a short period in a worse plight than before. There can be little doubt that the employment of mercury in the treatment of syphilis is occasionally followed by very dire results, and that some persons are unable to take the smallest quantity of the mineral without great inconvenience and damage. To all such Mr. Parker promises great things. His plan also does not require confinement to the house, or more than ordinary attention to diet on the part of the patient; and in many instances it is unnecessary to combine with it the administration of internal medicines. It never (he avers) injures the most delicate person, whether male or female; and is seldom followed by relapses, and often makes the patients fatter instead of thinner. It is equally applicable to the primary, secondary, and tertiary forms of syphilis, although Mr. Parker seems to have more confidence in its efficacy in the latter stages of the disease. Add to this that, although the full benefit and effects of mercury are induced, the exhibition of it never occasions salivation or ulceration of the mouth, and we think enough has been premised to bespeak an adequate attention to the proposals of our author. At present, having no personal experience of this method of treating syphilitic diseases, all we can do is to place before our readers an

outline of the plan, and the details of one or two cases from Mr. Parker's work. In no long time, however, the method is sure to have been fairly tried by others; and authentic accounts of its real value may then be given to the profession.

The treatment itself mainly consists in the fumigation of the body with mercurial preparations, whilst the patient is in a vapour bath. The salts employed are the bisulphuret, the binoxide, and the black oxide of mercury, and sometimes the iodide. The quantity of the first three mentioned varies from half a drachm to three or four drachms; of the latter, from five grains to half a drachm. It is often useful to combine several of these together, and generally so with the iodide of mercury. Thus, "In affections of the testes (sarcocoele) and of the bones (the various forms of ostitis, or periostitis), a combination of a scruple of the iodide, and one or two drachms of the bisulphuret or binoxide, would be a proper form." The binoxide of mercury is less irritating to the lungs than the others, and is to be preferred when it is desired to get the vapour applied to the head or throat. The exact mode of its application is thus stated:—

"The patient is placed on a chair, and covered with an oilcloth, lined with flannel, which is supported by a proper framework. Under the chair are placed a copper bath, containing water, and a metal plate, on which is put from one to three drachms of the bisulphuret of mercury, or the same quantity of the gray oxide, or the binoxide. Under each of these a spirit-lamp. The patient is thus exposed to the influence of three agents, heated air, common steam, and the vapour of mercury, which is thus applied to the whole surface of the body in a moist state. After the patient has remained in the bath from five to ten minutes, perspiration generally commences, and by the end of twenty or thirty minutes, beyond which I do not prolong the bath, it is generally excessive. The lamps are now removed, and the temperature gradually allowed to sink; when the patient has become moderately cool, the coverings are removed, and the body rubbed dry; the patient is suffered to repose in an arm-chair for a short time, during which he drinks a cup of warm decoction of guaiacum, sweetened with syrup of sarsaparilla." (pp. 5-6.)

When mercury is to be employed internally in combination with the baths, Mr. Parker prefers the binoxide or bichloride, in doses not exceeding the twentieth of a grain.

Such are the main features of Mr. Parker's method of treating that protean disease, Syphilis. There are some minor details, such as attention to the bowels, the regulation of the quantity of mercury, and the time of remaining in the bath, which will readily suggest themselves, or can easily be learned from the book itself. Our object is only to direct the attention of the profession to it; and we shall therefore abstain from the expression of any opinion, until we have an opportunity of judging for ourselves. In transcribing some cases from Mr. Parker's volume, the observation occurs, that they are almost all destitute of dates, and of many of those minute particulars which alone can give a scientific value to them; nevertheless, such as they are, we present them to the consideration of our readers. The first is an example of superficial disease of the skin, in which the constitution of the patient was inimical to the employment of mercury in the ordinary way.

"A gentleman consulted me respecting certain symptoms, which he considered, and which doubtless were, due to constitutional syphilis. He had had primary ulcers eight months previously, for which he could not take mercury; the smallest quantity produced diarrhoea, and it even affected him so, when used by friction.

The ulcers had healed under a simple treatment, but, soon afterwards, the skin became covered with small, scaly blotches; there was deep redness of the throat and nasal fossæ, and the hair and eyebrows came off rapidly. He had taken iodine and sarsaparilla, under various forms, without success; occasionally there was a partial amendment, but he constantly relapsed when medicine was discontinued.

"So confident was I of success in this case, that I predicted a cure by the baths in three weeks. I told my patient he would not relapse. The result justified my prophecy.

"The baths were used twelve times, the gums rendered uneasy and swollen, but nothing more, not a bad symptom accompanied the treatment, and the patient has had no fresh symptom for fifteen months. He took no internal medicines whilst under my care. The third bath checked the falling of the hair and eyebrows, which began rapidly to reappear before the termination of the treatment." (pp. 21-2.)

We shall next cite a case of sloughing phagedæna quickly arrested.

"A gentleman contracted, from a suspicious connection, a discharge from his urethra, which, in the commencement, was supposed to be gonorrhœa, and for which he was treated. The discharge did not yield to the remedies employed, and about ten days afterwards there appeared round the orifice of the urethra a white ring of ulceration, which spread rapidly. His surgeon became alarmed, and sent him to Birmingham, to be placed under my care. When I first saw this case there was an ulcer the size of a shilling surrounding the meatus, covered with a white slough, and the whole of the glans penis was intensely red, swollen, and shining. On separating the lips of the urethra, the ulcer was seen to extend some distance down the passage.

"I placed this patient immediately in the bath, and kept him there nearly an hour; he was directed afterwards to take a full dose of opium, to apply some decoction of poppies to the part, and to confine himself strictly to bed. On the next day the bath was repeated, and the same practice followed. On the third day the bath was again taken, by which time the sloughs were separating, and a healthy granular surface appeared underneath.

"There had been no extension of ulceration since the first bath. Nine baths completed this patient's cure in less than three weeks, and the medicines employed, as well as the local applications, were of the simplest character. The mutilation was very trivial. The under surface of the urethra and glans penis was destroyed to a small extent, but from this the patient suffered very little inconvenience.

"This was a case of phagedænic ulceration, commencing, as it frequently does, in or at the orifice of the urethra, with the nature of which I was unhappily too familiar, having seen frightful mutilation from sores of this nature under ordinary plans of treatment.

"The first case of secondary phagedæna of the throat, in which I had employed the mercurial vapour-bath, had been so successful, that I felt confident of success here, and the result justified my expectations. I reflected that rules of practice in cases of phagedæna were quite unsettled, mercury being frequently employed as a last resource after the failure of other remedies. It is during this period, of bringing remedies to bear upon the disease, and the uncertainty of what to use, that the mutilations so commonly witnessed in such diseases occur. The remedies which I advocate are without risk, and may be employed from the very first appearance of phagedæna, with every hope of success even in the worst cases." (pp. 29-31.)

Lastly, a case which is too recent to be of much value in showing the power of the baths in preventing relapses, but is still sufficiently remarkable to merit quotation.

"A dramatic artist of celebrity, who had been married for some years, and who had not had any primary venereal disease, in any shape or form, since his marriage, consulted me in the early part of 1849. He came to me as a forlorn hope, despairing

of relief, as he had constantly relapsed after discontinuing the best-framed ordinary treatments, conducted by eminent surgeons, amongst whom may be mentioned the late Mr. Aston Key. When I first saw this gentleman, his chief complaint was of the left fore-arm, the bones of which were much enlarged, very painful, and tender to the touch; but his sufferings were much increased during the night, when the pains were at times so excruciating as to deprive him more or less completely of rest; he had not slept one night, without pain, for seven years. The radius and ulna near the wrist were much enlarged, and were nodulated and uneven. The bones of the nose were a good deal thickened, and he had shooting pains in them. The left testis was five times the size of the right, heavy and lobulated, but neither painful nor tender.

"I considered this case as one of a decided venereal character; although mercury, pushed to salivation, and iodine had previously failed in affording more than a mere transient and temporary relief. Knowing the benefit which I had derived in many former cases, from the use of the baths, I held out to my patient a hope that they might be serviceable to him also.

"I directed him to use half an ounce of the bisulphuret of mercury, and half a drachm of the iodide of mercury for each of the first three baths, which were to be taken every third day, and to take internally some small doses of the hydriodate of potass with colchicum.

"After the third bath, which had slightly affected the mouth, the nocturnal pains had disappeared, and the tenderness was gone from the arm; he could bear the bones pressed and handled, although previously they had been exquisitely tender. The treatment was continued at intervals for three months; sometimes the baths were taken once a week, at other times not so frequently, the medicines also were continued. The pains never returned, and at the end of the period I mention, the nose and testis had been long perfectly well.

"This was a well-marked case of chronic syphilitic periostitis; the treatment of which, by my method, was rapid, safe, and successful. Perhaps no forms of constitutional Syphilis are more formidable than those which are seated in the periosteum and bones; and such affections are very frequently due to exposure, neglect, or want of care during a mercurial course which has been prescribed for the cure of some form of secondary disease, but which not only frequently fails in curing it, but disposes the system to the production of new symptoms of a still more formidable character than those for which the remedies were originally prescribed. It has been said that the iodide of potassium is all but a specific for periosteal inflammation, more particularly that of syphilitic origin; it is certainly, in many cases, an excellent and efficient remedy; but there are many cases also in which it totally and completely fails; not so with the plan of treatment I am advocating: it is all but certain in its influence over such diseases, and the rapidity with which it cures is very remarkable.

"The patient, whose case I have just detailed, was so convinced of the superiority of this method, that I could with difficulty prevent him sending the details of his case, and its treatment, to a Metropolitan daily paper. He had been under the best ordinary treatment for nearly seven years, with little or no benefit; and he was completely and permanently cured in three months by my method. In such cases I believe, after several experiments made on the subject, that the best remedies to employ are combinations of the bisulphuret and the iodide of mercury in the proportions, or nearly so, which I have mentioned in the details of the case." (pp. 98-102.)

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## ART. V.

1. *Fruits and Farinacea the Proper Food of Man; being an Attempt to prove from History, Anatomy, Physiology, and Chemistry, that the Original, Natural, and Best Diet of Man is derived from the Vegetable Kingdom.* By JOHN SMITH. Second Edition.—London, 1849. Fcap. 8vo, pp. 342.
2. *Lectures on the Science of Human Life.* By SYLVESTER GRAHAM. (Reprinted from the American Edition.)—London, 1849. Royal 8vo, pp. 289.
3. *A Letter to a Friend, in reply to the Question, What is Vegetarianism?*—London, 1849. 12mo, pp. 47.
4. *Recipes of Vegetarian Diet; with Suggestions for the Formation of a Dietary, from which the Flesh of Animals is excluded.*—London, 1848. 12mo, pp. 40.
5. *The Vegetarian Messenger.* Nos. I-V.—Manchester, 1850. 8vo.
6. *De l'Influence comparative du Régime Végétal, et du Régime Animal sur le Physique et le Moral de l'Homme.* Par EMILE MARCHAND, D.M.P., Lauréat de l'Académie Nationale de Médecine, &c. &c.—Paris, 1849. 8vo, pp. 266.

*On the Comparative Influence of Animal and Vegetable Diet on the Physical and on the Moral Condition of Man.* By DR. EMILE MARCHAND.

WHAT is "Vegetarianism?" The answer will vary, according as it describes the *principles* or the *practice* of Vegetarians; for between the two there is a most extraordinary discrepancy. Look at this picture, and look at that:

## PRINCIPLES.

"The principle of Vegetarianism, like any element of food, is plain and simple;—that man, as a physical, intellectual, and moral being, desiring the development of all his faculties to their fullest extent, can best accomplish his desire by living according to his original constitution or nature, which requires that he should subsist on the *direct productions* of the vegetable kingdom, and *totally abstain* from the flesh and blood of the Animal creation."

"An individual who subsists upon the products of the vegetable kingdom, and abstains entirely from the flesh of animals, is considered a Vegetarian; and is eligible as a member of the Vegetarian Society." (The Vegetarian Messenger, p. 2.)

"Although the Vegetarian practice varies considerably, with different individuals and families, it may be generally described as partaking of farinaceous food in the form of bread, soups, omelets, puddings, moulds; of vegetables, pre-

## PRACTICE.

## RECIPES OF VEGETARIAN DIET.

*Principal Dishes.*

5. *Omelet.* Ingredients;—5 Eggs; 2 oz. Onion; 2 oz. Bread-crumbs;  $\frac{1}{2}$  teaspoonful of Sage. To be fried in Butter.

6. *Rice Fritters.* Ingredients;—6 oz. of Rice and 5 Eggs. To be fried in Butter.

7. *Baked Bread Omelet.* Ingredients;—6 oz. of Stale Bread; 5 Eggs;  $\frac{1}{2}$  oz. of Parsley;  $\frac{1}{4}$  oz. of Lemon Thyme. To be baked in a well-buttered dish.

8. *Onion and Sage Fritters.* Ingredients;—5 oz. of Onion; teaspoonful of powdered Sage; 4 Eggs; and 4 oz. of Stale Bread. To be fried in Butter.

9. *Baked Rice Omelet.* Ingredients;—6 oz. of Rice; 6 Eggs; and  $\frac{1}{2}$  oz. of Parsley. To be baked in a well-buttered dish.

10. *Bread and Parsley Fritters.* Ingredients;—6 oz. of Stale Bread; 4 Eggs; and  $\frac{1}{2}$  oz. of Parsley. To be fried in Butter.

11. *Forcemeat Fritters.* Ingredients;—8 oz. of stale Bread-crumbs; 3 oz. of

pared in various ways, according to the best principles of cookery; and of fruit, ripe, dried, preserved, or prepared separately, or in combination with pastry." (Op. cit., p. 24.)

"It can be shown that the organization of Man has been especially adapted for the digestion of Fruit and Farinaceous substances; that upon that diet Man possesses sounder health and is less liable to disease; that his development is more complete, and his organs better fitted for discharging their respective functions; that the mental powers are more active, the animal passions more controllable, the moral and social virtues more practicable, and, consequently, the pleasures arising therefrom of a much higher and more satisfactory character; that that diet is more consistent with universal benevolence, and more favorable to a long and healthy life; and that it is capable of supporting a much larger population upon the same extent of ground." (Speech of Mr. John Smith, at the Vegetarian Festival, July 12, 1849.)

"Thus, after having carefully and minutely examined all the anatomical and all the physiological evidence in relation to the natural dietetic character of Man, we perceive that there is not the slightest reason for considering Man an omnivorous animal; but that every jot and tittle, both of anatomical and physiological, and I may add of psychological, evidence relevant to the question, go to prove most clearly and conclusively that MAN IS NATURALLY A FRUGIVOROUS AND GRANIVOROUS, OR A FRUIT AND VEGETABLE-EATING ANIMAL." (Graham's Lectures on the Science of Human Life, § 1236.)

Butter; 4 Eggs; 6 oz. of Cream; 1 oz. of Parsley;  $\frac{1}{2}$  oz. of Leeks;  $\frac{1}{2}$  oz. of Sweet Marjoram, Winter Savory, and Lemon Thyme. To be fried in Butter, and garnished with 2 Eggs, previously boiled hard and sliced.

#### Secondary Dishes.

12. *Buttered Eggs.* Ingredients;—7 Eggs;  $\frac{1}{2}$  oz. of Butter, and 1 oz. of Milk.

13. *Fried Eggs with Parsley.* Ingredients;—7 Eggs;  $\frac{1}{2}$  oz. of dried Parsley; and  $\frac{1}{2}$  oz. of Flour. To be fried in Butter.

14. *Baked Eggs.* Ingredients;—6 Eggs; and 1 oz. of Butter.

15. *Egg Fritters.* Ingredients;—6 Eggs; and 1 oz. of Shalot or Chives. To be fried in Butter.

16. *Fried Eggs.* Ingredients;—6 Eggs; and 2 oz. of Butter.

17. *Minced Eggs.* Ingredients;—4 Eggs;  $\frac{1}{2}$  oz. of Flour; 1 pint of New Milk; and  $\frac{1}{2}$  oz. of Parsley.

18. *Fried Vegetable Marrow.* Ingredients;—1 large Marrow; 1 Egg; and 2 oz. of Bread-crumbs.

#### Sauce.

26. *Brown Sauce.* Ingredients;—2 oz. of Butter; and 1 oz. of Flour.

#### Puddings.

27. *Baked Bread Pudding.* Ingredients;—6 oz. of Stale Bread-crumbs; 1 pint of Milk; and 2 Eggs.

28. *Boiled Vermicelli Pudding.* Ingredients;—2 oz. of Vermicelli; 3 Eggs; and 1 pint of Milk. To be boiled in a buttered basin.

29. *Hominy Pudding.* Ingredients;— $\frac{1}{2}$  lb. of Hominy; 3 Eggs; and 2 $\frac{1}{2}$  pints of Milk.

30. *Light Batter Pudding.* Ingredients;—2 oz. of Flour; 4 Eggs; 1 pint of new Milk; and  $\frac{1}{2}$  oz. of Butter. To be baked in a buttered dish.

31. *Baked Ground Rice Pudding.* Ingredients;—2 oz. of Ground Rice; 2 Eggs; and 1 pint of Milk.

32. *Baked Vermicelli Pudding.* Ingredients;—2 oz. of Vermicelli; 3 Eggs; and 1 pint of Milk.

33. *Moulded Rice.* Ingredients;—10 oz. of Rice; and 1 $\frac{1}{2}$  pint of Milk.

34. *Baked Custard Pudding.* Ingredients;—4 Eggs; and 1 pint of Milk.

35. *Milk Rice Pudding.* Ingredients;—10 oz. of Rice; and 3 pints of Milk.

Thus, then, we see that the "Vegetarian" *professes* not merely the negative principle of total abstinence from the flesh and blood of the Animal creation ; but also the positive doctrine that the nature of Man requires that he should subsist on the direct productions of the Vegetable kingdom, namely, Fruits, Farinacea, and other substances which it furnishes in a state fit for human food. Nothing, we should have thought, can be more simple and intelligible than these assertions. Now let us examine how far the disciples of the Vegetarian system act up to their profession of faith. In order to determine this point, it is not requisite for us to invade the sanctity of domestic life,—to pry into the kitchen-economy of Mr. Joseph Brotherton, M.P., or to investigate what Mr. James Simpson (President of the Vegetarian Society) eats for dinner. We simply take one of the publications referred to by the advocates of the system, as containing directions for a vegetarian dietary, and examine the composition of the dishes which are therein recommended. Nos. 1 to 4 of these recipes are for *Soups*, which are strictly vegetarian, with the exception of a small allowance of butter. Then follow the directions for seven *Principal Dishes*, of the entire series of which (to avoid any charge of unfairness in selection) we have quoted the ingredients. To these succeed the directions for seven *Secondary Dishes* ; of which, also, we have catalogued the materials. We have not thought it necessary to quote the directions for the *Side Dishes*, which are truly vegetable ; but we observe that, in the directions for preparing nearly all of them, the use of butter is enjoined. The Brown Sauce (No. 26) is repeatedly directed as an adjunct to the preceding dishes. Then follow the *Puddings*, of which we have brought in the whole series.

Now, upon looking through the list, the first thing that will strike the reader is the extraordinary number of *Eggs* employed. These enter largely into *all* the principal dishes, and form the chief staple of most of the secondary ; whilst out of the whole list of puddings, there are but two of which they do not form a part. But it is not merely the *presence*, but the *predominance* of eggs, that strikes us as strangely inconsistent with the Vegetarian professions. Thus we may suppose our vegetarian friend to begin with Rice Fritters (No. 6) as his *pièce de résistance* ; then to dally with a secondary dish of Fried Eggs (No. 16), served, as directed, with Brown Sauce ; adding thereto, from a side dish, a due proportion of Cauliflower or Celery fried in butter, and a decent allowance of simple vegetables ; to wind up with a Baked Ground Rice or Custard Pudding (Nos. 31 and 34) ; and then to declaim upon the excellencies of his Vegetarian Dinner, and upon his mental, moral, and physical superiority to the poor deluded eaters of animal flesh ! Can any absurdity be more monstrous ? Thus, take the Sunday's dinner (p. 28), consisting of "Force-meat Fritters (No. 11), Fried Eggs (No. 16), Boiled Macaroni (No. 24), Fried Beet-root (No. 25), Potatoes (boiled, or mashed and browned), Cauliflower or Spinach, Baked Vermicelli Pudding (No. 32), Fruit Tarts." This is described as an adequate allowance for seven persons ; and we find it to contain no fewer than *thirteen Eggs* (or very nearly two per head), *five ounces of Butter*, besides that used in frying the vegetables, and in sauce, which will be at least two ounces more (making an ounce per head), a teacupful of *Cream* and a pint and a half of *Milk*, together equivalent to *two pints of the richest Milk* (or nearly six ounces per head).

Now a middle-sized egg (without shell) weighs about 800 grains, more or less; and we have ascertained that the proportion of dry solid matter contained in an egg is about  $28\frac{1}{2}$  per cent., whilst that contained in ox-flesh is only  $22\frac{1}{2}$  per cent.; consequently, of the 1600 grains contained in two eggs, as much as 456 grains consist of *dry animal matter*, nearly the whole of it albuminous; and this amount is equivalent to that which would be furnished by 2000 grains, or about *four ounces and a half* (avoirdupois) of ox-flesh, alias the lean of beef steak. To this we have to add the equivalent of the albuminous matter contained in six ounces of milk; which is nearly another two ounces of lean beef-steak, making in all above *six ounces*. To this we have also to add an ounce of butter, and fully half as much more for the oleaginous matter of the milk; making the equivalent of at least an ounce and a half of the fat of meat. So that in the Vegetarian dinner, of which we have given the bill of fare, we may estimate that each individual will consume the equivalent of more than *seven ounces of animal flesh and fat*, in albuminous and oleaginous matters derived from animal sources alone.

Such is Vegetarian practice! Now let us compare this with the practice of moderate flesh-consumers. For this purpose we take the actual experience of a family consisting of eight persons, five adults and three children of different ages, all of them accustomed to eat as much meat as they feel an inclination for, and making their usual dinner off meat and plainly cooked vegetables, with an occasional plain pudding. The average quarterly consumption of this family is 320 lbs.; that is, 3 lbs. 9 oz. (avoird.) per day, or 7 oz. per head. But this includes not merely the suet employed in making puddings &c., which may stand as the partial equivalent of the butter of the Vegetarian, but also bone and other waste; for these it is not certainly too much to allow one seventh of the weight; so that the actual amount of lean flesh consumed is not more than *six ounces* per head. Thus, even throwing in the odd ounce for eggs employed for puddings, butter for sauces, &c. &c., we find that the so-called vegetarian positively consumes, according to his own diet-scale, as *much animal food* as the avowed flesh eater.

But we may be told that milk and eggs differ essentially from flesh-meat; and that although they are derived from an animal source, yet that they are not to be put in the same category with animal flesh. Now we have carefully examined the various manifestoes of the English Vegetarians, and can find no allusion to this point,—nothing, in fact, that can at all explain away this extraordinary discrepancy between principles and practice. Mr. Sylvester Graham, indeed, plainly tells us that he considers milk and eggs as coming under the same objectionable category as flesh-meat; and, with a laudable regard to consistency, discourages their use almost as strongly. But he would seem to have but few followers in this country; at any rate they do not lay great stress on this part of their tenets; for the only recognition of their existence we have met with, is contained in the following passage of the Preface to the *Recipes of Vegetarian Diet*:

“The recipes are adapted, principally, for the use of Vegetarians making use of milk, butter and eggs; but, since there are many who do not allow these to enter into the composition of their food, it is intended, shortly, to issue a second part, containing such recipes as will supply a Dietary suited to those entering upon Vegetarianism in this last acceptance of the term.”

These last are evidently the only true Vegetarians. The *ovophagi* and *galactophagi* are really *kreatophagi* under another name. For all chemical and physiological purposes, an egg is the precise equivalent of the chick developed from it; and a pint of milk represents a certain measure of calves' flesh, bone, and fat. The only difference consists in the fact, that eggs and milk can be consumed by man without the sacrifice of *conscious life*; whilst in the consumption of flesh there is a necessary destruction of sentient existence. This, however, is a difference which concerns the moralist rather than the physiologist. We shall have a word or two to say upon the question presently; but in the mean time we confine ourselves to the physiological view of the subject.

Now to us it appears that, with all due respect to the very well-meaning individuals who are getting up the Vegetarian agitation, they are committing the very same blunder as if a professed Teetotaler were to construe his pledge against Alcohol into abstinence from distilled spirits alone, and were to refrain from brandy, gin, and brandied wines, to indulge liberally in beer, ale, and porter. And even those who "go the entire animal," but who sit down at the same table with the aforesaid *ovophagi* and *galactophagi*, at a Vegetarian festival, are therein as inconsistent as a genuine Teetotaler would be, in uniting in a Total-abstinence festival with the drinkers of malt liquor and unbranded wine. Yet we find the author of 'Fruits and Farinacea' partaking of a banquet, of which the following Savoury Dishes formed part: "Savoury Pies, Mushroom Pies, Savoury Fritters, Bread and Parsley Fritters, Rice Fritters, and Beet-root." (Vegetable Advocate, p. 4.) We have shown what is the composition of these Fritters; and that of the Savoury and Mushroom Pies is probably not very dissimilar in regard to the one essential point of containing Eggs. Then, again, we find as the sweets of the banquet, "Moulded Rice, Moulded Ground Rice, Moulded Sago, Moulded Barley, Blanc Mange, Cheese-cakes, and Custards;" in the first five of which we believe that Milk is a principal and necessary ingredient; whilst the yolk of eggs forms the staple of the last two! And yet Mr. Smith can get up and make a speech upon the claims of the Vegetarian system to "the attentive consideration of Society, as conducive to the physical, moral, and spiritual well-being of Man;" totally oblivious of the fact, that if the world were to become Vegetarian after the fashion of his society, the only essential change would be that instead of fattening sheep and oxen, the farmer must devote himself to the care of milch-cows and poultry, his sheep-pens being converted into hen-roosts, and his ox-stalls appropriated entirely to the feminine gender. Smithfield must undergo a similar reconstruction; and peaceful flocks of poultry will take the place of the rampant beasts which at present render its neighbourhood undesirable. Instead of premiums for fat oxen, our agricultural societies will offer prizes for the best milkers and the most prolific hens. The butcher will give up his bloody trade, and will start as an egg-merchant. And then will be the time for the formation of the oft-projected Company, for supplying all London with milk, "laying it on" fresh from the cow, through a service-pipe to every house, so as to secure its genuineness.

Now all this *may* be very desirable; but it is not true Vegetarianism, being nothing else than the substitution of one form of Animal food for another. That any body will be a gainer by the change, in an economical point of view, we are at a loss to imagine. Impressed with the appetising

character of some of the "Recipes," we have requested to have them introduced into our own *cuisine*; but the reply from the head of our domestic establishment has been invariably the same to each one of our propositions,—namely, that the ingredients are too expensive, and the time occupied in their preparation too great! And upon carefully comparing the cost of the Vegetarian dinner we have cited, or of any others in the week's list contained in the "Recipes," with that of our own ordinary fare of good butcher's meat and plain vegetables, we find that the objection is well founded. It is true that the so-called Vegetarian has some advantage in point of variety; but this is gained at the expense of a large amount of time consumed in preparation; and we are not sure but that it is a questionable advantage after all, as tending to an excitement of the gustative propensity,—the very thing which Vegetarians so loudly declaim against, and charge the flesh-eaters with encouraging. Moreover, all this variety is just as legitimately within the range of the omnivorous feeder, as within that of the Vegetarian. We should like to know who may most consistently place omelets and egg-fritters upon his table,—the man who believes that the Creator intended him to eat the products of Animal as well as Vegetable life,—or he who maintains that the welfare of the human race, both physically, morally, and intellectually, is best consulted by a diet of fruits and farinacea? Answer us that, Mr. Smith!

We need not, we think, dwell any longer on the *practice* of these self-named Vegetarians, who so amiably endeavour to delude themselves and the public into the idea, that they are wiser and better than the poor blinded *kreatophagi* around them. We could raise many a laugh at their expense, by analysing the speeches of the orators at the vegetarian festival, and some of their other manifestoes; but "ridicule is not the test of truth;" and we have introduced the subject to our readers, rather for the sake of calmly inquiring what there is in the *principles* of Vegetarianism that deserves their serious consideration, than of taking its professors to task for the mode in which they carry them out. We think we shall be able to show, that the inquiry will not be profitless; and that in studying the principles of dietetics we may take a leaf out of the vegetarian book, which may afford us materials for the improvement of our dietetic treatment of disease, even if we find no sufficient reason for changing our usual habits in health.

Before we go into the physiological and therapeutic part of the inquiry, we may say a few words upon the one point in which the practice of Vegetarians differs from that of the world in general. So far as the real nature and source of their staple food are concerned, we have found it to be essentially the same; but the consumption of it does not involve the destruction of sentient existence; and in this appears to be, in their eyes, the distinguishing merit of their system. Now for ourselves we must say, that we hold this to be a piece of maudlin sentimentality, quite unworthy of people of moderately-enlarged understandings. Carry out this principle, and see to what it leads. The vegetarian who chances to inhabit a house overrun with mice and rats, will most assuredly be acting contrary to his principles if he attempt to extirpate them, whether by a trap or a cat; for although an ingenious casuist may say of the latter mode, that in setting a natural enemy to destroy them, he merely carries out the purposes of the

Creator, yet to this we may reply in two modes ; namely, by the well-approved adage—*qui facit per alium facit per se* ;—and also, that in taking advantage of the mouse-hunting propensity of the cat, he merely substitutes one kind of trap for another less agreeable to his feelings, like the monkey who used the cat's paw to take the chesnuts out of the fire. Nor, if his kitchen be befouled by cock-roaches and black-beetles, may he bait a trap or set a hedgehog for their destruction. Nor, if his bedroom be besieged by fleas or bugs, may he take any measures to annihilate the unclean pests, but must patiently give his body to be bitten, and permit them to increase and multiply at discretion. Once admit the principle that man is not to take away the life of any of the inferior animals, for anything short of the defence of his own ; and to carry it out consistently, will involve a degree of self-sacrifice which our vegetarian friends seem little to dream of. There is no more evil, to our minds, in killing sheep or oxen, game or poultry, for human nutriment, than there is in killing the blood-thirsty insect pests which harass our repose, the consequences of whose unrestrained increase can be duly appreciated by those alone who have had the ill fortune to be personally subjected to them. Perhaps it may come more directly home to the business and bosoms of our vegetarian friends, if we point out to them, that upon their theory the farmer and the gardener have no right to take any measures for the destruction of the "vermin" that would devour the produce of their fields and gardens, but must quietly submit to any amount of depredation ; since even the introduction of their natural antagonists, such as the rooks are to the cockchaffer-grubs and wire-worms, or the moles to the earth-worms, is but the equivalent (as we have just pointed out) of setting a humanly-constructed trap for their annihilation.

The simple fact appears to us to be, that the present system of rearing animals for human food is productive of a large amount of animal enjoyment, and is attended with a very small countervailing proportion of suffering. The whole character of the domesticated ruminants adapts them to the society of man ; and they find their highest happiness in the constant supply of their animal wants, which it is his interest to provide for them. Let our objectors look at the calm placidity of an ox in the act of rumination, and say whether he is not enjoying life as much as it is in his nature to do. We allow that, at present, much needless pain is inflicted in the killing of animals for food ; but this is not a necessary part of the system, and is quite capable of being put an end to. But even at present, who will assert that a short period of suffering, unanticipated, suddenly inflicted, and speedily terminated, approximates the torments of a lingering death by disease, the decay of old age, and the consequent starvation ; or of that death from the attacks of their carnivorous enemies, which must, in a state of nature, be continually before the apprehensions of these quadrupeds, as the special adaptation of their acute sensations to give them timely warning, and that of their locomotive apparatus to a rapid flight, viewed in connexion with their timorous nature, most fully demonstrate. Which mode of death, we would ask, is to a stag or a hare the least attended with animal suffering ; to be killed at once by the shot of a sportsman, in the midst of the tranquil enjoyment of life ; or to be hunted by fierce hounds, to be kept for hours in all the agony of the death-struggle, and to be torn to pieces while yet alive at the last ? The first method is that which we

should most certainly prefer, could we fancy ourselves placed in similar circumstances; and it appears to us to be the duty of civilized society, to require that those who discharge the office of slaughterers should perform their task with the least possible amount of suffering to the animals upon whom it has to be executed. The second is the mode in which death comes to the majority of herbivorous animals, in that peaceful state of nature, to which the Vegetarian Society would have us return. We conclude, then, that whether we look at the amount of sentient enjoyment secured by the present system of rearing animals as food, or at the diminished amount of suffering which is involved in that sudden and unexpected termination of their lives which is requisite for the supply of the wants of man, the system of flesh-eating is far from being worthy of condemnation on the ground of humanity,—notwithstanding all that may be felt in individual cases as to the sacrifice of a pet lamb, or the decapitation of a favorite chicken.

One objection to the use of animal flesh as food, which we find urged in vegetarian treatises, is so absurd, that, like the preceding, it seems to us scarcely worthy of grave consideration in a scientific inquiry. It is, that as animals are subject to disease, and as the flesh of diseased animals is used as an article of human food, their diseases are introduced into our own systems; and we are gravely told that “the fearful havoc made by epidemics apparently unaccountable, no doubt frequently arises from this cause;” and that “cancerous and pulmonary affections have been enormously increased of late years in London, by the quantity of meat unfit for human food which is daily disposed of within the bills of mortality.” Now, supposing these assertions to be true, so far as regards the habitual consumption of any considerable proportion of diseased meat, we ask the vegetarian whether he ever heard of diseased corn, and what safeguard he has against the admixture of bad flour with good, in every piece of bread he puts into his mouth. It is only, as in the case of meat, the *very bad* which is rejected, as we have ascertained by careful examination. The bread of inferior quality, sold by the second- and third-rate bakers, which constitutes the food of the great mass of the labouring classes, contains a large proportion of flour that ought not to be employed for human food, this being only useable when mixed with flour of a superior quality. We have seen specimens of such bread, which contained an admixture of fungous sporules that would not have been credited; and we have good reason to believe, that the use of it has, in many instances, been followed by the most pernicious results. It is, in fact, only by purchasing their bread of the first-class bakers, who pride themselves upon never employing any but the best flour, that the public can be secure against this introduction of a noxious ingredient into their staple article of food; and we are sure that it is just as easy for those who can afford to pay a good price to a respectable butcher, eschewing cook-shops and sausage-makers, to obtain the flesh of healthy fine-conditioned beasts for their daily food. The misfortune in each case lies in this, that the mass of the population has no check against the bakers and butchers by whom they are supplied; and we do not see how this evil can be remedied by any system of official supervision that would not be too intrusive. We must trust in the extension of information as to the evils of the present system, and in the improvement of the tone of general feeling as to the social duties of mankind; and however slow this may be, we feel sure, that we should be acting much more wisely in



endeavouring to show to the unprincipled butcher and the baker the evils of the system they are pursuing, and to appealing to their better nature to abstain from administering slow poison to their fellow-creatures, than in agitating for any system of legislative interference, which (according to all our experience of human nature) can only accomplish its end but imperfectly, and merely keeps the evil in check without destroying or weakening its source. For ourselves we do not hesitate to express the belief, that a *far larger* amount of mischief is done by the sale of bad wheat, than by that of diseased cattle and sheep; and that there is fully as much evidence as to the production of disease in individuals and communities by the habitual use of inferior bread,—poisoned as it is, not merely by the admixture of fungous and decomposing particles, but also by the various “doctorings” which are introduced for the improvement of the article to the tradesman’s eye,—as there is of injurious results from the consumption of bad meat, whose qualities are by no means capable of the same concealment. In regard to epidemics, we shall only say that none but those who have a cause to serve, would think of attributing their prevalence among all classes of the community, to a cause which can only affect a few with any degree of potency.

These little skirmishes, like those with which a serious battle usually commences, will leave us free to grapple with the part of the inquiry in which, as scientific investigators, we have the most concern; and we propose, therefore, to consider—

1st. The indications afforded by the history of the human race, and by the physical and mental constitution of man, as to the diet for which his Creator intended him.

2d. The results of experience as to the use of an exclusively animal, an exclusively vegetable, and a mixed diet.

3d. The relative influence of different dietetic systems upon the human economy, and the therapeutic advantage which may be derived from it.

Into the first of these inquiries, Mr. Smith, as well as Mr. Sylvester Graham, enter very minutely; and a great deal of unnecessary learning is brought to bear upon the question. Thus, we find Mr. Smith dwelling much upon the “original food of man,” and quoting profane as well as sacred history to show that, in the primitive ages, men lived upon the fruits of the earth, and eschewed animal flesh. This we take to have been because they knew no better. We quite admit that man’s natural instincts do not lead him to devour raw meat; so that, until the art of cookery was invented, he had no option between animal and vegetable food. But whether the discovery of the marvellous effects of fire was made in the way so admirably chronicled by Charles Lamb,\* or in some other, it is certain that from the time when the first roast was eaten, man has continued to be a “cooking animal.” And we think that there is abundant reason to believe, that the Creator intended man to use his wits in the coctorial art, just as much as in the arts of cultivation, or in providing himself with those external defences against the weather, of which other animals are made independent from the first. The question is narrowed, indeed, by the candid admission of Mr. Smith, that as man

\* See the Essay of Elia on “Roast Pig.”

was destined by the Creator to extend himself over the whole earth, and as there are certain climates where nothing but animal flesh can be obtained, there is a virtual permission given for its employment where necessary. This is, however, represented by him as an *evil* permitted by God, like many other evils, of which it is not consistent with the Divine plan to effect the immediate extinction; and he endeavours to show that, as "cruelty, immorality, and disease marked the progress of man in this unnatural diet," so a return to vegetarianism is necessary in order to bring back the golden age of innocence and healthfulness.

According to this doctrine, then, the progress of "cruelty, immorality, and disease" among different nations, should stand in a direct relation to the amount of animal food they consume, a position which Mr. Smith will find it hard to maintain. Take, for example, the Tahitians, who, until the introduction of European animals, were nearly model vegetarians, only partaking of a baked dog occasionally. What was *their* morality? The very existence of the Arreoi society, and the wholesale practice of child-murder, which formed a part of its system, constitute a sufficient answer. How little such a condition of morals has to do with the regimen, appears further from the fact, that it is usually in warm climates, and among races which are almost exclusively vegetarian, that there are the grossest exhibitions of the sexual passion. Take, for example, the following picture, given by Virey (*De la Femme*), of the general character of the Malay race in this respect:

"En général, les peuples Malais, jaloux et féroces dans leurs amours, sont extrêmement voluptueux. Les lois de la pudeur et de la virginité paraissent à ces peuples des conventions factices trop raffinées pour leur simplicité naturelle; aussi ne pensent-ils qu'à jouir; l'amour est, en quelque sorte, érigé en culte parmi eux, et l'acte le plus digne d'honorer l'Auteur de la nature leur paraît être de procréer son semblable. Il y a même des pays où les pères ne se font pas scrupule d'abuser de leur filles, prétendant que celui qui plante un arbre a bien le droit d'en goûter les fruits."

Of the females in particular, he says:

"Celles surtout qui ne vivent que de végétaux ont le teint moins olivâtre que les autres; et paraissent très passables aux marins. Leur constitution est grêle, nerveuse, d'une souplesse remarquable; mais leur caractère joint, pour l'ordinaire, l'inconstance à la perfidie. Elles montrent, dit on, des passions furieuses dans leurs amours, et poignardent où empoisonnent l'infidèle qui les a trompées."

And this is the golden age of innocence to which Vegetarianism is to restore us! For ourselves, we had much rather live in peaceful possession of our domestic establishments, fearing neither steel nor poison from our wives, and entertaining that confidence in their chastity which it is the glory of the middle class of *kreatophagous* Britons to be able to hold up as a pattern to the world. Now, however absurd our readers might think it, to attribute such a condition as that which we have described to the prevalence of a vegetable diet among the Malayan races, it is not one whit more absurd than a large part of the arguments adduced by Messrs. Graham and Smith, who, as we have seen, consider the departure from the supposed primeval condition of innocence as dependent upon the introduction of flesh into human diet,—totally regardless of the fact, that some of the most depraved races in the world are, and have ever been (so far as can be ascertained), almost exclusively vegetarian; whilst there are

many others, whose diet is as exclusively animal, who are by no means remarkable for blood-thirstiness, perfidy, or the violence of their animal passions.

Through the whole of the first part of his inquiry into the 'Original Food of Man,' Mr. Smith proceeds upon the supposition that, if it could be established that fruits and farinacea afforded his destined sustenance in the primitive period of his history, he is therefore intended by the Creator to feed upon fruits and farinacea to the end of time. In any such hypothesis (for it does not deserve to be ranked as an argument), it is entirely forgotten that man was created a *progressive* animal, whilst the nature of brutes, with few exceptions, is fixed and stationary. The ruminants will be herbivorous, and the felines will be carnivorous, to the end of their generation; the law, according to which they were at first conformed, being one that admits of but little modification by habit or external circumstances. But the case is far different in regard to man, whose whole history exhibits the gradual unfolding of a plan, of which no one period can be regarded as adequate to exhibit the entire scope. It might be just as well affirmed that man was destined to remain in the condition in which he still exists in the least advanced portions of the globe,—finding his whole support, in countries where abundance of food can be obtained from the spontaneous products of the earth, without any other labour than that of gathering them, and passing his time in stupid idleness; that he was not intended to till the ground, and increase its productiveness by his own exertions; that the Creator never designed him to live in great cities, or to build factories, or to construct rail-roads and electric telegraphs, or to throw bridges across broad rivers and arms of the sea, or to bore into the earth for coal and metals, or, in a word, to practise any of the arts of civilized life,—as that man was destined to live upon fruits and farinacea through the whole course of his existence, because his progenitors found these the most available sources of nutriment. To us it appears that a contemplation of the history of human progress leads to the conclusion, that, as mankind spread over countries more adapted by soil and climate for the support of graminivorous animals than for the production of articles of human food, he was compelled, by a latent instinct, to have recourse to the flesh of those animals as his partial sustenance; just as, when he advances into those ungenial climes where neither corn nor fruits can be raised from the soil, he was adapted to derive his whole support from the flesh and fat of the animals which the Creator has enabled to find their living there. It is perfectly obvious to us, that the demand for a larger supply of food than the earth's bounty spontaneously yielded, and the need felt of protection against the inclemencies of a climate very different from that in which the race was originally located, were among the first calls to increased exertion of his yet dormant powers, that man would feel. There never was a truer proverb than that "necessity is the mother of invention;" and it appears to us obvious that the Creator designed the "necessity," as the very means of calling forth the inventive faculty. Those races who obtain their food the most easily, are those who take the least trouble about anything in life; and until it shall be shown that the most desirable state for man is to feed upon pumpkins and bananas, and to lie dozing in the sun all day, we must rather take the experience of the world as it is, than appeal to the

wisdom, or rather to the ignorance, of our progenitors about what we ought to eat.

None of our readers can be ignorant, that very diverse inferences have been drawn from the conformation of the digestive apparatus of man, in regard to the nature of the food for which it is best adapted. It may be freely admitted, that when we look to the organs of the senses and to the locomotive apparatus, for indications of the method in which the food is to be obtained, we altogether miss the usual organization which accompanies the carnivorous propensity; but it does not hence follow that man was exclusively destined for an opposite regimen. One of the most wonderful peculiarities of the human constitution, is the improveability which is manifested in every one of its faculties. The senses of man may all be cultivated by constant attention to their indications, to such a degree as to serve every purpose that the keenest sight, scent, or hearing of Carnivorous animals can answer; in swiftness of foot, by the continual practice of running, he may rival or even surpass the less fleet among the Ruminants;\* and over all he has the advantage which his intellect gives him, enabling him to obtain that by sagacity, which his physical powers would not at first seem adapted to secure. The Creator has obviously fixed, in the organization of most animals, the conditions of their existence, in such a manner that they cannot be departed from. Under no conceivable combination of circumstances can we imagine the Deer or the Antelope to be capable of exchanging habits with the Tiger or the Wolf. The Ruminant could no more catch the prey of the Feline than it could digest its food. The Feline could no more find support in the food of the Ruminant, than it could crop and triturate the herbage that serves for its nutriment. On the other hand, there are many animals whose regimen seems to have been less determinately fixed by the Creator; their habits of life and their digestive powers being subject to considerable modifications under changes of external conditions, so that the carnivorous dog becomes a devourer of fruits and grains, and the herbivorous pig takes kindly to a flesh diet; thus being able to accommodate themselves to the habits and wants of man, in a way that seems to show that they were intended by the Creator so to do. In like manner, then, from the very fact that the digestive apparatus of man is adapted to make use of any kind of food which his taste leads him to appropriate,—that his taste adapts itself to almost any aliment of which his circumstances enable him to avail himself,—and that, notwithstanding his physical disadvantages, there is no spot where any kind of food is to be obtained, in which he cannot support himself,—we should be led to conclude, in the absence of any decidedly herbivorous or carnivorous character about his teeth, stomach, and alimentary canal, that the Creator had purposely left him to a free choice. This is admitted by the Vegetarian advocates: thus Mr. Sylvester Graham, as quoted with approval by Mr. Smith (p. 43), allows that “the alimentary organs speak no distinct and unequivocal language, from which, without reference to anything else, we can learn the natural dietetic character of man.”

Great stress is laid, however, on the resemblance of the human digestive apparatus, to that of the Quadrumana, and especially of the anthropoid

\* It will be remembered, that Alexander Selkirk, after the expenditure of his powder and shot, was accustomed to run down the goats which abounded on his island; and, at the time of his quitting it, he had attained a fleetness much greater than theirs.

Apes, whose natural regimen seems to be essentially frugivorous ; but the resemblance is not nearly so close as is represented by these writers ; and it would have been well if, in undertaking the office of public instructors, they had informed themselves a little more carefully as to the facts of the case. The fact is, that a considerable variety exists in the structure of the *stomach* of the different genera of *Quadrumanæ* ; for whilst in the anthropoid Apes this organ is formed upon the same general plan as in Man, in most of the Monkeys the cul-de-sac at the splenic extremity is relatively larger than in the human stomach ; and in the genus *Semnopithecus*, which includes most of the ordinary monkeys of the east, the stomach is of very complicated structure, being divided into three chambers,—first, a cardiac pouch, with smooth and simple parietes, slightly bifid at the extremity ; secondly, a middle, very wide, and sacculated portion ; and thirdly, a narrow elongated canal, sacculated at its commencement. Now these monkeys seem exclusively frugivorous, and do not readily take to animal food : but the Chimpanzee and Orang, when once introduced at their master's dinner tables, seem to relish a mixed diet amazingly. Even in a state of nature, many of the *Quadrumanæ* devour eggs and young birds with an avidity which shows a natural relish for them. In the structure of the *intestinal canal*, too, there is no more decided mark of the diet proper to man, than there is in that of the stomach. Most herbivorous animals which have a simple stomach, have an enormous colon, and a large and sacculated cæcum ; this is the case, for example, with the horse and other *Pachydermata*, and also in the hare. Now in man, although the stomach is simple, the cæcum is of very moderate size, and the large intestine presents no such great excess of diameter above the small ; so that in these points he bears more resemblance to the Carnivora. From the *teeth* of man, scarcely any but a negative inference appears to us to be deducible. They are certainly not organized for the gramineous regimen of the Ruminants, nor for the trituration of the tough vegetable substances which form the natural food of the Rodents. On the other hand, they are as certainly not intended, like those of the Felines, to tear and divide animal flesh. But if from this it be inferred that they are only intended by the Creator for crushing fruits and other pulpy vegetable substances, it must also be admitted that seeds and grains of all kinds were not intended to form part of Man's diet ; since, without some preparation, either by mechanical appliances or by heat, he would be entirely unable to avail himself of them ; so that his residence must be restricted to those parts of the globe in which the supply of fruits is adequate for his support. We cannot see how Mr. Smith and his co-partisans, who maintain that the farinacea enter into the natural food of man, can deny the same (on any *à priori* principle) of animal flesh ; for if the latter cannot be obtained in sufficient amount without artificial means, neither can the former ; if the latter is not agreeable to the taste of man until it has been cooked, neither are the former ; and if the latter be not specially adapted to the conformation of the masticating and digestive apparatus, the former are still less so. For man, although quite able to digest raw flesh if he should take a fancy to it, could not make any use of the ordinary farinacea, without calling in the aid of heat or of mechanical appliances to bring them into a condition in which his gastric fluid can act upon them ; being unprovided with roughened molars,

like those with which the horse rubs down his oats, or with a muscular gizzard, such as that which enables the granivorous birds to feed upon seeds.

We do not find, in any of the Vegetarian treatises, any reference to the relative composition of the *Blood* of Herbivorous and Carnivorous animals as compared with that of Man. This is a point which seems to us of fundamental importance; since the nutritive operations are immediately dependent upon the qualities of the circulating fluids, whilst they are only remotely connected with the operations of the digestive apparatus, through whose instrumentality the materials of that fluid are prepared. Hence, in a case in which the appropriate regimen is not indicated by the conformation of the teeth and of the alimentary canal, the composition of the blood seems to offer a valuable means of guidance. Now the researches of MM. Andral, Gavarret, and Delafond, give the following as the average result of their observations on the composition of the blood of Carnivora, Herbivora, and Man, respectively:

	Globules.	Fibrin.	Solids of Serum.
Herbivora . . . . .	95	3·5	90
Man . . . . .	125	2·0	88
Carnivora . . . . .	148	2·0	76

Now from this it appears that, in regard to the proportion of globules, Man is midway between the Carnivora and Herbivora; in the proportion of fibrin he approximates to the latter; whilst in the proportion of solid matter in the serum, he closely agrees with the former. The proportion of globules is capable as (we shall see hereafter) of being considerably affected by regimen, especially when it is much above or below the usual average; but the proportions of fibrin and of the solids of the serum would seem to be more definitely determined, or at any rate not to be easily affected, by an animal or a vegetable diet. Hence we seem justified in concluding, that the true place of Man is between the exclusively Herbivorous and the exclusively Carnivorous races.

In discussing the question whether Man is *Omnivorous*, Mr. Smith expresses the doubt whether any animal is strictly omnivorous;—that is, formed for feeding indiscriminately, or without preference, upon either animal or vegetable substances;—and with organs adapted for procuring, masticating, and digesting each kind of food with equal facility;—so as to attain the highest degree of perfection of which its nature is susceptible. Whilst admitting that man *can* feed with comparative health and pleasure on a mixture of both animal and vegetable substances,—that he *can* live on flesh alone,—and that he *can* support himself exclusively on fruits and other vegetables; he still maintains that man is not omnivorous, because this capacity for subsisting on a great variety of animal and vegetable productions—just as climate or circumstances may determine—“only evinces the wide range of adaptability which his organization has received.” Now this admission seems to us virtually to settle the question; because the very existence of this adaptability, to any one who believes that the human constitution is the work of a Designing Mind, speaks of the intention that the diet of Man *should* vary with climate and circumstance; and hence the attempt to fix upon any one regimen as that which is best adapted to develop the physical, mental, and moral powers of man, under all circumstances and in all climates, seems to us utterly futile.

Having thus disposed of the first question which we undertook to discuss, and having come to the conclusion that the Creator intended man to make use of every kind of nutriment, of which his taste might prompt him to avail himself, and which his ingenuity might enable him to appropriate, we come to the second head of our inquiry; namely,—The results of experience as to the use of an exclusively animal, an exclusively vegetable, and a mixed diet.

It is necessary, in entering upon any discussion, to have a clear understanding of the meaning of the terms employed; and, although we might suppose our readers to be sufficiently well informed as to the fundamental doctrines of dietetics, a brief recapitulation of them may be here desirable, in order to prevent misconception.

1. The aliment of man, whether derived from the vegetable or the animal kingdom, must contain two classes of compounds; namely, the *albuminous* and the *non-azotised*.

2. The former are the only substances capable of being applied to the replacement of the albuminous constituents of the blood, which are being continually drawn off in the acts of nutrition; and the demand for them varies in proportion to the activity of the nutritive processes, which again depends, *ceteris paribus*, upon the amount of previous waste or disintegration, resulting from the activity of the animal functions.

3. The latter serve only for the maintenance of the combusive process, whereby that additional heat is generated, which is not supplied by the oxidation of the matters set free by the waste of the tissues; and the demand for them will mainly depend upon the amount of caloric which must be evolved, in order to keep up the proper temperature of the body. It will, therefore, be greater in a low external temperature, and less in a higher,—greater within the arctic circle, and less between the tropics.

4. Both these classes of constituents exist in nearly every substance, whether vegetable or animal, ordinarily used as food. In vegetable substances, however, the proportion of the albuminous constituents is for the most part much smaller than in animal; but the relative amounts of the two differ so widely, that a "vegetable diet" may contain either a very large or a very small quantity of albuminous matter. Animal flesh, on the other hand, is almost entirely albuminous, and the only admixture of the non-azotised constituent is in the fat which may accompany it: hence an "Animal diet" ordinarily involves the predominance of the albuminous constituent, in every case save where (as among the inhabitants of the frigid zone) an enormous proportion of fat is consumed.

We learn from the chemist, that the substances upon which the renovation of the blood and tissues of the animal immediately depend, are all prepared for it by the plant; and we see the massive carcasses of the herbivorous animals gradually built up at their expense, without the assistance of a single particle of animal food. There is, therefore, no difficulty in understanding that man *may* be effectually supported upon the same regimen; his digestive power being adequate, with the assistance of the various mechanical and chemical appliances which his ingenuity suggests, to extract the alimentary materials from almost every kind of vegetable substance in which it is not too intimately combined with actual poisons. When we remind our readers that bread has been made from saw-dust, we think that we have said enough in regard to man's capacity

to avail himself of even the most unpromising materials. The question is not, however, whether the vegetable kingdom *can* afford adequate sustenance to man; but whether a vegetable regimen is universally, or even generally, the best for him, and whether the moderate use of animal food is attended with any such evils as should lead to the abandonment of it.

Now we freely concede to the advocates of Vegetarianism, that, as regards the endurance of physical labour, there is ample proof of the capacity of what is commonly called the vegetable regimen, that is, abstinence from flesh meat, to afford the requisite sustenance. It is to be borne in mind, however, that in a very large proportion of the cases quoted by the vegetarians in support of this position, *milk*, or some of its preparations, is employed, to a greater or less extent, and we can by no means admit that these are instances of pure vegetarianism. The Scotch peasant uses milk or buttermilk with his oatmeal, the Irish has the same with his potatoes; the English labourer, when he cannot get meat, eats cheese; and among many nations which do not manufacture cheese after the European fashion, curdled milk is employed as a staple article of diet. Still there appears ample and unexceptionable evidence, that where neither milk nor any of its preparations is in ordinary use, a regimen consisting of bread and fruits, &c. is quite adequate to the wants of a population subsisting by severe and constant toil. Of this evidence we shall quote one or two samples:—

"I have made several voyages to St. Petersburg, in Russia," says Capt. Cornelius S. Howland, of New Bedford, Mass. "The people of Russia generally subsist, for the most part, on coarse, black rye-bread and garlicks. The bread is exceedingly coarse,—sometimes containing almost whole grains; and it is very hard and dry. I have often hired men to labour for me in Russia; which they would do from sixteen to eighteen hours and 'find themselves,' for eight cents per day, the sun shining there sometimes twenty hours in the day. They would come on board in the morning, with a piece of their black bread weighing about one pound, and a bunch of garlicks as big as one's fist. This was all their nourishment for the day of sixteen or eighteen hours' labour. They were astonishingly powerful and active; and endured severe and protracted labour far beyond any of my men. Some of these men were eighty and even ninety years old; and yet these old men would do more work than any of the middle-aged men belonging to my ship. In handling and stowing away iron, and in stowing away hemp with the jack-screw, they exhibited most astonishing power. They were full of agility, vivacity, and even hilarity;—singing as they laboured, with all the buoyancy and blithesomeness of youth." (p. 164.)

The experience of the Russian army is to the same effect. Whatever they may be as to their mental and moral qualities, the Russian soldiers are remarkable for their robustness and powers of physical endurance. No animal food of any kind enters into their allowance, which consists of black bread, oil, and salt; and their pay is insufficient to enable them to procure the luxuries of meat or cheese. The diet of the Russian peasantry is chiefly composed of similar black bread, with pickled cucumbers, cabbages, or mushrooms; in some parts, however, milk enters largely into it. —In Spain, it appears that the regimen of the lower classes is altogether vegetable; onions or fruits being the only accompaniments to their bread; and these people are remarkable for their power of sustaining severe physical exertion.

"Those who have penetrated into Spain, have probably witnessed to what a distance a Spanish attendant will accompany on foot a traveller's mule or carriage;



not less than forty or fifty miles a-day;—raw onions and bread being his only fare. ‘With respect to the Moorish porters in Spain,’ says Capt. C. F. Chase, of Providence, R. I., ‘I have witnessed the exceedingly large loads they are in the habit of carrying; and have been struck with astonishment at their muscular powers. Others of the labouring class, particularly those who are in the habit of working on board of ships, and called, in that country, *stevedores*, are also very powerful men. I have seen two of these men stow off a full cargo of brandy and wine in casks, after it was hoisted on board and lowered into the hold, apparently with as much ease as two American sailors would stow away a cargo of beef and pork. They brought their food on board with them; which consisted of coarse, brown wheat-bread and grapes.’” (pp. 165-6.)

The following statements, in regard to the diet of the labouring classes among the Greeks, and the physical power and endurance which they display, are to the same effect:

“‘The Greek boat-men,’ says the venerable Judge Woodruff, of Connecticut, who went out as the agent of the New York Committee for the relief of the Greeks, ‘are seen in great numbers about the harbours, seeking employment with their boats. They are exceedingly abstemious. Their food always consists of a small quantity of black bread, made of unbolted rye or wheat-meal, generally rye; and a bunch of grapes or raisins, or some figs. They are, nevertheless, astonishingly athletic and powerful, and the most nimble, active, graceful, cheerful, and even merry people in the world. At all hours they are singing;—blithesome, jovial, and full of hilarity. The labourers in the ship-yards live in the same simple and abstemious manner, and are equally vigorous, active, and cheerful. They breakfast and dine on a small quantity of their coarse bread, and figs, grapes, or raisins. Their supper, if they take any, is still lighter,—though they more frequently take no supper, and eat nothing from dinner to breakfast. It is, indeed, astonishing to an American, to see on how small a quantity of food these people subsist. It is my serious opinion, that one hearty man in New England ordinarily consumes as much food, in a day, as a family of six Greeks. Yet there are no people in the world more athletic, active, supple, graceful, and cheerful. In Smyrna, where there are no carts or wheel-carriages, the carrying business falls upon the shoulders of the porters, who are seen in great numbers about the wharves and docks, and in the streets near the water-side, where they are employed in lading and unlading vessels. They are stout, robust men, of great muscular strength, and carry at one load, upon a pad fitted to their backs, from four hundred to eight hundred pounds. Mr. Langdon, an American merchant, residing there, pointed me to one of them in his service, and assured me that, a short time before, he carried, at one load, from his warehouse to the wharf, about twenty-five rods, a box of sugar weighing four hundred pounds, and two sacks of coffee weighing each two hundred pounds—making, in all, eight hundred pounds; that, after walking off a few rods with a quick step, he stopped, and requested that another sack of coffee might be added to his load; but Mr. Langdon, apprehending danger from so great an exertion, refused his request.’ Lieut. Amasa Paine informs us, that one of these porters carried a load, weighing nine hundred and sixty pounds. Mr. Luther Jewett, of Portland, Maine, says, that one of his schooners came into Portland, laden with *barilla*, from the Canary Islands; and that he stood by while the schooner was discharging its cargo, and saw four stout American labourers attempt, in vain, to lift one of the masses of *barilla*, which the captain and mate both solemnly affirmed was brought from the storehouse to the vessel by a single man,—a native labourer where they freighted; and he subsisted entirely on coarse vegetable food and fruit.” (pp. 166-7.)

Among the Japanese, there is a religious interdict, not merely against animal flesh, but also against milk and its productions. Their staple food is rice, to which they give flavour and variety with pulse, fruits, roots, and herbs; yet they are represented by such travellers as have visited them, as robust, well-made, and active, and also as remarkably healthy, long-lived,

and intelligent. The diet of the lower classes among the Chinese, also, appears to be entirely vegetable; rice being their staple support, and fruits and confections being used to flavour it. Many of them, from chewing opium and other pernicious habits, are in a state of great wretchedness; but those who get enough to eat, and do not drink spirits, or chew or smoke opium, are possessed of great strength and agility:

“‘A finer-shaped and more powerful race of men exists nowhere,’ says Sir John Davis, the present Governor of Hong Kong, ‘than the coolies or porters of Canton; and the weight they carry with ease, on a bamboo between two of them, would break down most others. The freedom of their dress gives a development to their limbs, that renders many of the Chinese models for the sculptor.’ Gutzlaff says, that, on a certain occasion, ‘not being able to walk, we procured sedan-chairs. The bearers appeared to be the lowest of the low,—clad in a few rags, and looking as emaciated as if they were going to fall down dead. But under this unseemly exterior they hid great strength. I certainly believe, that a well-fed horse would not have been able to carry some of us, who were stout and hale, over the cragged mountains, without sinking under the load. But these men walk on briskly and sure-footed, and ascend acclivities with greater speed than we could have done in walking. Yet, though these men were meagre, and hungry as wolves, they were cheerful and boisterous. Of the scanty livelihood upon which the poorer classes, and, indeed, nine tenths of the nation, are obliged to subsist, those who have not witnessed the reality can hardly have an adequate idea. The wages are so low, that a man who has worked hard from morning till evening receives, perhaps, ten cents; and with this he has to maintain wife and children.” (pp. 169-70.)

We purposely select examples from all the principal families of mankind, to show that this is not, as some have supposed, a matter of *race*; but that, in every great group of nations, there are large bodies of men, to whom a purely vegetable regimen is habitual,—these consisting of the very individuals by whom the *work* of the community is chiefly done. In India, as every one knows, a large proportion of the native population subsists upon a vegetable diet; and here the general order of things is reversed, for it is among the higher classes that the abstinence from flesh is universal, and among the lower that the greatest consumption of meat takes place. Whatever may be their mental capacity, there can be no doubt that the physical conformation of the high-caste Hindoos is as perfect as that of any nation in the world:

“Men of six feet stature, and with well-proportioned, symmetrical, vigorous, and active bodies, are by no means uncommon; and, for natural ease, grace, and urbanity, this class of Asiatics are exceeded by no people in the world.”

The powers of endurance of the Indian vegetarian are severely tried in the performance of the postal service; which is (or rather, we believe, was) accomplished entirely by a certain caste of Hindoos, called Pattamars, whose sole occupation is to carry letters and despatches by land. They travel from sixty to seventy miles a day; and their journeys (as that from Calcutta to Bombay) sometimes occupy as much as twenty-five days. These men are generally tall and vigorous; yet their diet is said to consist entirely of boiled rice.

If we turn to the African races, we find the same rule to hold good. The labouring classes in Egypt subsist upon bread made of wheat, millet, or maize, together with various fruits, and other vegetable productions; they rarely taste fish, and still more rarely eat meat; and they can get but little either of milk or of its preparations.

"It is indeed surprising," says Mr. Lane, "to observe how simple and poor is the diet of the Egyptian peasantry, and yet how robust and healthy most of them are, and how severe is the labour which they can undergo. The boatmen of the Nile are mostly strong, muscular men. They undergo severe labour in rowing, poling, and towing; but are very cheerful."

And Mr. Catherwood, an equally unexceptionable witness, says of them :

"The Egyptian cultivators of the soil, who live on coarse wheaten bread, Indian bread, lentils, and other productions of the vegetable kingdom, are among the finest people I saw."

Of the natives of Central Africa, it is stated by the Landers, that, though their diet is what we should term poor and watery—consisting chiefly of preparations of the yam and of Indian corn—yet a stronger or more athletic race of people is nowhere to be met with :

"Burdens with them are invariably carried upon the head; and it not unfrequently requires the united strength of three men to lift a calabash of goods from the ground to the shoulders of one; and then, and not till then, does the amazing strength of the African appear. Some of the women that we saw bore burthens on their heads that would tire a mule; and children, not more than five or six years old, trudged after them with loads that would give a full-grown person in Europe a brain-fever." (p. 171.)

The Kroomen of the coast near Cape Palmas, who are much employed on board European ships of war and merchantmen, to execute duties in which considerable fatigue and exposure to the sun are to be borne, live upon yams with a little palm-oil; and are a remarkably hardy and vigorous race. When transported by enslavement into other parts of the world, many of the native Africans continue, from choice, to subsist on their original regimen, and show no degeneracy in physical power. Thus, the slaves of Brazil are a very strong and robust class of men, living on rice, fruits, coarse bread, and farrenia roots; they are generally very strong, and carry enormous burthens on their heads; and many of them are tall, athletic, and very well proportioned. They are generally very healthy, and are said to be particularly free from fever; this last, however, seems to be rather a matter of *race* than of *regimen*.

If we examine the diet of the native races of the New Continent, which still retain their original habits, we shall find that among some of those tribes which are most remarkable for their powers of labour and endurance, the diet is most purely vegetable; although it must be freely admitted, that the opposite practice is found to be equally compatible with health and vigour in the experience of others. We are not at present, however, attempting to test the *relative* merits of the two systems; all we are now concerned with is the evidence of the adequacy of a Vegetable regimen to keep the system in good *working* order. The following extracts afford satisfactory proof to this effect :

"The principal article of food among the Indians of Mexico, and more particularly in the state of Tobasco," says Mr. Pope, who resided several years among them, "is Indian corn. It consequently constitutes the most important article of agriculture; and three crops may be obtained in a year, without the labour of tillage. From the corn they prepare a thin cake called the 'tortilla;' which is a bread universally used by the better class of the inhabitants, and a dough from which is made what they call 'posol:' the latter article is prepared by boiling the corn, and afterwards crushing it on a flat stone fitted for the purpose, and which every family possesses;—it being substituted for grinding, as corn-mills are unknown in the

country. This dough is laid aside until wanted for use, and in a short time becomes sour; in which condition it is generally preferred. It is then mixed with water, to such a consistency as may be drunk; and sometimes a little sugar is added. On this food alone they are enabled to subsist and undergo far more fatigue, under the tropical sun of Mexico, than our northern labourers in the northern latitudes, with the free use of animal food. I have not unfrequently been forty hours in ascending the Tobasco river, to the capital,—a distance of about seventy-five miles,—in one of their canoes, against a current of from three to four miles an hour;—the men poling the canoe (a very laborious employment) sixteen hours out of the twenty-four. Those who abstain from the use of ardent spirit are muscular and strong; and among them are to be found models for the sculptor.

"Sir Francis Head informs us, that immense loads are carried by the South American miners, though fed entirely on grain and pulse. 'It is usual for the copper-miners of Central Chili to carry loads of ore of 200 lbs. weight up eighty perpendicular yards twelve times a day. When they reach the mouth of the pit they are in a state of apparent fearful exhaustion, covered with perspiration, their chests heaving, yet after briefly resting they descend again. Their diet is entirely vegetable: breakfast consists of sixteen figs and two small loaves of bread; dinner, boiled beans; supper, roasted wheat grain. They scarcely ever taste meat; yet on this simple diet they perform a labour that would almost kill many men.'"

Among the Spanish population of South America, instances present themselves which are not less remarkable:

"The Spaniards of Rio Salada, in South America,—who come down from the interior, and are employed in transporting goods overland,—live wholly on vegetable food. They are large, very robust, and strong: and bear prodigious burdens on their backs,—such as require three or four men to place upon them,—in knapsacks made of green hides: and these enormous burdens they will carry fifty miles into the country;—travelling over mountains too steep for loaded mules to ascend, and with a speed which few of the generality of men can equal without any incumbrance." (pp. 172-3.)

"In 1823, General Valdez, a Peruvian general, led his army from near Lima, to the southward of Arequipa,—a distance of two hundred and fifty leagues, or seven hundred and fifty miles,—in eleven days; or more than sixty-eight miles a day, for eleven days in succession; and, at the close of this forced march, met and routed the patriot army, which was much larger than his own. During this march, the soldiers subsisted on the parched corn which they took in their pockets. 'These Peruvians,' says a highly intelligent gentleman, who has spent twenty years among them, 'are a more hardy race, and will endure more fatigue and privation, than any other people in the world. They subsist wholly on vegetable food; and, being very improvident, their diet is generally coarse and scanty. Parched corn is their principal, and generally their exclusive article of food, when engaged in any particular enterprise or effort which requires great activity and power of body: at other times, they subsist on such of the various products of their climate, as they happen to have at hand. In travelling, and in many other respects, the women are quite equal to the men in muscular power and agility.'" (p. 174.)

Of the Polynesian races in general, it may be said, that previously to the introduction of domesticated animals of Europe, they were almost purely vegetarian in their regimen. Cannibalism was confined to a few among them, and human flesh was rather an occasional dainty than a staple article of diet. Of any other flesh they had little enough, and fish was more consumed by the chiefs than by the people in general. Yet all voyagers agree in describing the inhabitants of several of the Polynesian islands as among the finest specimens of physical development that any part of the world could furnish:

"Judging from the accounts of all navigators who have visited the Friendly and Society Isles, I am inclined to think," says a recent voyager, "that the people of the Marquesas and Washington Islands, excel in beauty and grandeur of form,—in regularity of features, and of colour, all the other South Sea Islanders. The men are almost all tall, robust, and well made. We did not see a single cripple, nor deformed person; but such general beauty and regularity of form, that it greatly excited our astonishment. Many of them might very well have been placed by the side of the most celebrated master-pieces of antiquity, and would have lost nothing by the comparison. One man (a native of Nukahiva) whom we carefully measured, corresponded perfectly, in every part, with the Apollo Belvidere. The food of these people consists of bread-fruit, cocoa-nuts, bananas, yams, batatas, &c. and mostly in a natural state." (p. 252.)

"The interesting natives of Pitcairn's Island, who sprang from the mutineers of his Britannic Majesty's ship *Bounty*, strikingly illustrate the principles before us:—Yams constitute their principal food;—either boiled, baked, or mixed with cocoa-nut made into cakes, and eaten with molasses extracted from the taro-root. Taro-root is no bad substitute for bread; and bananas, plantains, and *appoi*, are wholesome and nutritive fruits. The common beverage is water; but they make a tea from the tea-plant, flavoured with ginger, and sweetened with the juice of the sugar-cane. They but seldom kill a pig;—living mostly on fruit and vegetables. With this simple diet, early rising, and taking a great deal of exercise, they are subject to few diseases; and Captain Beechey says, they are certainly a finer and more athletic race, than is usually found among the families of mankind. The young men, all born on this island, were finely formed, athletic, and handsome; their countenances open and pleasing; indicating unruffled good humour. Their teeth are described as beautifully white,—like the finest ivory; and perfectly regular, without a single exception." (pp. 252-3.)

The following account of the Pitcairn islanders, recently given (on the authority of Lieut. Wood, Commander of the *Pandora*) in a letter from the Rev. W. Armstrong, Chaplain at Valparaiso, to the Secretary of the Christian Knowledge Society, fully confirms the preceding statements as to their great muscular vigour. "They are described as a remarkable strong and healthy people; for instance, a young woman, eighteen years of age, being accustomed to carry on her shoulders a hundred pounds' weight of yams, over hills and precipitous places, and for a considerable distance, where one, unaccustomed to such exercise, would hardly be able to scramble. And a man of sixty years old with ease carried the surgeon of the *Pandora* up a steep ascent, from the landing place, that he had himself in vain attempted to mount, the ground being very slippery from recent rains; and the officer being a large man, six feet high, renders it the more surprising. Indeed Lieut. Wood said he was himself borne aloft in the arms of a damsel, and carried up the hill with the utmost facility."

In concluding, then, that the Vegetable kingdom is perfectly capable of supplying the necessary wants of Man, under all ordinary circumstances; and that, in particular, it is quite adequate to the production of an amount of physical force which can probably not be permanently surpassed on any other dietetic system, we consider that the advocates of Vegetarianism have a wide and secure basis of experience, such as can scarcely be shaken by any negative testimony,—certainly not by the fullest proof of the unsuitableness of a vegetable regimen to *individuals*. It is altogether a different question, however, whether a purely vegetable regimen is *more* favorable to health, vigour, and endurance, than a purely animal or a mixed diet. That a purely animal regimen does not of itself produce nervo-muscular

power, or superior bodily development, is sufficiently proved by the inferior *physique* of some of the most purely kreatorphagous people on the face of the globe; namely, the inhabitants of the northern coast of Asia, the Samoiedes, Ostiaks, Tungoosees, and Kamschatdales, with the Laplanders at the European extreme, and the Esquimaux by whom this race is continued into North America,—these nations being among the smallest, weakest, and least courageous people in the world. But that, on the other hand, an almost purely animal diet is consistent, under certain conditions, with the highest development of the physical powers, is shown by numerous examples of which we do not find that the vegetarian advocates make any mention. Thus the experience of the North American Indians, whose staple article of food (until they began to change their habits under European influence) was deer's flesh, may be set against that of their Southern brethren; and the Guachos of South America, who pass their whole day in the saddle, and scarcely ever taste anything but beef, may be fairly matched for powers of endurance against the Hindoo postmen. It is only, however, when a life of extraordinary exertion is being led, when, in fact, the human being is in a state of constant exercise, not unlike the restless activity of a carnivorous animal, that a purely animal diet can be at all favorable to vigour; but that it is then quite compatible with health, and with the highest production of physical power, seems to us to be taught by experience, of quite as satisfactory a kind as that which has established the powers of vegetable food. Further, that under ordinary circumstances, the ordinary mixed diet which is adopted by all in this country who are able to get it, provided that they have not embraced the vegetarian doctrines, is at least as favorable to muscular vigour as that consisting of vegetable food alone, has never been disproved; and here, too, we must take the teachings of a wide experience, against the assertions of individuals that they have improved in vigour after giving up animal food. The only reference to this point that we find in Mr. Smith's treatise, is contained in the following paragraph:

"Mr. Brindley, a celebrated canal engineer in this country, informs us, that in the various works in which he has been engaged,—where the workmen, being paid by the piece, each exerted himself to earn as much as possible,—men from the north of Lancashire and Yorkshire, who adhered to their customary diet of oat cake and hasty pudding, with water for their drink, sustained more labour and made greater wages, than those who lived on bread, cheese, bacon, and beer,—the general diet of labourers in the South. I am aware that statements have been made which seem to contradict what Mr. Brindley here affirms to be true. It has been said that Frenchmen, when employed in the forming of canals and railroads, have not been able to compete at all with the English, till they have begun to eat animal food and to drink beer. Perhaps the work was at first new to them; and habit, rather than food, might effect the change. It is not denied, however, that a more stimulating diet might excite them to more strenuous exertions; and as they became more accustomed to their work, they would perform it with much greater ease; and although a man on vegetable diet might not accomplish so much in a short space of time as a man living on more exciting food; it is nevertheless certain, that if their constitutional stamina be equal, the former will bear a continuance of labour much longer than the latter; and, by his steady and unremitting exertions, will in the end perform a much greater amount of work." (p. 176.)

We have been informed on good authority, that on some recent occasions on which Scotch "navvies" were pitted against English, in the

construction of railways in the north of England, the oat cake carried the day in the long run against the bread and beef, although for a single effort the latter diet appeared superior. No such comparisons, however, can be worth anything, unless made upon bodies of men whose habits and constitutions are as nearly as possible the same; and when so much depends upon "race," and upon habits which have been so early acquired as to have become constitutional, as well as upon individual differences when small numbers of men are pitted against each other, it would be just as safe to conclude that the Scotchman beat the Englishman because he was a Scotchman, as because he fed off oat cake. The case of the Frenchmen, however, is different, because we have them pitted, not so much against Englishmen, as against themselves. Mr. Smith is obliged to admit, that their more generous diet was favorable to the performance of a greater amount of labour; but he appears to us to go far beyond what any evidence yet collected will justify, in the assertion with which the paragraph closes. We do not dispute this assertion; on the contrary, we are inclined to believe, that a judiciously chosen vegetable regimen may be conducive to the greatest *endurance* of physical exertion; but we do not find here or elsewhere, either in Mr. Smith's book or in Mr. Graham's lectures, any adequate proof of it. Some *individual* cases of remarkable strength among vegetarians are cited; but these do not prove the superiority of the vegetable regimen over the mixed, any more than the fact that men who have drunk freely all their lives have occasionally attained to a good old age, proves that intemperance is favorable to longevity.

Here, for the present, we must quit the subject. We purpose in a future article to consider the relation of the different systems of diet to varieties in climate and exposure; to inquire which of them is most favorable to mental activity and vigour, and whether there is any ground for the assertion that the moral character is likely to be improved by abstinence from flesh-meat; and to investigate the influence of diet on the prevention and cure of disease.

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#### ART. VI.

*A Treatise on the Inflammations of the Eyeball.* By ARTHUR JACOB, M.D.  
—Dublin, 1849. 12mo, pp. 344.

THE treatise here presented to us was originally published, in the shape of detached essays, in the 'Dublin Medical Press.' As we learn, however, from the author's preface, his object was not to supply a mere ephemeral sketch of extant information on the subject, but to provide a digested summary for the permanent use of students and practitioners. The statements and observations which Dr. Jacob has made on his own authority, are the result of a very long practice in this department of surgery. He offers the work as an introduction to the study of the diseases of the eye, believing that inflammation and its consequences constitute the larger proportion of these maladies; and he addresses it more to physicians and surgeons in general practice, and to students, than to oculists, because he thinks that the study of these diseases should be restored to its original conspicuous place amongst the most favoured topics of medical instruction, rather than abandoned to a more limited cultivation. In these views,

we cordially agree; and are very glad that he has so earnestly and satisfactorily enunciated them.

Dr. Jacob confines his attention to the inflammations of the eyeball, exclusive of the conjunctiva. The first half of the volume is devoted to the varieties of what is generally called *iritis*; then follows inflammation of the cornea, the aqueous chambers, the retina, the choroid, and the crystalline; while the whole is brought up with the consideration of traumatic, sympathetic, phlebotic, and neuralgic ophthalmiæ.

A point at which Dr. Jacob labours throughout the volume, is the neglect which (as he alleges) is generally shown to the inflamed state of the retina, in the consideration of the various species of *iritis*. His views on this subject will be understood from the following passages:

"Inflammation of the eye is now very generally called *iritis*, and the invention or application of this term has been looked upon as an improvement in the nomenclature of disease, if not somewhat of a discovery or novelty: yet no one, I believe, ever saw the iris inflamed without corresponding inflammation of the sclerotic, and seldom without inflammation of other parts of the organ. I am also sure that no one ever saw inflammation of the iris proceed unchecked until it terminated in blindness, who did not at the same time see that the disease had extended to the retina, the crystalline lens and its capsule, the membrane lining the chamber of the aqueous humour, and probably to the choroid and vitreous humour. I do not mean to say that what is called *iritis*, when it terminates in blindness, is always accompanied or followed by opacity of the lens, or disorganization of the vitreous humour; because we often in such cases see the lens remaining transparent, and find the vitreous humour firm; but I do mean to say that the consequences of *iritis* are rarely, if ever, confined to that particular part, but extend to one or more of the other textures. I would, therefore, with the greatest respect for the opinions of eminent men who have written on this subject, suggest that the term should be abandoned as mischievous and calculated to lead inexperienced persons astray, by fastening their attention on the iris when the retina should be the prominent object." (pp. 1-2.)

"The functions of the retina are so frequently impaired from *iritis*, both during the existence of inflammation and after it subsides, that the extension of the inflammation from one to the other, or their coexistence in both, must be admitted; and therefore the general adoption of the term *iritis*, and its frequent application to inflammation of all the contents of the eyeball, has proved most injurious, by directing the attention of the practitioner to the state of the iris exclusively, and placing out of view the retina, which, from its greater delicacy, and from its integrity being so much more essential to vision, should be an object of greater anxiety. It is not denied that *iritis* does occur without corresponding inflammation of the retina; but it is certain that they are both frequently engaged, and that the disease, in such cases, is general inflammation of the eyeball, or internal ophthalmia." (p. 224.)

While we cannot concede that there is any novelty in the statement, that the choroid and the retina are generally involved in that inflamed state of the eye which is called *iritis*, yet the practical importance of attending to the retina has, undoubtedly, been too slightly dwelt on by systematic writers. We do not wonder, then, at the earnestness and frequency with which the doctrine is insisted on by Dr. Jacob:

"The first step towards the successful treatment of inflammation of the eyeball is to become convinced that it is not a mere *iritis*, but a general inflammation of the whole organ, terminating, if unchecked, in its destruction, and consequently in blindness." (p. 3.)



The following observation, regarding the result of operations for artificial pupil after iritis, is just and important :

"I find it urged as an argument in favour of the view that the iris undergoes inflammation without corresponding inflammation of the other parts of the eye, that when vision is lost by closure of the pupil in iritis, the formation of an artificial pupil will often restore it ; but my experience leads me to a different conclusion, for, as I shall have to state hereafter, I find that the operation for artificial pupil in such cases is generally useless, because the so-called iritis has been accompanied by inflammation of the retina terminating in blindness or amaurosis." (pp. 2-3.)

We cannot, however, subscribe to a remark of an opposite sort, which Dr. Jacob makes, regarding sympathetic ophthalmitis and its results. He says :

"Although the prognosis in cases of sympathetic inflammation of the eye must be unfavorable, the surgeon should not despair of success if well-directed efforts to save the organ be steadily continued. It is also to be borne in mind respecting it, that notwithstanding its destructive effects on the iris and lens, the retina escapes destruction in its progress more frequently, perhaps, than in other inflammations of the eyeball, unless the disease causes total disorganization, with irregularity in form, of the entire globe. I operate for artificial pupil on eyes which have suffered from severe injury, or this sympathetic inflammation, with more confidence than upon those having the pupil closed by the other species of disease." (pp. 305-6.)

We consider it certain, that inflammation of the retina is one of the earliest and one of the most serious parts of sympathetic ophthalmitis ; and as for artificial pupil, it has never succeeded in our hands on the eye sympathetically affected, and very rarely on the one originally injured. Both eyes, but especially that sympathetically inflamed, are generally left in an amaurotic condition, altogether past relief.

The cause of the defect or loss of vision, which occurs in every form of iritis, or inflammation of the eyeball, as Dr. Jacob prefers to term the disease, he discusses at considerable length :

"To what this rapid diminution of sight is owing seems doubtful, for it often, if not generally, in syphilitic inflammation at least, returns as rapidly, and in a short time is as perfect as before. It can scarcely be from the inflammation attacking the retina to such a degree as to suspend its sensibility to light altogether, because in that case we could not expect to find it recover so completely and so quickly as it often does. A texture of such exquisite delicacy of organization could scarcely escape injury if subjected to such destructive influence. I am more inclined to attribute the sudden but temporary loss of vision to the increased vascularity of the choroid upon which the retina is expanded." (pp. 17-18.)

Upon this we would remark, that were the cause of the defective vision, under the circumstances in question, an increased vascularity of the choroid, photopsia would, in all probability, be a prominent symptom, from the pressure of the gorged vessels of the choroid on the retina. This is notoriously not the case. Photopsia is rarely complained of by those labouring under iritis. We are, therefore, more inclined to attribute the obscurity of vision to two other causes, both of which are signalled by Dr. Jacob. The one is inflammation of the retina, attended with an effusion of lymph on its concave surface, and the other is the evidently opaque, or semi-opaque, condition of the anterior capsule. If the observer, in an early period of iritis, before the pupil has become much contracted, but when the patient complains of seeing objects as if through a thick fog,

will examine the eye catoptrically, he will find the inverted image almost or altogether invisible, owing, no doubt, to the effusion of lymph over the surface of the capsule. As soon as the appropriate depletory and sorbefacient means take effect, the inverted image again becomes distinct. With regard to the retinal part of the disease, we concur in the following remarks of our author, as of the highest importance :

"It should not be forgotten, that inflammation of the eyeball sometimes commences with defective vision only, and that very serious loss of sight takes place before any redness of the sclerotic or discoloration of the iris is perceived. When this occurs the inflammation is said to commence with amaurotic symptoms, and such are sometimes described as cases of amaurosis, but they are evidently cases of inflammation of the eye, commencing in the retina or choroid, and extending ultimately, if unchecked, to all the other parts, as I shall endeavour to explain when considering amaurosis and retinitis. In this stage of the inquiry, it is not, however, premature to warn the practitioner, especially in dispensary practice in Ireland, to exercise the greatest vigilance when applied to by persons complaining of defective vision suddenly or recently perceived ; much valuable time being often lost from a generally received opinion, that such defect may be caused by some derangement of the stomach or other organs." (p. 18.)

Dr. Jacob speaks rather strangely, we think, of irregularities of the pupil, when he says :

"Adhesion to the capsule of the lens is the consequence, if not the cause, of these irregularities." (p. 11.)

Certainly, in our apprehension, adhesion is the cause. Again, when he says :

"I would also advise practitioners to place little reliance on the assumed fact, that these adhesions take place from an effusion of lymph, or that any such effusion can be seen in such cases ; because it is by no means proved, that what is called coagulable lymph is essentially necessary to form these adhesions, and if it is necessary or does exist, it is in such small quantity that it cannot be seen. Serous membranes when inflamed, if in contact for any considerable length of time, adhere together without any visible layer of lymph being deposited between them." (p. 12.)

This doctrine of adhesion is quite at variance with that which is universally received ; namely, that in all cases where two naturally separate tissues are united by inflammation, a material susceptible of organization is effused, and becomes the connecting medium, and that this material is coagulable lymph.

"A small, white, defined patch," says Dr. Jacob, "is seen at the point where the margin of the pupil adheres, corresponding exactly to the point of adhesion and confined to that spot, and not spreading to the centre, which remains perfectly transparent. There are generally two or three of these opaque specks, corresponding to the same number of points of adhesion. In other cases, the whole margin of the pupil adheres to the capsule, and at the place of adhesion a dense, distinct white circle corresponds to the edge of attachment, leaving the centre free and transparent. These opacities have been called spurious cataracts, and fibrinous cataracts, and are attributed to depositions of lymph ; but I do not think that the existence of the lymph has been proved, or that coagulable lymph is necessarily the cause of such." (p. 28.)

This denial of fibrinous effusion as the cause of adhesion of the iris, is quite contradictory to the facts ascertained on dissection, as well as at variance with the whole course of the morbid phenomena seen during life. Indeed, in the very next page, Dr. Jacob acknowledges that the colour of

the iris may be altered by effusion of lymph—thus admitting, as a cause of one of the phenomena in iritis, what he had just denied as a cause of another of these phenomena.

Dr. Jacob thus defines the stages of inflammation of the eyeball :

"We should consider the first stage to be confined to that period, in which no change, except increased vascularity and its immediate consequences, tumefaction, pain, loss of transparency, discoloration of the iris, inactivity of the pupil, and defective vision take place. The second stage, if to be defined, should, I think, be restricted to that period during which adhesion of the iris to the capsule of the lens, effusion or secretion of lymph or purulent matter, and disorganization of the lens or retina, causing cataract and amaurosis, ensue. If a third stage be spoken of, it should be limited, I think, to that period in which the effects or consequences of the two preceding stages are conspicuous, such as increased size of blood-vessels, permanent adhesion or closure of the pupil, cataract, amaurosis, alteration in shape of the eyeball, yielding of the sclerotic, and softening of the vitreous humour." (p. 22.)

With regard to the treatment to be followed in these three stages, we must again quote Dr. Jacob's own words :

"The heart's action is to be weakened and the size of the capillary vessels reduced by diminishing the quantity of circulating blood by bleeding; and that weakness of the heart's action and reduction in size of vessels is to be continued by nauseating medicines until the local vascular disturbance is subdued, or until the attempt is found fruitless. The supply of new blood is at the same time to be cut off by removing the contents of the intestine by purgatives and a denial of nutritious food; while the secreting powers of the liver, kidneys, and skin, are to be put in requisition to rid the fluids of pernicious ingredients, should such be present. These are the resources of medicine in the first or acute stage of the disease, but when the inflammation proceeds unchecked other remedies must be employed. In what I consider the second stage the effects or consequences of inflammatory action are to be prevented or arrested, such as adhesion, effusion, opacity, thickening, and complete disorganization, with loss of healthy functions; and this is to be effected by medicinal agents exercising peculiar or specific influence, the nature or direct operation of which we do not well understand; of which mercury, iodine, bark, colchicum, and turpentine, are the principal. In the third stage, when the inflammatory action has entirely subsided, and its more permanent consequences only remain, the efforts of the practitioner are directed, often in vain, to effect the stretching of adhesions, the restoration of transparency, and the revival of sensibility and muscular contractility, by the application of belladonna, and the internal administration of mercury or iodine in smaller doses and for longer periods, with local stimulation and cutaneous irritation." (pp. 34-5.)

Now, we cannot admit the propriety of deferring the use of mercury till the period arrives of adhesion, effusion, and loss of transparency. We should commence the exhibition of mercury immediately after the use of bloodletting in the first stage, and thereby endeavour to ward off the very pernicious changes which characterise the second—thus following up the wise injunction of the poet :

"Venienti occurrere morbo."

Let effusion, adhesion, and loss of transparency set in, and it may be impossible to remove them. Bring the constitution under the influence of mercury, and, as Beer pointed out, these changes may be prevented. To use Dr. Jacob's own words :

"Of all the injurious effects or consequences of iritis, the most frequent is adhesion of the iris at the margin of the pupil to the capsule of the crystalline lens; and to prevent this every effort should be made." (p. 51.)

Dr. Jacob's directions (p. 51) for the application of the extract of belladonna, with the view of counteracting the tendency to closure of the pupil in iritis, are remarkably good. His assertion (p. 12), "that the margin of the pupil, in a young and sound eye at least, is actually in contact with the capsule of the crystalline lens, and that, if the membrane of the aqueous humour be inflamed, adhesion must, therefore, inevitably take place unless the parts can be kept separate," he admits, "is not generally believed." He says, he "endeavoured to convince anatomists and others that it is so," in a paper in the '*Medico-Chirurgical Transactions.*' On turning to the paper in question, we do not find the subject touched on.

One or two other anatomical statements of Dr. Jacob we consider hardly borne out by any investigations of which we are aware.

For instance, he states respecting the iris, that "there is not, perhaps, in the whole body, a part which receives so great a quantity of nerves in proportion to its size." (p. 19.)

Zinn's ever-copied figure (tab. iv, fig. 1) has been a great source of the notion, so commonly entertained, respecting the rich distribution of the ciliary nerves to the iris. The figure is a gross exaggeration, and Zinn's statement (p. 191), that the nerves form the whitish radiating fibres seen on the anterior surface of the iris, is an entire mistake. Even the mind's eye of Valentin we suspect to have been helped by Zinn's figure, when he wrote his description of the nerves of the iris. The *orbiculus ciliaris* of Zinn, the ciliary muscle of the present day, is the part which receives and retains the chief portion of the ciliary nerves.

Dr. Jacob makes repeated references to what he terms the "membrane of the aqueous humour," which he evidently regards as a serous membrane, forming a short sac, and differing from the pleura or peritoneum chiefly in this respect, that "in the serous cavities, merely as much fluid as moistens the surface is poured out, while in the chamber of the aqueous humour sufficient to distend the cavity is secreted." (p. 188.)

"Analogy also favours," says he, "the inference that the whole cavity of the chamber must be lined by serous membrane, inasmuch as all structures, of whatsoever nature they may be, in the serous or synovial cavities, are so covered or lined." (p. 189.)

We do not require to inform our readers, that the several surfaces forming the parietes of the aqueous chamber are anatomically dissimilar, and that, therefore, the idea of their forming a serous sac cannot be entertained. Serous membranes, and also synovial ones, invariably present a tessellated epithelium; but there is none on the anterior surface of the iris, nor on that of the anterior capsule of the lens.

After an interesting account of the inflammation of the eye which follows remittent fever, Dr. Jacob proceeds to syphilitic ophthalmia. The following statement regarding this disease is too remarkable to be overlooked:

"In my own practice, I think I may safely say, that I more frequently meet with the disease unaccompanied by any eruption or sore throat, than otherwise. So much so, that I can only satisfy myself respecting its nature by inquiries as to the previous occurrence of primary sores, and the present existence of nocturnal pains and perspirations, periosteal tenderness, emaciation, and general ill health." (p. 76.)

We suspect that the experience of few practitioners will agree on this point with that of Dr. Jacob. The patient often says, indeed, that he has had no eruption; but if we strip him, in nine cases out of ten of syphilitic iritis, we find the indubitable marks of a cutaneous affection. Perhaps there may not be more than the remains of half a dozen pustules or leprous spots; but how characteristic are those half-dozen spots! Pass the finger over them, and the pit left by the destruction of the rete mucosum is a thing which cannot be mistaken, and which continues for life, although the dingy colour may gradually fade, and the natural hue of the skin be nearly restored.

Speaking of the diagnosis of syphilitic inflammation of the eyeball, Dr. Jacob rests a good deal on the opacity of the membrane of the aqueous humour, by which he appears here to mean the lining membrane of the cornea. A punctuated opacity of the lining membrane of the cornea, similar to what occurs in aquo-capsulitis, is not an unfrequent attendant on syphilitic iritis; but we cannot subscribe to Dr. Jacob's assertion, that "the opacity of the membrane of the aqueous humour is indeed almost exclusively found in syphilitic iritis." (p. 77.)

In gonorrhœal iritis, a disease which Dr. Jacob confesses (p. 99) to have found a rare one, opacity of the lining membrane of the cornea is one of the earliest and most striking symptoms. The opacity, however, is not the punctuated variety, but presents either a diffuse and uniform haziness, or exists in irregular patches plainly caused by effused lymph. Dr. Jacob describes the cornea in this disease as milky throughout its entire breadth, and its surface rough as in corneitis,—a description so totally at variance with what we have observed, that we suspect Dr. Jacob to have mistaken for gonorrhœal iritis some other affection of the eye, possibly following gonorrhœa as an incident only, and not as an effect.

In some remarkable particulars, the eyeball may be compared to the ball of a ball and socket joint. Its motions of ascent and descent, adduction and abduction, circumduction and rotation; the muscles by which these movements are accomplished, their tendinous expansions, and the fasciæ by which they are covered; the smooth, moist, cellular capsule on the concave surface of which the motions are performed; the fibrous periorbita and periosteum which involve the orbit, and are prolonged into the eyelids; all have their counter-parts in the joints. We need not wonder, then, if the diseases of the eyeball and surrounding tissues should bear an analogy to those of the joints. We need not wonder that exposure to cold should excite inflammation in the fibrous tissues of the organ of vision; that, from the numerous nervous branches which radiate out of the orbit to the face, this inflammation should be attended with much pain; that the pain should suffer nocturnal exacerbations; that a quick pulse, buffy blood, loaded tongue, and other signs of fever, should attend the disease; that it should yield to the same remedies as inflammation of a joint, or that it should receive the name of rheumatic ophthalmia.

Those pathologists who believe in the existence of a peculiar *materies morbi* in rheumatism, or, in other words, that rheumatism is a blood-disease, depending, for instance, on the accumulation of lactic acid in the system, may deny the claims of this ophthalmia to the appellation of *rheumatic*—a name which seems to have been bestowed on it more for

the sake of convenience, than from any well-weighed consideration of its pathology.

Some such views as these Dr. Jacob seems to have embraced. He doubts, in the first place, that the disease of the eyeball is rheumatic; secondly, that rheumatism is an inflammation; and thirdly, that the fibrous system is the seat either of the disease called rheumatic ophthalmia, or of rheumatism.

Certainly, in the great majority of cases of what is commonly called rheumatic ophthalmia, there is no reason to suspect the previous presence of a morbid matter infecting the vital fluid; the disease is just local inflammation excited by exposure to cold, and is quite independent of any constitutional affection or diathesis.

"Neither these peculiarities, nor any other modification of symptoms, will, however, in my opinion, justify the practitioner in pronouncing positively that the disease is true rheumatic inflammation of the eye, unless there be unequivocal proof of the previous or present existence of rheumatic constitution or diathesis, as indicated by inflammation of the joints, with the peculiar accompanying fever, or that disturbance of the system called acute rheumatism; or unless there be at least transient shifting pains of joints or muscles with brief febrile paroxysms, perspirations, lithic deposits in the urine, and general ill health." (p. 124.)

Dr. Jacob raises the same objection against the name *catarrho-rheumatic ophthalmia*, which is bestowed on a very peculiar form of inflammation of the eyeball.

Dr. Jacob considers it likely, that a true rheumatic inflammation of the eyeball may exist; and lays down, *à priori*, rules for its treatment. Bleeding, he reasons, will be unsafe and unsatisfactory; purgatives are not to be relied on, except as helping out of the system the pernicious ingredient which lies at the root of the rheumatic diathesis; antimony and Dover's powder may sweat the unwelcome visitor out; while calomel, with opium, is to be recommended, as a means with which to follow up the enemy, already in full retreat before the sudorifics. Some very judicious remarks are made by our author on the use of bark, iodine, and other remedies. We do not find any clear signs laid down, however, by which true rheumatic ophthalmia is to be recognised. All that is said is, that it must be preceded by other rheumatic affections, and depend on the rheumatic diathesis. On this we shall venture a single remark. If Dr. Jacob believes, "that the eye is sometimes attacked by gouty inflammation, even before any joint or other organ has been affected" (p. 139), why should he be so incredulous as to rheumatism affecting the eye in a similar way?

We cannot quit the subject of rheumatic ophthalmia, without observing, that we entertain considerable doubt of the essential correctness of the name *scleritis*, which has sometimes been applied to it. In the disease in question, the deep-seated conjunctival network is injected with blood, but this does not constitute an inflammation of the sclerotica. That network is seated in the prolongation of the orbital capsule, or membrane of Tenon, over the tunica tendinosa; while, as far as we know, the sclerotica is perfectly free from redness. Indeed, the sclerotica is a structure so poor in vessels, that it may be doubted whether it ever becomes red under any circumstances. The dim vision, the discoloured iris, and the sluggish and irregular pupil, which attend rheumatic ophthalmia, show it to be an internal inflammation of the eyeball, merging into idiopathic or rheumatic iritis and retinitis.

All this discussion about rheumatic ophthalmia shows the difficulty of attaining to an essentially correct nomenclature of eye-diseases. To teachers, and especially to clinical teachers, belongs the duty of explaining the conventional meaning of those names which are in common use.

When speaking of the membrane of Descemet, whom he calls Decemet, we do not understand what Dr. Jacob means by saying, that in a paper on the Anatomy of the Eye, in the 'Medico-Chirurgical Transactions,' he "endeavoured to direct attention to it without effect." (p. 185.) The membrane in question has always commanded, since Descemet published on the subject, nearly a century ago, a considerable share of attention.

The cornea is generally recognised as a non-vascular tissue. Dr. Jacob, however, thinks "that this structure presents no exception to the general rule of growth by vascular ramification." (p. 194.) If he means that vessels penetrate into the healthy cornea, we fear he maintains a notion altogether abandoned by recent physiologists. Every part of the human frame is supported, no doubt, through the medium of vascular ramifications. It is not necessary, however, that vessels should penetrate into the parts to be nourished; and, in fact, every part may, in one sense, be styled non-vascular, as the capillaries never penetrate into the proper substance of any tissue, and never terminate by open extremities. Red vessels are seen on the surface of the cornea in cases of chronic inflammation; and Dr. Jacob says, he knows not "how those who maintain that the cornea lives and grows without blood-vessels can well account for this appearance." The fact that the elements of the cellular tissues, such as cartilage and the cornea, do not come into direct contact with the blood-vessels, seems unknown to our author. The tissues in question are surrounded by vessels. Two sets of vessels, a superficial and a deep-seated, surround, for instance, the margin of the cornea. The superficial set belong to the conjunctiva, and are continued over the margin of the cornea to the distance of from one eighth to half a line, and then return as veins. The deep-seated set do not pass into the true cornea, but terminate in dilatations from which veins arise, just where the cornea becomes continuous with the sclerotica. By the fluid which is drawn from these vessels by the cells which enter as component elements into the fabric of the cornea, this structure is nourished. In diseased conditions of the cornea, both sets of vessels extend themselves over and through it.

The diffuse opacity of the cornea resulting from corneitis, Dr. Jacob treats with stimulants:—

"From the variety of stimulants used from time immemorial to remove opacities of the cornea, and the number of them extolled as infallible, it may be presumed that any stimulant will answer the purpose. Solutions of nitrate of silver, sulphate of copper, sulphate of zinc, or the combination called *lapis divinus*, will perhaps answer. I use a solution of iodide of potassium, ten grains to the ounce of water; or, as a substitute for animal bile, said to be effectual, touch the surface with the camel-hair pencil previously dipped in water and brushed two or three times on soap. The fumes of prussic acid, so much vaunted as a quack remedy, I have not used, being dangerous and troublesome. If this nostrum has any influence at all, it is as any other stimulant." (pp. 205-6.)

As to the use of prussic-acid vapour being troublesome, we do not think it half so much so as Dr. Jacob's barbarous brushing of the cornea with soap and water. Neither is it dangerous, if administered with ordinary

care. Certainly, it has a powerful effect in giving a turn to corneitis in the chronic stage, and in promoting the absorption of the diffused opacity in question. Considering the very powerful sedative effect exercised by prussic acid, even when applied locally to the skin, we should doubt that its action on the opaque cornea is that simply of causing the surrounding vessels to fill with blood. Let the reader judge for himself on this point, by putting a little strong prussic acid in a glass tube, the extremities of which are to be kept closed for a minute or two by the application of the finger and thumb. The numbness which he will feel for some time in the parts which had been in contact with the vapour, he will probably refer more to a sedative than a stimulant power.

We are led to quote the following remarks on inflammation of the retina, on account of their importance, and the frequent mistakes which are fallen into in practice regarding this disease:—

“The absence of such vascularity is no evidence of the absence of inflammation of the retina; on the contrary, I wish to impress upon the mind of the practitioner, that inflammation of that structure may and does take place without the slightest appearance of inflammatory action in the sclerotic or iris, and consequently without any external visible symptom of so formidable a disease. This it is necessary to state emphatically, because there is a prevalent belief, that inflammation of the retina may always be easily recognised from external appearances, a belief which has caused loss of sight, I believe I may say, to thousands. This erroneous view, along with the equally erroneous one, that the defect or loss of sight, known by the vague name of amaurosis, is generally caused by derangement of the digestive organs, has, I am convinced, been the cause of more loss of vision than any other established error.

“From what is here stated, it follows that the symptoms of inflammation of the retina are to be sought for elsewhere than in external appearances, and that this most destructive disease may, and often does, take place without any visible change in the exposed parts of the eye. There may, in fact, be most formidable changes in progress in this most delicate structure, while neither the sclerotic and iris, nor the cornea, membrane of the aqueous humour, or crystalline lens, display any alteration. If the practitioner expects to derive information as to the extent or progress of the disease from the amount of pain or intolerance of light, he is equally disappointed. In some cases, perhaps, the proportion of them cannot be correctly stated, both these usual effects of inflammatory action in this organ are present; but in many, if not the majority, they are not to be observed. This should be well understood, and as it is a fact, that inflammation of the retina may take place without redness of the sclerotic or change in the iris, so is it that it may take place without pain or intolerance of light.” (pp. 236-7.)

We must here take leave of Dr. Jacob, whose work we have no hesitation in pronouncing a valuable contribution to ophthalmological literature. It is written, in general, in a lively, and rather combative style, for which we like it all the better; while the subjects discussed, and the views enforced, are of the highest practical interest. We have used, in our remarks upon some of these, the same freedom of criticism which Dr. Jacob has himself exercised towards others; and we trust that he will not regard them as dictated by an unfriendly spirit. On the whole, this little volume appears to us admirably adapted to the objects which the author had in view in producing it; and we strongly recommend its perusal, as calculated to place the inflammations of the eyeball before the mind of the practitioner in a far juster light, than that in which he is led to view them by the mode in which the subject is too frequently treated in ophthalmological works and surgical lectures.



## ART. VII.

1. *Descriptive Catalogue of the Pathological Specimens contained in the Museum of the Royal College of Surgeons of England*. 5 vols. 1846 to 1849. Vol. I: General Pathology.—London, 1846. pp. 144.
2. *Traité d'Anatomie Pathologique Générale*. Par J. CRUVEILHIER, Professeur d'Anatomie Pathologique à la Faculté de Médecine de Paris; Médecin de l'Hôpital de la Charité; Membre de l'Académie Nationale de Médecine; Président Perpétuel de la Société Anatomique, &c. Tome Premier.—Paris, 1849. pp. 743.
- Treatise on General Pathological Anatomy*. By J. CRUVEILHIER, Professor of Pathological Anatomy in the Parisian Faculty of Medicine, &c. &c. Vol. I.—Paris, 1849.
3. *Transactions of the Pathological Society of London*. Vol. I. Including the Reports of the Proceedings for the Sessions of 1846-47, and of 1847-48.—London. pp. 349.
4. *Report of the Proceedings of the Pathological Society of London*. Third Session, 1848-49.—London. pp. 140.

WE congratulate the College of Surgeons, and the profession at large, on the completion of the great work which stands at the head of our list. Seven years of continued labour have enabled Mr. Stanley and Mr. Paget to produce a Catalogue worthy of the collection it describes; not a mere list of the contents of the Museum, but a treatise which stands absolutely unrivalled as an exposition of the present state of pathological knowledge.

It would have been difficult to have named two individuals more eminently calculated for the task intrusted to them by the Council of the College, than the two gentlemen who have been associated in the production of these volumes. The untiring industry, the habits of patient investigation, and above all, the undeviating candour, which have distinguished the long career of Mr. Stanley,—that candour, which has never been known to colour or bend a fact to suit any preconceived theory, joined with the extensive learning of Mr. Paget, whose grasping intellect has sought improvement from every language of modern Europe, and gained for its possessor a reputation abroad, even greater than at home, together with a gift of eloquence that serves to make the dullest subject interesting,—could hardly fail to produce a work worthy of the College, and worthy of the Physiological Catalogue of Professor Owen. It is still higher praise to say,—worthy of that Museum of which Hunter was the founder.

This descriptive Catalogue may be considered to constitute at once the exponent and the illustration of the peculiar doctrines laid down by John Hunter. It affords an explanation of much that without it is unintelligible in his writings; and it shows, more clearly than the closest student of his labours could be prepared to expect, how greatly he was in advance of the times in which he lived, and how just is the homage which posterity has paid, and is still paying, to his unadorned and unfettered intellect.

No one ever entered the lists against a false philosophy with more courage and singleness of purpose than John Hunter; and to no one, with the single exception of Lord Bacon, has there ever been vouchsafed so great

a measure of success. The most distinguishing feature of Hunter's mind was his ardent love of truth, and his never-failing zeal in the pursuit of it. His aim was ever the attainment of some practical end. With him theory was altogether subordinate to fact; and hence it was, that he had rather the reality than the appearance of knowledge. Self-aggrandisement seems never to have entered his thoughts; he engaged in no controversies for the sake of victory; and whilst no one ever broached more novel opinions, none was more careless of his own right to the credit they might bring. Even when his discovery, that gonorrhœa arose from inflammation, and not from ulceration of the urethra, was filched from him, it could provoke no other remark than a mere narrative of the way in which the discovery was made, and the observation: "This new fact being ascertained, it could not escape Mr. Gataker, ever attentive to his emolument, who was then attending Dr. Hunter's lectures, and also practising dissection under me. He published soon after, in 1754, a treatise on this disease, and explained fully, that the matter in a gonorrhœa did not arise from an ulcer, without mentioning how he acquired this knowledge, and it has ever since been adopted in publications on this subject." (Hunter on the Venereal Disease, Works, vol. ii, p. 158.)

His knowledge was rather for futurity than for his own time; even yet the deep meaning of his thoughtful sentences has not been fathomed; nor from the study of his writings merely, could they ever have been understood. In his Museum is to be found the true monument to his fame, the imperishable evidence of his industry and love of truth. It is not too much to say, that up to the present time, the value of this collection has, owing to the want of a guide, been very imperfectly known to all but a select few of the members of our profession. This guide is now supplied, and in a manner worthy of the Museum, and of the distinguished body who are its guardians.

Our readers must bear in mind, that this Catalogue is not confined to the specimens contained in the Hunterian Museum merely, but comprehends the entire morbid collection of the College of Surgeons, which is now twice as large as it was when Mr. Hunter's Museum passed into their hands. In the year 1830, the Council of the College published a Catalogue of the Hunterian Museum, drawn up by Mr. Clift, Sir Everard Home, and Sir Benjamin Brodie, conjointly, from memoranda and descriptions left by Mr. Hunter, chiefly in the handwriting of amanuenses, but bearing evident marks of Mr. Hunter's approbation. The descriptions of the dried specimens were written by Mr. Clift from observation, aided by such notes as were inscribed on them, or on the various drawings. Since that time, there have been added to the collection, by donations and purchases, about 1800 specimens, which, with the Hunterian Museum, constitute an invaluable Pathological Museum of not less than 3500 specimens.

The task of arranging this vast collection, so as to make it easy of reference, and intelligible as a storehouse of pathological facts, can have been no light one; as will readily be acknowledged by any one who has engaged in a similar labour for a much smaller museum, or has even set about cataloguing the library of books which forms part of the treasures of most of our professional brethren. But great as must have been the toil and anxiety expended in this way, Mr. Stanley and Mr. Paget have, in our opinion, executed another labour of equal importance, in affixing to each

class of diseased specimens, a lucid, and in some instances extremely learned dissertation, on the different morbid conditions which they illustrate; and have added to them passages from Mr. Hunter's works, both published and unpublished, which will materially assist the student in appreciating their value. Further, they have collected into a distinct museum, a certain number of specimens illustrating the general principles of pathology, the study of which cannot fail to confirm and advance the knowledge of the most accomplished pathologist. The first volume of the Catalogue is occupied with the contents of this Museum, and to it, for the present, we shall confine our attention.

The specimens in this department are divided into six series, with two appendices, as follows:—

SERIES I. Hypertrophy.

“ II. Atrophy.

“ III. Repair and reproduction.

“ IV. Process and effects of inflammation.

“ V. Mortification.

“ VI. Tumours, and other allied morbid growths.

*Appendix A.* Tumours of uncertain nature.

“ B. Pendulous and loose growths.

Each series is again subdivided into convenient divisions, which will become apparent as we proceed.

This arrangement is the one which has been followed by the Professor of anatomy and surgery to the College, in the successive courses of lectures which for several years past he has delivered in its theatre; and it is that which we believe was adopted chiefly with the view of illustrating and making better known the riches of this pathological collection.

SERIES I contains examples of *hypertrophy*; 1st, by growth; 2dly, by development. A few words will here suffice to introduce and explain these specimens to our readers, the admirable lectures of Mr. Paget on the subject having rendered any detail superfluous.

Hypertrophy or over-nutrition is the first and simplest change to be considered in relation to morbid anatomy; and becomes a subject of pathological investigation, only when it occurs as the result of disease in some part of the body, with which the hypertrophied organ is in close functional relation. Not every enlargement of a part is an example of hypertrophy; but only such as exhibit increased growth of natural textures, or the development of such as already exist, into tissues situated higher up, as it were, in the scale of organization. The difference between these two forms is precisely that of quantity and quality. In the one there is mere increase of bulk by the addition of healthy tissue; in the other, there is not necessarily increase of size, but only an advance in the quality of the newly-formed tissue, which still resembles that which is natural to some other part of the body. Both these kinds of hypertrophy are essentially conservative or reparative changes, the tendency of which is ever towards health, and are regulated by the laws of ordinary nutrition; the purpose of that nutrition being to repair the waste which organs sustain from the lapse of time, and from being called into functional activity. A certain degree of wear and tear is inseparable from animate as well as from inanimate bodies. In living beings, the ordinary and habitual nutrition of the body exactly compensates for the loss that is thus sustained; but increase this loss by

augmenting the function of an organ, and immediately, by virtue of this power inherent in the body, the nutritive energy is increased to such a degree, that within certain limits it exactly makes up for the increased waste. In the Catalogue, this power is called the "reserve power of nutrition," which lies dormant until the necessity for it calls it into action. A simple increase in the quantity of blood flowing through any part will sometimes induce hypertrophy; and, in like manner, it is induced when the function of an organ is impaired, and may be remedied for a time or permanently by the increase of power in another part. One or two examples from the Catalogue will make this clear:

No. 3. "The pelvis and part of the chest, with the urinary and some of the adjacent organs, of an Argus pheasant, (*Phasianus Argus*, Linn.) The left ureter is dilated, and the left kidney is very small, granular, and shrivelled. In compensation, the right kidney is increased to an unusually large size; not by distension, but apparently by growth of its natural texture, so that through it as much urine may have been excreted as through both kidneys in the ordinary state. There is a corresponding difference of size in the renal arteries; the size of each is adapted to that of the organ in which it is distributed." (Hunterian, p. 4.)

This is a very interesting specimen, especially as showing the diminished size of the artery supplying the atrophied kidney, in accordance with that law by virtue of which the heart becomes diminished in size during wasting and emaciating diseases, when the quantity of blood circulating through the system is lessened. Such, for example, is the case in phthisis and cancer.

Specimen No. 5 is also worthy of attention:—

No. 5. "Part of a gall-bladder inverted. It was distended by calculi, of which some small portions are still fixed in its mucous membrane. In consequence of this distension and the obstruction to the passage of bile, slender bundles of muscular fibres have been developed beneath the mucous membrane. The greater part of them have a nearly circular direction; but many, sweeping in various oblique directions, or radiating from certain points, interlace with these circular fibres, and form layers of irregular but close network, like a miniature of that seen on the inner surface of a *columnar* urinary bladder. The mucous membrane is so thin, that the muscular fibres appear as if covered only by a delicate shining epithelium, and are prominent in the cavity of the gall-bladder. The peritoneal coat is thick and soft, as if it had been oedematous, and it seemed like a membrane which has partially recovered from over-distension." (Hunterian, p. 5.)

SERIES II illustrates *atrophy*; first, *by disease*; second, *by degeneration of tissue*. It is well in considering this change, the converse of hypertrophy,—in which a part instead of being increased is diminished in size, and becomes able to discharge only a less amount than usual of its healthy function,—to bear in mind the distinction which was so useful in the consideration of hypertrophy, and to limit the term, in the first place, to cases in which there is mere diminution of size and strength without change of texture; and secondarily, to those cases in which, with or without decrease of size, there is deterioration of tissue, and the substitution of one capable only of lower functions than that which originally existed. Common emaciation, such as follows long illnesses—and that change of texture in which the substitution consists in the development of fibro-cellular or fatty tissue—are the types of these forms of atrophy.

Now, although the conditions under which atrophy occurs cannot be so clearly pointed out as those which give rise to hypertrophy, yet in many

instances the necessary conditions are the simple opposites of those that exist in the latter state.

The want of exercise induces atrophy of muscles; so does a diminished supply of blood. It is naturally brought about in old age by certain defects in the chemical constituents of the blood,—the destruction of the nervous influence of a part may occasion it; finally, it occurs in apparently healthy individuals, from causes of which we are ignorant. It is to be observed, that the condition which gives rise to atrophy does not determine the form in which it will be manifested; for example, among old persons, in whom the nutrition of the entire body is defective, some shrivel up and become very thin without any change of texture, while others actually increase in bulk, by the accumulation of fat in their several organs. Similar phenomena present themselves in paralysis of limbs. Preparations 8 and 9 illustrate the two forms of atrophy:—

No. 8. "The skull of an old edentulous person. Together with a general diminution in the thickness and weight, and probably also in the size, of all the bones of the skull, there is a peculiar atrophy of the jaw-bones and of the parts immediately connected with them, the consequence of the loss of the teeth. The alveolar margins of both jaws are completely absorbed; so that the border of the upper jaw is nearly on a level with the surface of the hard palate, and that of the lower jaw is but just above the mental foramina. Scarcely a trace of a tooth-socket can be seen; the margins of the jaw-bones are rough and hard. The whole texture of both maxillary and palate bones is light, dry, and smooth-surfaced. The posterior half of the hard palate is (except in the median line and at the posterior border), so thinned that it is transparent and flexible, and crackles as it bends. In the lower jaw, the circumference of which is every way more extensive than that of the upper jaw, the condyles are very small and narrow, and their compact layer has been removed; the rami and angles, like all the rest of the bone, are light, thin, and slender; and they present scarcely a mark of the attachment of the masseter, pterygoid, or any other muscles. Among the other bones of the skull, the atrophy is especially shown in the temporal and frontal. In each of the former, the plate forming the glenoid cavity is thin; the eminentia articularis is flattened, and, as it were, expanded into a broad and flat slight elevation; the zygoma is slender, and the marks of insertion of the temporal muscle are slight. In the frontal bone the orbital plates are thin and in parts transparent. The sutures which are usually open are all yet unclosed.

"The lower jaw is fitted with a cross-bar and springs, to show the movements to which it is usually limited when thus changed after the loss of the teeth." (Hunterian, p. 8.)

No. 9. "Parts of the gastrocnemius and of two of the deep posterior muscles of the leg of a young man, who, probably from congenital paralysis of the leg, was lame from his birth, and had long carried his leg in a sling. The muscular tissue cannot be discerned in the gastrocnemius: in its place there is fat with only obscure traces of a linear arrangement of its elements. In the other muscles the same change has taken place; but a few pale muscular fibres remain. The tendons are small, but appear of healthy texture.

"Ulcers, very slow in healing, and leaving imperfect cicatrices, often formed on the leg; and the limb having become useless and inconvenient, was amputated." (pp. 8-9.)

SERIES III is devoted to the *repair* and *reproduction* of lost parts,—a subject on which Mr. Hunter bestowed much attention.

Sub-series A. *Organization of blood in the vessels, or effused.* The first specimen in this compartment is a coagulum in the end of a femoral artery after amputation, which Mr. Hunter believed he had injected. A

long extract from Mr. Hunter's 'Treatise on the Blood,' (Works, vol. iii, p. 118), explains this preparation.

Sub-series B. *Process of healing*. Formation and structure of granulations—granulation independent of suppuration—formation and structure of cicatrices.

Sub-series C. *Reproduction of parts similar to those lost*.

Sub-series D. Union of different parts of the same, or of different bodies—transplantation or grafting of parts.

This series includes the well-known cases, in which Mr. Hunter turned hens into cocks, by transplanting the testicles of the latter into the bellies of the former, and in which he ornamented a cock's comb with a human tooth. The interesting point in these preparations is, that the blood-vessels from the hen's intestine are seen passing to the surface of the testicle in the one case, and from the comb into the pulp of the tooth in the other. There are also other specimens, in which a cock's spur transplanted into the comb has grown to an amazing size.

Sub-series E. *Effects of the continued presence of foreign bodies in the tissues*. This series of preparations is chiefly designed to show the little inflammation which foreign bodies produce when deeply seated; this being often not more than sufficient to set up the adhesive process, by which they become surrounded by a cyst, and remain unaltered for years. Hunter alludes to the fact, that cattle which feed in bleaching fields are seldom killed without their stomachs being found stuck full of pins, which have produced no deleterious effects upon the animal. He remarks, that these pins do not penetrate to the fourth or digesting stomach, and therefore do not occasion that disturbance to the constitution which they otherwise might.

*Preparation 59* shows a maggot (*Estrus Tarandi*) deposited beneath the skin on the back of the reindeer, which has given rise to just sufficient inflammation to form for itself a little sac or chamber in which it resided.

*Prep. 61* shows a similar power in vegetables. The eggs of an insect are deposited in little protuberances on an oak-leaf.

*Prep. 68* is a most remarkable one, being "a hundred needles of various sizes, and most of them entire, which were extracted from the body of a Danish Jewess." A history of the patient from whom these needles were extracted, accompanies the preparation; this history having been furnished by Professor Herholdt of Copenhagen, under whose care the patient fell. The woman seems to have been one of those singular hysterical creatures, occasionally met with, who present phenomena quite removed from the ordinary course of nature, and in whom the affection developed itself in the highest extent. In August 1807, being then fifteen years old, and previously an acute healthy girl, she was seized with an attack of colic and peritonitis, from which she recovered only to become afflicted with the most varied and distressing hysterical symptoms, which continued with scarcely any interruption until the end of the year 1810. She had almost every variety of hysterical manifestation—apparent threatenings of suffocation—sudden faintings—convulsions—cataleptic seizures—attacks of delirium, in which she sang and recited, with singular accuracy, passages from Shakespeare and some of the most esteemed German Poets—long periods of insensibility and heavy sleep, out of which nothing could rouse her,—and prolonged suppression of urine, with

vomiting, and purging of blood and black matter. After 1810, she became free from these symptoms, and remained in good health for a space of eight years. On the 8th of January 1819, however, she was seized by an acute pain in the abdomen, followed in a month by a swelling just below the umbilicus, out of which a sewing-needle was extracted. From this time numerous other swellings formed, out of which similar foreign bodies were extracted in such numbers, that "between the 12th of February, 1819, and the 10th of August, 1820, 273 needles of different sizes were extracted through incisions in various parts of the body, from one to seventeen being extracted at each time, and from one day to five months intervening between the operations. They were all blackened by oxydation, and many were broken. "Her whole abdomen was so marked with scars of the wounds from which the needles had been extracted, that it was like a map."

Subsequently, between May 1821 and July 1822, a hundred more needles were taken from about the shoulder. After this it was discovered, that sundry hysterical phenomena were feigned; she was observed to write with an apparently paralytic arm, and to inject air with an enema-pipe and syringe through the urethra into the bladder; but about the needles there can be no mistake. The history concludes without enlightening us as to the subsequent career of this erratic lady; nor does it inform us whether she had swallowed the needles, or pushed them under the skin; and of course leaves us equally ignorant as to when she had done either or both of these tricks.

SERIES IV illustrates the *process and effects of inflammation*. This series is divided into four. 1st, that which unites parts, the adhesive inflammation; 2d, the suppurative inflammation; 3d, the ulcerative inflammation; 4th, mortification; and they are all designed to illustrate the peculiar doctrines of Mr. Hunter upon these topics.—As we expect that Mr. Paget will give us an early opportunity of taking up the subject of inflammation, we shall not dwell longer upon this part of the catalogue.

SERIES V includes various specimens of *mortification*, or death of parts of the body, from violent mechanical injury, from heat, from cold, from escharotics and chemical action, from obstruction of the larger blood-vessels, and from dry gangrene. One of this series, Prep. 140, which is an example of mortification from the action of an escharotic, is important enough to justify our quoting its description at length.

"*Prep.* 140. A breast, which together with a large carcinomatous tumour in it, separated by sloughing.

"The following history of this case in Sir E. Home's '*Observations on Cancer*,' London, 1805, 8vo, p. 91, Case XXII, was taken from the Hunterian MSS. The original title was 'Case of Cancer of the Breast, where the whole tumour mortified and separated, in consequence of a fever attended and followed by great debility; which shows the theory of the action of arsenic when applied to parts whose powers are small; and also gives a good hint respecting mortification:—

"A lady had for some time been afflicted with a cancer in her breast. The whole breast was involved in this disease. It was become considerably swelled and very firm in texture. Besides the swelling of the breast, there was a fungous tumour, about the size of a large egg, situated between the breast and axilla, in the course of the absorbents; and the glands of the axilla were tumefied. Both the breast and this tumour were ulcerated, discharging a large quantity of thin matter, and often bled, so as to be alarming. In one of these bleedings Mr. Hunter was sent

for, and applied lint dipped in oil of turpentine, which stopped the bleeding for some time. It gave her great pain for the whole day, through the substance of the breast, as also in the axilla, and down the inside of the arm; but the pain went off, and afterwards a little blood only oozed out, to stop which Mr. Ruspini's styptic was got, and applied. Her state of health was very much impaired, and she was expected to lie-in every day. Under these circumstances she was brought to bed, became extremely low, the pulse sunk, and it was hoped, by those who were interested about her, that she might die. When she was in this low state, the breast and the tumour near the axilla began to mortify; and the whole surface, from the axilla nearly to the sternum, appeared like an eschar, the depth of which was not then known. Her health was still bad, but it was on the whole rather getting better. A separation took place round the edges of the dead part, and in a few days the whole dead part, consisting of the breast and tumour, came out, leaving the pectoral muscle quite bare. Her health began to mend, although slowly. Bark, porter (which she became fond of), wine, with nourishing food, were given, and a hope remained that she might recover of her general indisposition. The edges of the sore put on a very suspicious appearance; they became thick and ragged, and rather turning out, attended with extreme tenderness. How far the glands of the armpit had been contaminated, though they did not mortify with the breast and tumour, was not known. While there were appearances of a possibility of a recovery of her general health, the remaining contaminated parts became an object of consideration; but, as it was impossible to ascertain the extent of the disease, and as she was too weak either to bear an operation, or any irritating applications, it was resolved to wait till her strength should recover; but the signs of recovery were not of long standing. Her strength began to fall off, her appetite failed her, the sore became stationary, looked pale, and there was hardly any discharge; nor were any granulations formed. Different parts of the body began to mortify; wherever there was a scratch, or pimple, it ran into a kind of mortification, and soon after she died.

"In this case the fever produced the same effect as the preparations of arsenic used for the extirpation of cancerous breasts; but, unfortunately, did not remove all the contaminated parts, only those which were in a diseased state, and whose vital powers were so much weakened by the action of disease as to make them more readily mortify than the other parts. It is upon this principle that the arsenic acts; and this case illustrates very clearly in what way also it too often fails of entirely removing the disease." (pp. 58-9.)

The specimens illustrating specific diseases are not placed here, but with the other diseases of the same parts in the appropriate portions of the museum; but a list of references renders their access easy. They include the following: rheumatism, gout, syphilis, scrofula, glanders, typhus, scarlatina, and smallpox.

A distinction is made between such specimens of scrofulous disease as are attended with the formation of tubercle, and those in which the existence of tubercle is not evident. The latter are alluded to in the above reference, and the specimens of tubercle are included in the next series.

**SERIES VI. *Tumours and other allied morbid growths.***—The interesting and important specimens comprised in this series have been arranged in accordance with the classification generally adopted in reference to them. This has been done, because the uncertainty of our knowledge respecting tumours does not afford encouragement for the adoption of any but that classification which is best known. All these growths are, therefore, classed into the encysted and the solid; the latter being either innocent or malignant. Such names are applied to the various species, as are in most common use, and most plainly indicate the structure and other



principal characters of the growths to which they are applied; and such an arrangement has been made of them, as shows the varieties of appearance which each one may present without losing its specific characters, in the progress of its development or subsequent degeneration.

Sub-series A. *Cysts or encysted tumours* comprehend wens, hydatids, adventitious serous membranes, &c., but excludes what are called *secondary cysts*, that is, capsules or cysts formed round solid tumours or other bodies imbedded in the tissues. Many of these *encysted tumours* are really only unnatural enlargements of naturally existing parts, such as the hair-follicles and gland-ducts; and some, commencing in this way by the retention of the secretion and enlargement of a gland-duct, are by subsequent growth converted into solid tumours.

*Simple cysts, with serous or other liquid contents.* There are examples from almost every part of the body; and there are two singular specimens of *cysts containing air*, from the intestines of a pig, which are referred to by Hunter, as proving that air may be eliminated from the blood. Next we have—

*Cutaneous cysts; cysts containing cuticle, sebaceous matter, hair, or other products or appendages of skin.* The skin and ovaries are almost exclusively the seats of these formations. In the skin their occurrence is easily accounted for by the accidental obstruction of a follicle—the gradual accumulation within it of epithelium or hair,—and its subsequent growth and development into a distinct cyst.

We quote the account of their formation in the ovaries:

“When similar substances are found in ovarian cysts, they are probably always formed on a part of the cyst-wall which has the structure of true skin, with a cuticular surface, hair-follicles, sebaceous, and sometimes perspiratory, glands. The structures and secretions formed on this portion of the cyst are shed into its cavity, and there accumulate; and they remain when, as often happens, the cutaneous structure on which they were produced has degenerated and disappeared.” (pp. 67-8.)

To these succeed specimens of *compound cysts; cysts having others on their walls*; including some of the compound cystoid growths of matter, and of the compound serous cysts, or compound adventitious membranes of Dr. Hodgkin; including also most of the multilocular cysts which occur in the ovaries. The best account of these is to be met with in Dr. Hodgkin's ‘Lectures on the Morbid Anatomy of the Serous and Mucous Membranes.’

The last division of this sub-series includes *cysts containing vascular growths from their walls*. The peculiarity of these consists in the production of solid vascular growths from the interior of cyst-walls, which project into the cavity of the cyst, and many at length entirely fill it. The female breast affords some of the best examples of this variety; in which, when the integuments have ulcerated, and the walls of the cyst have given way so as to allow the protrusion of the fungous growth, the disease presents a most appalling aspect. The specimen, of which the following is a description, presented to the museum by Mr. Henry James Johnson, is a very complete illustration of this type:

“*Prep. 175.* Section of a breast, in which is a large tumour presenting the same general characters as those just described. It is formed by a collection of cysts, most of which are filled by lobulated growths; but some contain a more uniform

and solid substance, and some, which contained fluid, are now empty. A part of the tumour has protruded through the integuments; and some of the intra-cystic growths in this part have protruded through their cysts in a soft mass with a granulated surface. The integuments around the protrusion are healthy.

"The patient was a florid healthy-looking woman, fifty-one years old. She had had ten children, four of whom she suckled from the breast in which this tumour was seated. The tumour first appeared nineteen years before it was removed. Its origin was ascribed to the bruising of the breast by severe pressure against a mattress during the patient's first confinement. Soon after this, a lump 'as large as a teacup' formed. Five or six years later the breast began to enlarge, and thenceforward gradually increased. When the patient was first seen by Mr. Johnson, the tumour measured thirteen and a half inches in its vertical diameter, and eleven inches in its transverse diameter. Its surface was irregular and firm, except above the nipple, where in one part fluctuation was felt. It was very heavy. The skin was adherent to the greater part of its anterior surface, and about the centre was dusky red, and covered with scales; but the tumour was moveable on the pectoral muscle. It was not painful, except after being handled; and the axillary glands were not enlarged.

"An attack of sore throat and fever delayed for a time the removal of the breast; and during the recovery of the patient from this illness, the dark-coloured patch of skin gave way, and a fungous growth protruded through the opening. When removed, the tumour weighed nearly seven pounds. The patient was in good health two years after the operation." (pp. 77-8.)

Sub-series B. *Solid tumours*. The species described are:—1. The adipose tumours. 2. The cartilaginous and osseous. 3. The fibro-cellular. 4. The fibrous or fibro-calcareous. 5. The osteoid. 6. The epithelial. 7. The schirrous tumour or hard cancer. 8. The medullary or soft cancer. 9. The alveolar or gelatiniform cancer. 10. The tubercle. In an appendix (A) are placed tumours of uncertain nature; and in a second appendix (B) pendulous or loose tumours found in the cavities of the body.

This arrangement corresponds with the accepted division of solid tumours into innocent and malignant. The latter are determined by the following characters, which it is important to note, because by the possession of them tubercle and phthisis are considered by the authors of this catalogue to be malignant diseases.

"1. Their structure is not like that of any of the fully-developed natural structures of the body, nor like that which is formed in any natural process of repair. 2. They may have the character of *infiltration*, i. e. their elementary structures, instead of being collected in a mass, distending and separable from the tissues in which it lies, may be diffused or infiltrated in the interspaces and cavities of those tissues; so that, at least in its early state, the tumour or growth may consist of its own proper elements mingled with those of the tissue in which it is formed, and which degenerates and disappears as the morbid growth increases, and, as it were, overwhelms it. 3. They exhibit a peculiar tendency to soften, and then to ulcerate. 4. The ulcer which succeeds to a malignant growth has no apparent disposition to heal; but a morbid substance, like that of which the original growth was composed, is formed in the walls of the ulcer; and as this substance passes through the same changes of softening and ulceration which the primary growth passed through, so the malignant ulcer extends, and makes its way through tissues of all kinds. 5. Malignant growths not only enlarge, but, apparently, multiply or propagate themselves, so that, after one has existed for some time, others like it grow, either around it in widening circles, or in parts remote. The reproduction of malignant tumours, after their removal by surgical operation, may be regarded as an example of this characteristic tendency to numerical increase. 6. In this multiplication, as

well as in the progress of ulceration, of a malignant disease, there is scarcely a tissue or an organ which the same disease may not invade." (pp. 80-81.)

Accepting, then, this definition of malignancy, the adipose, the cartilaginous and osseous, and the fibro-cellular tumours are decidedly not malignant. The schirrous, medullary, and alveolar tumours, and tubercle are as decidedly so; and the remaining three hold a sort of middle place, as they possess only one or more of the characters of malignancy. Thus the fibrous tumour, in its liability to return after operation, the osteoid tumour in its still greater liability to return and invade parts remote from its primary seat, and the epithelial tumour in its tendency to ulcerate and extend itself indiscriminately into the neighbouring tissues, all present some of the decided characters of malignant growths.

1. *Fatty tumours* are too well known to require description. Under the name of lardaceous tumours, the lipoma mixtum of Müller, are included certain growths, which combine characters belonging to both the fatty and the fibrous or fibro-cellular tumours.

2. Under the head of *cartilaginous and osseous tumours*, are included the enchondroma of Müller, sarcoma cartilagineum, sarcoma chondroides, osteo-chondroma, osseous exostosis, and ivory exostosis. Müller has described these at great length under the general term enchondroma. They are met with in the form of round or oval masses, which exactly resemble, in their firmness, grayish-blue colour, and half transparency, the cartilage of the foetal skeleton. They still more resemble it in their tendency to become bone, which transformation is effected in a similar manner to the ossification of ordinary cartilage; and, on this account, such osseous tumours only as are formed by ossification of cartilaginous tumours are arranged as examples in this part of the catalogue. Such growths have the microscopical character of foetal cartilage and of true bone. A species of degeneration or softening is also met with in them, whereby their investing membranes become bags of a viscid yellow fluid. We subjoin an example of this, and also one of the ordinary cartilaginous tumours, the history of which is taken from the unpublished manuscript of Hunter:

*Prep. 197.* "A section of a tumour which weighed 144 ounces, and was removed by Mr. Hunter from the side of a man's neck. It is of an irregular oval form, measuring nine inches in one, and seven inches in another, of its chief diameters. It is deeply lobed, and very firm; its outer surface is uneven and nodulated; its cut surface displays numerous round masses of pale, semi-transparent, and glistening cartilage, connected by their several investments or capsules of fibro-cellular tissue. These masses are of various size, from a quarter of an inch to two inches in diameter. A few of the largest are also traversed by thinner partitions of fibro-cellular tissue, and some of them are very soft.

"The case is thus recorded by Mr. Hunter:—

"*Tumour extracted from the neck, weighing 144 ounces.* John Burley, a rigger, thirty-seven years of age, of a middle size, dark complexion, and healthy constitution. About sixteen years ago fell down and bruised his cheek on the left side above the parotid gland. It was attended with a good deal of pain, which in four or five weeks went off, and the part began to swell gradually, and continued increasing for four or five years, attended but with little pain.

"At this time it was increased to the size of a common head, attended with no other inconvenience than its size and weight. He again fell and received a wound on its side, which gave considerable pain at first, but it got well in eight or nine weeks. After this the tumour increased without pain, on the lower part, as also at the basis, extending itself under the chin to the amazing size it now appears.

Lately he has perceived that its increase is much greater than what it was some time ago; he says he can perceive it bigger every month.

"The tumour is in parts the colour of the skin; in other parts of a shining purple where the skin of the cheek is elongated. The beard grows upon it, and is shaved in common. When by accident it is wounded, it heals kindly, because it is only the skin that is wounded, and has sensation in common with the skin. It is hard to the feel in some places, and in others softer, as if containing a fluid. It seems quite loose, and unconnected with the skull or lower jaw; and can be moved easily without giving pain.

"The tumour weighed 144 ounces.

"The operation was performed on Monday, October the 24th, 1785; it lasted twenty-five minutes, and the man did not cry out during the whole of the operation.

"His symptoms after the operation were mild and gentle. Does this gentleness arise from the want of sensibility, for the man who had the tumour taken out of the calf of the leg had his symptoms run high, and then soon sunk; and he seemed to feel the operation as if tortured." (pp. 86-7.)

*Prep. 207.* "Section of a tumour, thirteen pounds in weight, which grew in front of the lumbar vertebræ of a soldier thirty-seven years old. It was loosely connected with the vertebræ by its investing fibro-cellular tissue. It surrounded the aorta, and the inferior *cava* and iliac veins; the veins were compressed and filled by coagula. It was of an oval form, lobulated, sixteen inches long, and about six inches wide. Half the tumour was composed of a soft, pulpy and flocculent, medullary substance, of a brownish colour. A small portion of this is preserved, and hangs in long and loose shreds. The other half of the tumour, including the greater part of that which is preserved, consisted of nodules of cartilage of various forms—rounded, oval, elongated, or quite irregular—and from one fourth to three fourths of an inch in diameter. These are invested and held together by layers of fibro-cellular tissue. They have both the obvious and the microscopic characters of foetal cartilage. In the centres of some of the nodules of cartilage there are small portions of cancellous bone, like the points of ossification of the foetal skeleton; the centres of others are rather softened. The limit between the cartilaginous and the medullary part of the tumour is well marked; and although they are in close contact, there is no appearance of any conditions intermediate between them, as if the one had degenerated into the other.

"In a portion of the tumour, not shown in this specimen, the softening process had reduced many of the nodules of the cartilage into a yellow viscid fluid, which was retained, as in thick-walled cysts, in the fibro-cellular investments of the softened nodules, and was in some instances mixed with blood." (p. 90.)

3. The subdivision of *fibro-cellular tumours* comprehends the "cellular tumour of Lawrence, and includes, probably, the common vascular sarcoma, and the pancreatic sarcoma of Abernethy, and most examples of the chronic mammary tumour of Sir A. P. Cooper." The general character and progress of these tumours is tolerably familiar to the surgeon. They are stated to have two stages of development—an early and a late. In the former—

"These tumours are comparatively soft and brittle, and consist of a pale greyish or yellowish succulent substance, which glistens on its cut surface, has a vitreous aspect, and is partitioned or intersected by opaque-white circling and undulating bundles of fibro-cellular tissue. In their completely developed state, they consist, as their name implies, of a tissue similar to the fibro-cellular, areolar, or common cellular, tissue of the healthy parts. This tissue is formed, by the close weaving of its filaments without any apparent order, into masses of an elastic, compressible, compact, opaque-white substance, which, in different examples, presents a more or less obscurely fibrous aspect, and various degrees of toughness or hardness." (pp. 91-2.)

It is further stated, that there is so great a similarity between the solid growths found in cysts, and tumours of this species, that it seems likely that all fibro-cellular tumours have their origin in intra-cystic growth, although the intimate union between the growing tumour and the cyst-wall may become so firm as to obliterate entirely its original cystic character.

4. *Fibrous and fibro-calcareous tumours.* The well-known fibrous tumour of the uterus is the type of this class. Like the preceding, it derives its name from the resemblance which it has to some of the natural tissues of the body; and as in the natural structure the distinction between fibro-cellular tissue and fibrous tissue is not always distinctly marked, so in these growths there is no definite line of distinction between them. To quote the expression of the Catalogue, "Fibro-cellular and fibrous tumours may be said to *run into* one another." As is the case in fibro-cellular and cartilaginous tumours, this class also frequently consists of distinct growths united by fibro-cellular tissue.

It would not be possible, without quoting the whole of the descriptions in this division, to convey a correct notion of the peculiarities in the mode of growth and development which these tumours present; and this our space forbids.

5. *Of the osteoid tumour* (ossifying fungous growths or osteoid tumours of Müller), the majority of specimens are placed amongst the diseases of bone; as it does not appear that any parts besides the bones are ever their primary seat.

6. *Epithelial tumours.* This class of tumours is one of great importance, for the description of which we are indebted to microscopical investigations of no very ancient date. Our readers will recollect that it was described by Dr. Bennett, under the designation of cancrioid tumours (vol. iv., p. 197). The researches of one of the authors of this catalogue, upon the nature of this particular class of growths, have for some time past been familiar to the frequenters of St. Bartholomew's Hospital. We transcribe the entire description of them, and of the two examples which are here placed for their illustration. The second is especially remarkable on account of the circumstances under which it presented itself.

"The specific character of tumours of this species is, that they are chiefly composed of nucleated cells or scales, like those of such epithelium as covers the mucous membrane of the human fauces and pharynx, which cells are collected in masses or layers, or in wart-like growths, and are held together by tissue in which blood-vessels are distributed among them. The limits of the species, and the parts and circumstances in which it occurs, are not yet defined; but its characters are so peculiar that they warrant its distinction. Its most usual seats appear to be the lower lip, in which it constitutes one of the diseases regarded as cancer of the lip; and the front of the leg, after it has been long the seat of simple ulceration or of some other consequence of injury. It is probable also that some other growths regarded as cancers of the skin are of the same nature as the epithelial tumours.

"The epithelial tumours, like those of the two preceding species, are allied to the innocent tumours in the resemblance which their elementary structures bear to those of certain natural parts of the body; but they are allied to the malignant tumours in their tendency to spreading ulceration, and in the liability to disease which they appear to induce in the lymphatic glands receiving vessels from the parts in which they lie.

"*Prep.* 231. Part of a lower lip removed from a man at St. George's Hospital. A large portion of the border of the lip is covered by a slightly elevated warty growth,

which is probably composed chiefly of cells like those of epithelium. The border of the growth is sinuous, rises suddenly from the surrounding parts, is smoothly rounded, and a little everted. Except at the border, all the exposed surface of the growth appears excoriated or unevenly ulcerated.

"*Prep.* 232. Section of a tibia, and of the adjacent parts. Almost the whole thickness of the shaft of the tibia, near its middle, is softened, broken into small fragments, and absorbed; the part thus diseased, about an inch in length, is occupied by a soft, pulpy, flocculent, and very vascular growth, which is almost entirely composed of nucleated scales like those of the epithelium of the pharynx. The growth protrudes through the integuments, in which it appeared to commence, with a granulated and fissured wart-like surface; and its posterior part, extending through the wall of the tibia, is imbedded in the deep muscles of the calf. The integuments through which the growth protrudes are thin, livid, and shining, like those around old ulcers of the legs; but they are not otherwise diseased, even at the margins of the growth, and the bone around appears healthy.

"The patient was a sailor, who, thirty-three years before the amputation of his leg, was struck by a musket-ball on the front of the tibia. The ball was extracted half an hour afterwards, but necrosis of a portion of the anterior wall of the tibia ensued. After exfoliation of a thin long piece of bone, the wound healed, but imperfectly; and for thirty-two years it went on sometimes nearly healing and then again ulcerating. A year before the amputation, when the old wound was nearly healed, and the cicatrix was depressed, smooth, and tightly fixed to the tibia, there arose from the diseased parts a mass of vascular, warty, *fungous* granulations, which rapidly increased, giving considerable pain, and occasionally bleeding.

"The patient died seven weeks after the amputation, with sloughing of the stump and pneumonia. No other tumours were found in any part of the body after death." (pp. 104-5.)

7. The division of *schirrous tumours*; *hard or schirrous cancers*; includes the carcinomatous sarcoma of Abernethy; the schirroma of Carswell; the carcinoma simplex and carcinoma fibrosum of Müller; stone cancer; ivory cancer; and probably the carcinoma reticulare of Müller.

"The common hard cancer of the female breast may be regarded as the type of the disease of which specimens are described in this section, from 233 to 251. The principal designs of the arrangement adopted are to illustrate, first, the general characters of scirrhus tumours or cancers of the breast, and of such others as, judging chiefly by their peculiar hardness, and their dense fibrous texture appear to be most intimately allied to them; secondly, the process of softening in scirrhus cancer, and the process and characters of the subsequent ulceration; and, thirdly, the changes connected with what appears to be a partial healing of the ulceration." (p. 106.)

8. *Medullary tumours, medullary or soft cancers*, have been described by different writers under a great variety of names. In the catalogue they are arranged as presenting six different varieties of one species, viz., *a*, the firm; *b*, the spongy, pulpy, grumous, or shreddy; *c*, the brain-like or encephaloid; *d*, the cystic; *e*, the bloody or hæmatoid; *f*, the melanotic variety. Respecting the last-mentioned or melanotic variety, it is stated, that all the specimens appear to be examples of medullary disease with the complication of black pigment deposited in their substance. Whether this be or be not the true pathology of melanotic tumours, we cannot say. Unquestionably that state of the system which gives rise to the excessive formation of black pigment is altogether obscure, and to our minds has always afforded grounds for separating entirely this class of tumours from any other variety.

9. *Alveolar or gelatiniform cancers*. The disease thus designated presents, on the contrary, features of remarkable uniformity ; so that although it has received different names, its characters are clearly marked out.

10. *Tubercles*. "The names *tubercle* and *tuberculous matter* are here assigned to the peculiar substance which is commonly described as scrofulous tubercle or as scrofulous or caseous matter, and the progress of which in the lungs constitutes pulmonary phthisis." We have already intimated that Mr. Stanley and Mr. Paget regard tubercles as coming under the category of malignant diseases. In other respects we do not know that remarks in the catalogue throw any new light on their pathology.

SERIES VI. *Appendix A. Tumours of uncertain nature*. A considerable number of growths are classed thus, some of them of a very singular character ; but no general account can be given of them.

SERIES VI. *Appendix B. Tumours or growths, pendulous or loose, in the close cavities of the body*. These include the loose cartilages found in joints ; and a number of others found either loose or hanging by a pedicle, in the abdomen of man and the lower animals.

We cannot conclude this notice of the first volume of the 'College Catalogue,' without expressing our earnest hope, we would almost say expectation, that the Council will be induced to re-issue it, in some other form, whereby it may become accessible to every member of the medical profession. At present its costly nature will put it beyond the reach of all but a favoured few. It is understood that the college is wealthy ; no part of that wealth could be better employed than in cheapening and thereby increasing the circulation of so valuable a publication.

We shall now offer to our readers some account of the treatise of M. Cruveilhier, which opens with a dedication to Dupuytren,—a very graceful compliment, since it is to the Baron's munificence that the Faculty of Medicine in Paris owes the foundation of the chair of pathological anatomy. A considerable number of pages of the first part are devoted to a variety of general considerations, which need not detain us long from that which is more practical. The author defines Pathological Anatomy to be "that branch of the medical sciences which has for its object the determination of all the material lesions of which organized beings are susceptible." It is, therefore, the science of morbid organization, in the same way as physiological anatomy is the science of healthy organization ; and in like manner extends to plants as well as to animals. It is further divided into *special pathology*, which has for its object the study of but a single species, and *comparative pathology*, which should embrace the entire series of organized beings. Upon this should be founded a grand and beautiful science, which should comprehend in its vast domain the morbid lesions of all organized beings from the lowest vegetable up to man, which should show the analogies and the differences of the lesions that occur among beings of so diverse a character, and should express them in comprehensive laws of universal applicability. As yet, however, such a *philosophical pathology* does not exist ; although we need scarcely tell our readers that the conception of it was clearly present to the mind of John Hunter. It is the aim of M. Cruveilhier to supply this want in his own department,—that of Human Pathological Anatomy ; and after pointing out what he conceives to be its relations to Practical Medicine and Surgery,

he enunciates fourteen propositions, which *formularise* rather than *generalise* his doctrines.

*Proposition 1.* The number of morbid species, or of the varieties of morbid actions, is limited.

2. The morbid species are identical, whatever be their seat.

3. Lesions which are general or common to all the tissues, constitute the rule; special lesions are the exception.—This is precisely the opposite of the doctrine of Bichat.

4. Each tissue and each organ has its own morbid affinities.

5. There exists a certain number of special lesions.

6. Anatomical characters should form the basis for the determination of the different varieties of morbid lesions.

7. The anatomical characters display themselves in the external conformation, and in the intimate texture, of the diseased organs.

8. The study of the evolution of morbid lesions is required for the determination of their species.

9. Experimental pathology is one of the most fertile bases of pathological anatomy, and consequently of pathology.

10. Compound morbid species result from the association together of a certain number of lesions; and this association occurs according to certain rules, which may be called the laws of morbid association.

11. The morbid species do not become transformed one into another.

12. One morbid lesion may be considered as occasioning an immunity as regards other species of lesions.

13. Living tissues are in themselves unalterable; all abnormal structures being of new formation.

14. The ultimate seat of all morbid nutrition, and of all morbid secretion, is in the capillary system.

Several of the postulates here enumerated are obscure, and are explained by observations of considerable length; some of them are also, to our minds, more than questionable. We shall not run the risk of being supposed to undervalue the merit of those who aim at making pathological anatomy something more than a matter of mere observation and statistical record, if we repeat an old warning to those whose imaginative and enthusiastic dispositions prompts them to early generalization. As there is nothing more fascinating, so is there nothing more dangerous, or more fatal to the progress of any science, than this desire to lay down abstract propositions which are to afford the key to doubtful phenomena. Of all habits of mind, this is the one which most certainly incapacitates an individual for independent observation. It is this that bends fact to theory, and colours every narrative with an artificial hue. Positive statements of any kind, in the infancy of a science, may indeed, like beacons, afford good help to navigators who know their real position and ascertained value; but these beacons, if erected upon an insecure basis, or in false positions, impede progress instead of advancing it, and destroy the ship instead of saving it.

Take for an illustration of this, *Propositions 11 and 12* :—

“The morbid species are not capable of transformation the one into the other;” and “the existence of a morbid lesion confers immunity against other species of lesions.” Cruveilhier deduces from this the practical conclusion, that surgeons need not take into account, in advising extirpation



of a doubtful tumour, that it may undergo cancerous degeneration; and asserts, "It results from innumerable observations, that a cancer is cancer from the first moment of its formation: it cannot be otherwise than this—that a fibrous tumour is a fibrous tumour from the first moment of its appearance, and will still be a fibrous tumour thirty, forty years after, as I have seen at La Salpêtrière in numerous instances." (p. 52.)

But is this so? Is it possible to lay down a law of this sort, and to act upon it without danger to our patients? If this question were addressed to ourselves, we should decidedly answer No. Are there no instances in which a lump in the breast of a girl, has, after the lapse of many years, become the seat of a cancer? Has M. Cruveilhier never seen "a wen" degenerate, and become the seat of melanosis? There exist, we are persuaded, few surgeons who have not witnessed both these phenomena. It does not do to say that the malignant deposit takes place in the intermediate structure, nor does Law X explain the circumstance. "Compound morbid species result from the association of a certain number of lesions; and this association takes place in obedience to certain rules, which may be called the laws of morbid association." In the cases we have referred to, there seems to be a regular transformation—degeneration, if you will—of the structure of one morbid growth into that of another species. But take it in whichever light we please, so long as the fact is established that a tumour, in the breast for example, is more liable to become the seat of cancer than any of the healthy tissues around, the law has not the practical importance which M. Cruveilhier assigns to it; but, on the contrary, begets a false security if believed and acted upon.

In his previous 'Treatise on Pathological Anatomy,' M. Cruveilhier had adopted a classification, in which the material lesions were studied purely in themselves, without any consideration of their anatomical or nosological relations. On the other hand, M. Andral has adopted, as the basis of his classification, the nature of the morbid actions of which the several lesions are the results. This is objected to by our author, as substituting *pathogeny* for pathological anatomy, and as substituting theoretical considerations for matters of fact. He retains his former system, therefore, with some modifications; one of the principal of which is, that congenital malformations no longer constitute a group by themselves, but that they are considered according to their specific characters, along with extra-uterine lesions of similar nature. The following is an outline of his present system:—

In a *first* group are comprehended all those lesions which affect the *continuity* of the tissues:—1st. Solutions of continuity. 2d. Adhesions.

In a *second* group are comprehended all the *lesions of contiguity*; that is to say, all the changes of situation and connexion which affect parts naturally contiguous. This group of lesions, which may be called displacements, is divided into three well-marked classes:—1st. Luxations. 2d. Invaginations. 3d. Herniæ.

In alliance with the two preceding groups, a separate division is formed under the name of *deviations*; in which are comprehended all those changes of direction, all those abnormal positions, which occur in the several organs, whether affecting their own conformation, or their position respecting other organs.

In a *third* group, for which we have in English no appropriate word, but which M. Cruveilhier calls *lesions de canalization*, are included all

those derangements which appertain to the circulation of the fluids, and of the different substances contained in the numerous canals by which the frame is channelled. This group includes three classes; 1st, foreign bodies; 2d, contractions and obliterations; 3d, dilatations.

In a *fourth* group are included all *lesions of nutrition*, which are themselves divided into two classes; 1st, hypertrophies and atrophies, which include all those changes of form, size, and consistence, without alteration of texture, which are consequent on an augmentation or a diminution of nutrition; 2d, the metamorphoses, and organic productions of analogous nature, which comprehend all the transformations of one tissue into another tissue of analogous kind.

Into a *fifth* group are united together all *lesions of secretion*, including dropsies and fluxes, which in themselves constitute two sub-classes.

Into a *sixth* group are included, under the name of *hemorrhages*, all extravasations of blood of a traumatic or non-traumatic kind.

The *seventh* group is composed of gangrenes.

An *eighth* group includes, under the name of "*lésions phlegmasiques*," all sensible changes which are characteristic of the inflammatory process.

A *ninth* group includes, under the title of *strumous lesions*, all those abnormal changes known under the name of tubercles or scrofula.

A *tenth* and last group comprehends, under the name of *carcinomatous lesions*, all the sensible changes which make up that disease which is known in pathology under the name of cancer.

**Class I. Solutions of continuity.** These are divided into three very distinct sub-classes,—1st. Wounds, or traumatic solutions of continuity. 2d. Solutions of continuity from internal causes, or not of traumatic nature. 3d. Congenital solutions of continuity.

The first kind are considered under the heads of wounds, contusions, "solutions de continuité par escarification," ruptures, and fractures; in all, five kinds of solutions of continuity.

On the subject of *incomplete fractures* of the bones, we find that M. Cruveilhier admits their existence as regards the bones of the skull, but considers them extremely rare in the long bones. They may occur in young people by a sort of bending and successive breaking of the bony fibres; so that in a long bone which is suddenly bent, the bony fibres on the convexity of the head may be broken, while those of the concavity remain entire. (pp. 90-1.) There is no reference given to any preparation exhibiting this fact. Are *longitudinal fractures* of bones possible? Admitted by Duverney, they are denied by J. S. Petit and by Louis. Our author explains this discrepancy, by regarding what have been considered examples of this form of injury to be splitting (*éclats*) following the direction of the long axis of the bone. There are such things as distinctions without differences in medicine as well as in other sciences.

*Separation of the epiphysis of bones from the shaft.* Dead infants are sometimes born, which exhibit this phenomenon. In some cases the periosteum of the putrid bone becomes infiltrated with blood, and lifts off the epiphysis from the shaft. With respect to what Monteggia, Chaussier, and Velpéau profess to have seen, namely, the occurrence of this accident from mechanical injury inflicted towards the close of pregnancy, M. Cruveilhier is only restrained from expressing his doubts by his respect for these observers; but should the fœtus in which this accident is exhibited

be at all putrified, he regards the accident as simply an effect of this process. The observations of Rognetta render it also probable that difficult labours may give rise to this occurrence. The result of M. Guérétin's experiments on the artificial production of separation of the epiphysis from the shaft of a bone is as follows:—In an infant nine months old, he was not able to produce a single luxation, but fractures in various parts of the shaft of the long bones, as often as three times in four; separation of the epiphysis, once in four cases. In children from two to seven years old, dislocation once in five cases; fracture of the shaft, most frequently from six to twenty lines from the point of its union with the epiphysis, as often as seven times in eight cases; separation of the epiphysis, once in nine cases. In subjects from seven to fourteen years old, not a single instance of separation of the epiphysis, in ten examples of luxation and fracture. The following is the result of experiments made by MM. Jarjavay and Bonamy in the presence of M. Cruveilhier, upon five subjects from eight to ten years old. Traction, twisting the articulations, the most violent efforts to produce dislocation, or separation of the epiphyses, direct blows upon the articulations themselves with the large end of a hammer, or still more directly upon each of the osseous extremities, either in their natural state or deprived of the soft parts, gave the following results:—In no one instance was there separation of the epiphysis, but always fracture of the upper and lower end of the femur, humerus, or bones of the leg, and fracture of the upper end of the radius. In one instance, the lower epiphysis of the femur was separated to two thirds of its extent; as regards the other third, the violence had taken effect upon the neighbouring parts of the shaft of the femur; and further, in the two thirds which he allows to have been separated, osseous fragments still adhered to the cartilage of the epiphysis. In one instance, the epiphysis of the olecranon was entirely separated, and with it the cartilage of ossification and articulation of the sigmoid apophysis, to which some bony fragments still adhered. In almost all instances, they were able to produce separation of the epiphysis of the lower end of the radius, by applying force to the wrist, either by the weight of the whole body, or by a smart blow with the hammer. Occasionally the lower end of the ulna separated at the same time as that of the radius, but generally the separation affected the radius alone. In these cases of separation, it was rare that some fragments of bone did not remain attached to the cartilages of both the epiphysis and the shaft.

The conclusion drawn from these facts is, that separation of the epiphysis is a rare occurrence, requiring for its production such a combination of circumstances, in the cases in which it happens before the complete solidification of the bone of the extremity, as to render it quite an exception.

*Ruptures.* A distinction is to be carefully drawn between ruptures of all kinds which are preceded by some alteration in the texture of the affected parts, and those which occur in parts unaltered by disease. This section of M. Cruveilhier's treatise refers only to such ruptures as occur without previous change in the parts concerned.

*Ruptures of ligaments* occur in dislocation and sprain. Sprains are not, Cruveilhier observes, mere stretchings of ligaments, as is commonly supposed; some tearing or solution of continuity in them is a necessary feature. The proof of this is to be found in the extravasation of blood which always accompanies them.

**Rupture of tendons.** It is remarkable, in cases of these accidents, that the rupture never takes place at the attachment of the tendon to the bone; it is always found either in the course of the tendon, in its own substance, or at the surface of the muscle from which it proceeds. The common opinion which considers these accidents as consisting in simple separation of the tendinous fibres at the spot where they become attached to the fleshy fibres of the muscles, is negatived by the appearance which the ruptured surfaces present; which is always clean, and deprived of any of those denticular prolongations, by which tendons terminate in the surface or in the substance of muscles.

As regards the *rupture of nerves*, our author observes, that in violent efforts to reduce dislocation, the nerves sometimes become detached from their insertion into the spinal chord. He cites a remarkable instance, narrated by Flaubert, of an individual who died from immoderate efforts made to reduce an old dislocation of the shoulder, and in whom the nerves which concur to form the brachial plexus were separated from the chord. Cruveilhier states that this fact is by no means a solitary one; but in a foot-note he observes, that in several experiments on the dead body, he was unable to produce a similar lesion.

In speaking of the modes of death, when this occurs as the immediate result of injuries, M. Cruveilhier gives a very interesting chapter on the question of the entrance of air into veins. He states that: 1st, the vein must be near enough to the centre of circulation for the inspiratory efforts to reach it; and 2dly, the sides of the vein must be kept open: whence it happens that the accident occurs generally in the removal of tumours, when the veins are adherent to them, and are stretched by pulling at the tumour to facilitate the extirpation. Death occurs in these instances from the suddenness with which the air enters. Experiments upon animals show that a small quantity of air, *slowly* injected into veins, is not fatal; but that it is immediately so, if the quantity be large and suddenly thrown in. In the majority of cases, the right side of the heart, being distended with the injected air, cannot receive the blood, and consequently cannot transmit it to the lungs; but contracting upon the air within, it forces this back into the nearest veins, as the jugular veins, and into the brain, and death ensues almost immediately. M. Cruveilhier concludes by cautioning us not to confound the cases often met with, in which air, the result of putrefaction, is found in the veins and blood, with those rare instances in which it has entered them during life.

**Death by consecutive accidents.** On the subject of tetanus, although our author adds nothing to our previous knowledge, yet he has the following suggestions:—Premising that no morbid lesion is to be found after death from tetanus, he affirms that the fatal result is brought about by asphyxia, consequent on spasm of the respiratory muscles; and therefore, what we have to do is to relieve this spasm, and the disease will cure itself. With this view it occurred to him, that if it were possible to substitute a voluntary and powerful contraction of these muscles, for the convulsive one, some good might be effected. Accordingly, in the year 1821, Cruveilhier tried the following experiment on a young countryman, who was affected with violent traumatic tetanus, which had resisted all the ordinary means of relief:—

Placing himself opposite the patient, he directed him to take regular

deep inspirations, which were regulated by beating time with a baton before the sick-man's eyes. For the space of an hour, during which Cruveilhier himself performed the part of conductor, there was no spasm of any description; and at the expiration of four hours, during which assistants regularly relieved each other, profound sleep ensued. On the patient's awaking, the same process was repeated, and with similar success. A few trifling spasms came on when the deep voluntary inspirations were suspended; but eventually the young man got quite well. In another case, the same experiment was unsuccessfully tried, although the patient eventually recovered. We cannot here avoid making the observation, that since nearly thirty years have elapsed from the time of M. Cruveilhier's first successful experiment, it does not say much for his own faith in its efficacy, that he has not now any further experience to communicate on this subject.

Several of the succeeding pages are occupied with the subject of death from phlebitis; but, after a careful perusal of them, we are unable to find any facts which will be new to the readers of this Journal, or which require that we should devote our space to an analysis of them. The chief novelty seems to be a successful attempt to combat the doctrine of M. Teissier, who attributes those visceral abscesses which are regarded by most persons as consequent upon suppurative phlebitis, to a purulent diathesis (*diathèse purulente*).

We shall pass over, without comment, the whole chapter upon solutions of continuity of a traumatic kind. It really amounts to little more than a list or catalogue of them; and it would serve no good purpose to occupy our pages with the translation.

Class II. Under this head we find a long chapter upon the subject of *Adhesion*. The different examples of the process are divided into those of a reparative character, and those that are unhealthy or morbid. Among these latter are included the different kinds of ankylosis, of which M. Cruveilhier enumerates five varieties.

1. *L'ankylose périphérique, ou par invagination*; in which the articular extremities are united together by a sort of sheath of a more or less complete nature. The type of this is found in the vertebral column.

2. *L'ankylose par fusion*; of which there are two varieties: 1. Ankylosis by juxtaposition, in which the adjacent surfaces of a joint, deprived of their cartilages, are simply joined together, without deformity. 2. Ankylosis by fusion, properly so called, in which the adjacent surfaces are completely fused one into the other, that it is impossible to tell which portion belongs to one end and which to the other.

3. *L'ankylose par intermède*; in which there is an intervening plate of bone of greater or less thickness and density. Cruveilhier has seen an example of this in the knee-joint.

4. *L'ankylose par amphiarthrose*. In this variety the osseous extremities, deprived of cartilage, are united together by a fibrous tissue, in a manner analogous to the union of fracture *par cicatrice*.

5. *L'ankylose composée, ou mixte*; in which the first form is combined with interosseous union.

Under the head of *Congenital morbid adhesions*, we find the following, as constituting some of the varieties of malformations:

*Adhesion of the kidneys to each other*. This occurs in some cases;

(1) by adhesion of the upper, or (2) of the lower parts, of the kidneys across the vertebral column; (3) by complete fusion of one kidney into the other; (4) only one kidney exists, furnished with either one or two ureters. It is remarkable that, in all these abnormalities, the supra-renal capsules remain distinct, and in their proper situations.

*Adhesion of the testicles.* An authentic example of this was recorded by Geoffroy Saint-Hilaire and others. In a child of doubtful sex, who died when 18 months old, the scrotum was found empty, and the kidneys, supra-renal capsules, and testicles, were united on the median line; the spermatic vessels, the vesiculæ seminales, and the vasa deferentia, were double. This malformation can only take place in the abdomen, because the testicles do not descend to the scrotum until the close of intra-uterine life. There is no authentic example of a similar condition of the ovaries.

*Adhesion or fusion of the cerebral hemispheres.* In two examples of cyclops, Cruveilhier met with this malformation; but had no opportunity of carefully dissecting them.

The whole subject of malformations from adhesions is illustrated by M. Cruveilhier in a manner similar to the foregoing examples. We shall not stop to enumerate them, however, for it would be little better than an enumeration; but shall pass on to the Third Class or division.

*Class III.—Displacements by Luxation.* Among the accidents which attend the reduction of dislocations, our author mentions one which we trust and believe it has rarely fallen to the lot of English surgeons to witness; viz. rupture of either of the large vessels of the dislocated limb. Cruveilhier himself has seen it occur more than once; and from his observations we gather, that it is not an extremely unfrequent event in France, during attempts to reduce old dislocations. He observes, however, that in some of these cases, the attempts had been of a very moderate kind, and supposes an unnatural brittleness in the ruptured vessel, either from the effects of previous contusion, or degeneration in its walls. The same accident has occurred to nerves; when paralysis or death has of course followed.

In a paragraph with the title, *Practical Conclusions relative to the Treatment of Dislocations*, we find this question and answer: What are the obstacles to the reduction of dislocation?—The displacement is maintained:—1. By that portion of the ligamentous apparatus which has escaped being torn. 2. By the muscles which surround the articulation.

The proof that the ligaments form one of the chief obstacles to the reduction, is that in those dislocations where there is complete or almost complete rupture of the articular ligaments, the reduction is extremely easy; and the most difficult dislocations to reduce are those of joints which have a fibrous capsule (or fibro-capsular ligament), which is always incompletely torn. Contrast, on the one hand, the hip-joint, and on the other, the knee-joint. As a further proof of this truth, certain cases of distinction of the metacarpal articulation of the thumb may be cited; the obstacle to the reduction here unquestionably does not lie in the muscles, because the dislocated joint is as moveable as possible; but the difficulty, or even impossibility, of reducing this displacement, is due to the interposition, between the articular extremities, of the collar or fibrous semi-capsule formed by the anterior and lateral ligament. The impediment to reduction, occasioned by the ligaments, is as considerable in the dead body

as in the living; but to the latter there is added a further obstacle in the muscular contraction; and, in the case of some joints, as, for instance, with the hip and shoulder-joint, the impediment thus caused appears to be the principal one,—whence arises the facility with which these dislocations may be reduced in the dead body, or in a living man, whilst in a state of syncope, &c. From this, also, is derived the practice of reducing dislocations of the shoulder in the horizontal position, in order to neutralise the muscular action as much as possible; and also the remarkable success which attended Dupuytren's efforts in fixing the attention of the patient at the decisive moment.

M. Cruveilhier's article upon congenital dislocations of the hip-joint, is an interesting one; and the perusal of it will repay those who may be taking up the investigation of this obscure subject. The same remark will apply also to a previous article upon the question of the causes of the elongation and subsequent shortening of the thigh in morbus coxarius. Our space, however, will not permit of a lengthened notice of their contents.

Class IV. treats with great minuteness of *displacements by invagination*. These are considered under five subdivisions:—1, invagination limited to the mucous membranes, which can only occur at the outlets of these membranes: 2, invagination with three cylinders, as in the ordinary intestinal invagination: 3, invagination with two cylinders, as when it affects the anal extremity; while the remaining two comprehend invagination of the urino-generative organs, traumatic invaginations, and the congenital form. Many of these classes comprehend again other classes, or minuter subdivisions still.

Class V. comprehends the different varieties of *Herniæ*, of which the author admits the following forms:—1, tuniquary herniæ; 2d, aqueous herniæ; 3, traumatic herniæ, such as are occasioned by solutions of continuity; 4, ventral herniæ, or herniæ by eventration from yielding, or dilatation with attenuation, of the abdominal walls; 5, herniæ by separation of the fibres of boundaries of cavities; 6, common herniæ, by the natural openings of the abdominal cavities; and to these are to be added complicated herniæ, such as incarcerated and strangulated herniæ, and congenital herniæ.

The last division, which closes the volume, speaks of displacement by deviation, such as distortions of joints, the spine, &c.

We shall here bring to a conclusion our notice of this, the first volume of M. Cruveilhier's Treatise, because enough has been said to enable any one to form an opinion as to how far the work is likely to supply that desideratum in science,—a good general treatise on Pathological Anatomy. A few remarks upon its merits may prove an appropriate conclusion to this article. Both this work and the Pathological Catalogue of the College of Surgeons, have been long and anxiously expected by the profession; and they may fairly be considered to represent the state of pathological knowledge in their respective countries. M. Cruveilhier's treatise did not reach us until our notice of the "Catalogue" had been written; but after a careful reconsideration of what we then said, and a careful comparison of the two works, our original impression remains unchanged; and we still regard the work of our own countryman as unrivalled in its character, both in its conception and its execution. On a superficial view, it would

seem unfair to compare a "Treatise" with a Catalogue, since the two might have no other resemblance, than that they related to the same science. But a very little investigation will suffice to show, that what the French writer has named a "Treatise," might, in parts, have been more correctly called a Catalogue; and that which comes to us in the modest garb of a descriptive catalogue, contains within it material for many treatises. We cannot more correctly illustrate our own impression, than by saying the one is like the labour of a Boswell, and the other of a Johnson; the one rejoicing in petty details, revelling in endless divisions and sub-divisions, wearisome in its minuteness, yet, on the whole, conveying a correct picture of its subject, though not, by any means, a pleasing one; and the other sketches out, with bold and rapid hand, a picture complete, though yet in outline; comprehensive, because dealing rather in *series* of facts, than dwelling upon single ones; truthful in its simplicity, because adopting only such divisions as are to be found in nature, and that are necessary to make its details intelligible; and leaving in the mind the permanent impression of a grand and philosophic system. In short, comparing M. Cruveilhier's treatise with the works of Rokitansky and Hope, we cannot but regard it as of an inferior character; and consider that whatever credit it may gain for the minuteness of the author's observations, it will not do much to advance his reputation as a philosopher.

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#### ART. VIII.

1. *The Diseases of Children.* By FLEETWOOD CHURCHILL, M.D., M.R.I.A., Honorary Fellow of the King's and Queen's College of Physicians in Ireland, &c. &c.—*Dublin*, 1850. Fcap. 8vo, pp. 656.
2. *Die Bronchitis der Kinder. Eine auf eigene Beobachtungen und Untersuchungen gegründete Abhandlung.* Von Dr. CASPAR FRIEDRICH FUCHS.—*Leipzig*, 1849. 8vo, pp. 137.  
*The Bronchitis of Children; a Treatise based upon personal Observation and Investigation.* By Dr. CASPAR FREDERICK FUCHS.—*Leipsic*, 1849.
3. *Essays on Infant Therapeutics.* By JOHN B. BECK, M.D., &c. &c.—*New York*, 1849. Small 8vo, pp. 117.

OUR general opinion of the Dublin school of practical medicine is a very high one; and from amongst its teachers we could select some who, in our opinion, stand in the first rank as preceptors in those vital elements of our professional education—clinical medicine and surgery. Our American friends must, we presume, coincide with us in this favorable judgment; since, as we are informed by Dr. Churchill, in his preface, an application was made to him from across the Atlantic, to write a work on the 'Diseases of Children,' and this, too, after he "had given up all thoughts of again appearing before the profession as an author." Dr. Churchill remarks: "I did not like to refuse so flattering a compliment; and the present volume is the result." "The work has been written in the midst of the distraction of professional business, or at hours which are usually devoted to rest." (Preface.)

Considering, then, that there was in our author's mind neither the intention of again taking pen in hand, nor, so far as we can glean from



anything he has stated, any floating design of a treatise on infantile affections, which required the opportunity or the will to bestow upon it a substantive structure; keeping in view, also, that his work has been written under circumstances, some of which we should regard rather as drawbacks to the value of such a production, and others as by no means enhancing it; we think that Dr. Churchill deserves much praise for the general success of the result, although we shall feel called upon to qualify this laudation by some comments, less advantageous than we could desire, upon matters of detail. Even a cursory perusal of it will indicate that it displays much reading and research, and acquaintance with many of the better writers of France, Germany, Britain, and America; and although a more careful examination will prove that it is not always brought up to the advances made in pathology when the author penned several of his chapters, and sometimes long before, yet the failing in this respect is not so general as to lead us to dwell upon it as a very marked deficiency. We commend it also, because, with some exceptions, it is stamped with that scientific and precise character, which, until the appearance of the lectures of Dr. West—reviewed by us in our sixth number—we have not the least hesitation in saying, no systematic work on the Diseases of Children, published in this country, sufficiently possessed; and which is alone sufficient to raise it with that work to a position, to which the treatises of the Underwood type must relinquish, if they have not already resigned, all hopes of aspiring. This volume of Dr. Churchill's, like that of Dr. West, evinces the earnest endeavour to render such an account of the diseases of infancy and childhood, as may be placed side by side with the more advanced systematic treatises on the diseases of adults; and not to rest satisfied with the detail of such "trivial fond records" as the nurse in Romeo and Juliet might have impressed on the "table of her memory," or the matron of a lying-in hospital have jotted down as her midnight lucubrations during the tedium of her watchings as the handmaid of Lucina. We praise Dr. Churchill's book, also, because the "practice" inculcated in it, is one which, in most cases, is based upon a sound and fairly-advanced pathology, and is therefore in general a safe and a proper one.

But we cannot, in justice to the free expression of that judgment which it is our duty and privilege to make known, refrain from remarking, that although Dr. Churchill's work is marked with these commendable characteristics, it is, on the other hand, less distinguished by those features, which we should, *a priori*, have expected to have prominently existed in a work proceeding from the school of Dublin. We have been disappointed in finding how small an amount of matter included in its 650 pages is drawn from the author's experience, or is the expression of his own formed opinions; and how few are the attempts at a development of new views, or the more complete working out, by personal investigation, of those advanced within the last few years by continental pathologists. But we presume we have little right to look for such, except in a work which is the production of one who writes *con amore*, and not simply because he is asked to do so; who writes because he meant to write, and would write, and not to repay a compliment after he never intended to write again. A labour of intention and of love, and a work of forced labour, are two entirely different things.

We assume from the form in which the work before us is published—that of the Manuals of the publisher in Princes-street, Soho—and from the preliminary section ‘On the Management of Infancy and Childhood,’ that its author intended to have the requisitions of students and junior practitioners particularly in view. Be it so, or not, we feel that it is in its relation to them, that our remarks upon the deficiencies of the work are more particularly applicable. There can be no doubt that a work having such intentions, should of course proceed from one, whose opportunities for observation, in the *specialité* upon which he instructs and guides, should be well known to be sufficiently extended. That Dr. Churchill has possessed these opportunities, we will not doubt. But we hold it to be as equally true, that in writing for the class we have mentioned, the author should convey as much as possible the feeling, that in himself the reader is to place a very large amount of trust and dependency. Now we think that the feeling of this kind, which will arise on the perusal of Dr. Churchill’s book, will be of very slight intensity; the weight and personality of the teacher being so far merged in the responsibilities and confidence of other people, that the reader will be led to feel vast respect for, and surprise at, what every one else has done, except he who is teaching him. In our opinion, a work on practical medicine for the class we have alluded to, should contain just such an amount of extraneous counter-balancing opinions and labour, as is sufficient to lead the reader to see, that it will be of great use to him,—to use the words of Locke,—“to know the length of his line, though he cannot fathom with it all the depths of the ocean.” ’Tis well he knows that it is long enough to reach the bottom at such places as are necessary to direct his voyage, and caution him against running upon shoals that may ruin him.” (Human Understanding, Introduction, § 6.) In the chapter on *Pertussis*, for example, under the head of its pathology, the paragraph 355 contains about 80 full lines; and we meet with the following illustration of what we have above remarked upon:—“Linnæus maintained,” “Hoffmann attributed,” “Huxham thought,” “Dr. Watt judging,” “Dr. Dawson limits,” “MM. Marcus, Broussais, Boisseau, Guersant, Rostan, and Dugès regard,” “M. Danz places,” “Strong, Cullen, Astruc, Lettsom, and Darcy mention,” “Dr. Webster considers,” “Löbenstein Löbel met,” “Dr. Alcock states,” “M. Alph. le Roi agrees,” “M. Gilbert considers,” “M. Albers of Bonn found,” “Lænnec admits,” “Dr. Alderson makes,” “Dillon, Hufeland, Löbel, Breschet, Albers, and Eberle regard,” “Desruelles says,” “M. Blache is of opinion,” “Dr. Copland considers,” “Dr. James Duncan has recently proposed,” “broached by Volz,” “noticed by Jos. Frank,” “according to Dr. Duncan,” “Dr. Ley supposed,” “Dr. Fyfe in a late paper;” and, moreover, as helpmates in the formation of the piece of mosaic pavement which we are now treading on, the following names also lend their assistance, viz. Riverius, Dessault, Rosenstein, Sydenham, Butter, Waldschmit, Stoll, Webster, Breschet, Autenrieth, Kilian, Jadelot, Baron, Billard, Roe, Barrier, Rilliet, and Barthez. It may be replied, that we are not taking a fair illustration in selecting the “pathology” of whooping-cough; inasmuch as any attempt at its development, more especially if historically regarded, must necessarily include the detail of many various and contradictory opinions. If so, we dissent from the propriety of Dr. Churchill having made into an encyclopædic

dictionary, a treatise which seems intended as a handbook or guide for students and young practitioners. But we go further, and say, that the illustration we have given is not a bad example of very much of the whole work. Let us take the paragraph on the treatment of *Enteritis*; in it we find, "Dr. West speaks," "Riecke recommends," "recommended by Good, Dewees, Stewart, Condie," "Dr. Good gives," "Dr. Stewart alone," "Dr. Stewart speaks," "Dr. Eberle recommends," "Dr. Condie has found," "In bilious diarrhœa we are advised," "Dewees recommends," "M. Trousseau recommends," "Dr. Dewees advises," "Dr. Dewees ordered," "M. Trousseau states," "Dewees recommends," "Dr. Dewees and others," "Dr. Eberle recommends," "adopted by Dr. Parrish," "adopt Dr. Kuhn's," "Dr. Condie gives," "Eberle speaks highly," "Dr. Condie recommends," "Evanson and Maunsell suggest," "recommended by Dr. Condie," "Dr. Corrigan of this city," "Dr. West has remarked." As another and last illustration, we will take the paragraph on the "causes of *Pericarditis*," in it we are told, that "according to Rilliet and Barthez," "Puchelt however," "Dr. Todd's testimony," "Rilliet and Barthez's twenty-four cases," "Puchelt attributes," "Billard conceives," "Bouillaud considers," "Dr. Williams does not go so far," "Dr. Latham's experience," "Rilliet and Barthez found," "Dr. West mentions," "He adds," "noticed by Vieussieux," "Dr. West mentions," "related by Dr. Latham," &c. Hovering in doubt and despair amid the doctrines involved in such an array of the Æsculapian hierarchy, how happy would the student or young practitioner be, to have it in his power to follow the advice of Sterne, and give up the reins of his imagination into his author's hands. For a lecturer, however, we willingly allow, that such a bird's eye view of a variety of opinions, and minute references to their more complete exposition, as are given in the work before us, will be found advantageous in time of need.

We regret that we do not feel it due to ourselves, or to our readers, to arrest our general animadversions here. We are compelled to say, that the work of Dr. Churchill is incomplete, undue importance being given to the consideration of some maladies, whilst others, in our opinion more essential to be fully treated of, are too slightly passed over. The subjects of scrofula, general tuberculosis, phthisis, mesenteric disease, rachitis, rheumatism, purpura, and infantile syphilis, are altogether omitted; as are also those of epilepsy, paralysis, and night-terrors. Calculous disorders, enuresis, dysuria, and diabetes, are not alluded to; in fact, amongst the diseases of the "systems" treated of by our author, there is no place allowed to the genito-urinary system whatever. Albuminous nephritis finds its situation, along with œdema of the lungs, under the head of scarlatina. The interesting subject of sclerema, and its connexion with atelectasis, are unmentioned, along with other diseases not difficult to allude to. In the introduction to "Part II," we are told by Dr. Churchill that he will treat "of fevers, diseases of the eyes and ears, and other affections which do not admit of much classification." (p. 54.) Where are the diseases of the eyes and ears treated of? we cannot discover them. That some of the omitted subjects ought to have been dilated on, there can be no doubt; but with regard to others, we would have laid no stress upon their absence, if Dr. Churchill had not devoted thirty pages to the consideration of hooping-cough, and more than ten to that of porrigo,

whilst only twenty-five interpret the subjects of acute meningitis, acute arachnitis, acute hydrocephalus and their varieties, and barely ten illustrate the subject of fever; whilst, in other cases, the space is chiefly occupied by long tabular statements and extracts from foreign writers. A subject so important as fever in the child merits some fuller exposition than under the very vague title heading the seventh section of ten pages, viz. *infantile remittent fever, worm fever, gastric fever*. It is much pleasanter to us to turn to the following observations in our author's preface, and to state that on this point he has ably carried out his intentions:

"There is one portion of the history of infantile diseases which has hardly received the attention it deserves. I allude to the secondary affections,—those which occur in the course of other diseases, and are in some intimate but obscure way connected with them, almost in the relation of cause and effect. They complicate and often confuse the symptoms of the primary affection, always seriously increase its danger, and often render it hopeless of cure. Their early detection, or, what is far better, their anticipation and prevention, forms a very important part of the physician's duty; and I have endeavoured, as far as I could, to facilitate this object, by carefully noticing both the complications to which each disease is liable, and the primary disorders to which it may become secondary."

With these preliminary general remarks, we shall proceed in our analysis of specific details. The work is divided into two parts. The first embraces the "Management of Infants and Children." At the conclusion of some observations relating to the statistics of their mortality, it is stated:—

"The facts which have been laid before the reader, and which are not a tithe of what might be adduced,—are sufficient to show that a large proportion of infants are still-born; that another large proportion die in early infancy; that this proportion is vastly increased by bad management, and may be diminished by good management; and that there will still remain a large mortality from disease, arising from causes over which we have but little control, but which may still be diminished by judicious medical treatment. Thus the work before us is naturally divided into an inquiry, *first*, into the causes of disease during intra-uterine life, or immediately *after* birth; *secondly*, into the management of infants and children; and *thirdly*, into the diseases which are peculiar to or very prevalent during infancy and childhood." (p. 5.)

The first division occupies only between fifty and sixty pages; the rest of the work being allotted to that part which treats of special diseases. The chief points touched upon in the former, are those of the management of the infant at birth, the diet of infancy and childhood, cleanliness, dress, air, exercise, sleep, &c. Our own opinion is, that as both the *hygiène* of children from infancy to youth, and the pathology, &c., of their diseases, have now advanced to a very great extent, it is next to impossible to do justice to both in a single volume; and that it would be better to separate them for the future. The space occupied by the first part of the work we think might have been well displaced by a chapter on the method of examining sick children, and on the signs afforded by peculiarities of facial expression and general physical conditions of the body, as assisting us in the diagnosis of certain diseases. All who are acquainted with the study of infantile affections are aware, that there is no department of the healing art which, for its successful prosecution, more demands in the practitioner the cultivation of what may be termed a *clinical eye*. Without this, all hope of progress must be resigned, the most important symptomatic phenomena

will be passed over, and a world of trouble will be entailed upon him who might otherwise come quickly and safely to a right induction. If Dr. Churchill needed assistance in the formation of such a chapter, he would have found more than hints for it in the concluding section of the third volume of his favorites Rilliet and Barthez, in the second part of Bouchut, and in the introductory lecture to the prelections of Dr. West. As it is, a subject upon which we are inclined to lay the highest stress, receives no notice in the work before us.

We perceive that Dr. Churchill, although he does not think it *necessary* to apply a little whiskey to the head of the new-born child, does not believe it to be injurious, provided it be kept from the eyes; and advises castor-oil, with warm water and sugar, to be given to it after washing and dressing, &c. We think that, as a *rule*, the castor-oil is as unnecessary as the whiskey, but quite accord in the opinion that the oil is "a better medicine than either calomel, rhubarb, or magnesia." (p. 18.) Alluding to suckling, the author states, that—

"It is quite out of the question for any woman to nurse who is labouring under severe organic disease, fever, exhausting discharges, or complaints which are hereditary or transmissible, such as phthisis, epilepsy, scrofula, syphilis, mental disturbance, &c. Nay, even a known tendency towards these latter diseases, their occurrence in *any* member of the family, or a general delicacy of constitution, should give rise to very serious consideration on the part of the physician before he permits his patient to run the risk of perpetuating in her offspring such formidable disorders." (p. 23.)

Though we fully agree in much of the above statement, we cannot coincide in that part of it, which appears to us to teach that a woman is not to suckle her child, because her brother has had syphilis, or her sister epilepsy. In the following we go hand-in-hand with the author's advice:

"The term of nursing will depend upon various circumstances, such as the health of the child or mother, the abundance of milk, &c. &c. Some women are not able to suckle more than six or seven months; some continue for two or three years. I know a lady who nursed a child (now a tall, strong man) until he was able to draw down the blinds and bring her a footstool, previous to his taking his meal. Astruc and others advise nursing for two years, and the lower orders occasionally practise it to avoid pregnancy. But these cases are exceptions; and I believe it will generally be found, that nursing prolonged beyond twelve months is unnecessary for the child, and positively injurious to the mother in most cases. Taking this as one extreme, we may fix the other at nine months, and conclude that it is desirable that a child should not be weaned before nine months, nor suck after twelve. By this time he will be provided, generally, with a sufficient number of teeth to make use of the proper food, and he will have retained the comfort of suckling until he has passed through the first trouble of teething." (p. 28.)

We believe that, in the higher ranks of life, there is very little danger of over or undue lactation; and although occasionally we meet with it in the middle classes of society, it is not very often. But amongst females of the lower stations, this offence against health and propriety is one of constant occurrence, and entails an amount of disease and trouble, known only in its full extent to those whose duties lead them into daily contact with them and their children. How often do we not see a pale, thin, miserably clad and nourished woman, whose breasts are flaccid, nay almost wrinkled, suckling a child sixteen or eighteen months old, with many teeth; the mother suffering too from profuse leucorrhœa, or prolapsus uteri,

or phthisis, or dyspepsia, or with menstruation for some time regularly established; and more than half subsisting on tea and bread-and-butter. Of course, the chief purpose of this prolonged lactation is evident, viz. to endeavour to prevent the occurrence of conception; there is a secondary motive also,—that of keeping the child quiet during the night;—the child, for the attainment of such quietude, being allowed to remain with its mouth at the breast during the greater portion of that period, be it sleeping or waking. That, as has been commonly supposed, there is some functional antagonism between the mammæ and ovaria, is highly probable; but that the scale may be easily made to fall in favour of the latter is just as evident; and the researches of Mr. Robertson, as our readers know, have shown that more than 50 per cent. of the lower classes of Manchester become pregnant during the performance of the office of lactation. The inquiries of Dr. Laycock have further established the truth of the general fact involved in this assertion. The chief purpose in view, by the prolongation of suckling, is therefore easily negated; and as such prolongation is often attended with very serious effects upon the health of the mother and child, we should give it our greatest discountenance. In the greater number of cases, we believe that the health of the mother suffers more than that of the child, unless the latter be entirely confined to the breast. But it is not so usually, the child being generally fed as well as suckled. Of course, in those cases in which the mother is labouring under such a disease as phthisis, the child suffers nearly equally with her.

Dr. Churchill's observations on *artificial feeding* are judicious; and we fully agree with him, that although the earliest food should resemble the maternal milk, as nearly as possible, yet that it will be frequently found that artificial feeding—

“Differs practically from nursing in this, that whilst a child will thrive upon mother's milk alone for nine months or a year, it seems absolutely necessary to change the food occasionally, or the child will suffer from derangement of the stomach and bowels.” (p. 30.)

The following article of diet is most recommended, and is called by the author “bread-jelly.”

“A quantity of the soft part of a loaf is broken up; and boiling water being poured upon it, it is covered and allowed to steep for some time; the water is then strained off completely and fresh water added, and the whole placed on the fire and allowed to boil slowly for some time, until it becomes smooth; the water is then pressed out, and the bread on cooling forms a thick jelly, a portion of which is to be mixed with milk or water and sugar for use as it is wanted. The steeping in hot water, and the subsequent boiling, removes all the noxious matters used in making the bread, and it both agrees very well with the child, and the child likes it very much.” (p. 31.)

We are partial to a mixture of milk and isinglass. In a late part of the ‘*Journal für Kinderkrankheiten*,’ (a serial not generally met with, we believe, in this country,) there is a paper by Dr. Gumprecht of Hamburg, calling the attention of the profession to a novel kind of food, viz., carrot-pap (karotten brei). It is true that, as far back as 1819, Friedländer, in his work, ‘*Sur l'éducation physique de l'homme*,’ remarked, “On fait en Vallachie du bouillon avec des carottes;” and, in 1830, Schmidtman, in his ‘*Summa observationum Med.*’ stated that, in Turkey, carrot-pap was highly prized as a nourishment for young children; yet it is due to

Dr. Gumprecht to say that all credit attaches to him for bringing it prominently before the notice of the profession, and for its present use in Germany, through the recommendations of some of the first authorities there. At the request of Gumprecht, Müller of Hamburg, Mauthner of Vienna, Münchmeyer of Lüneberg, &c., have employed it, and speak highly of its value. Wakenroder gives the following analysis of the expressed and inspissated juice of the carrot :

Oil, fatty	} 1.00	Sugar	} 93
Oil, ætherial		Malic acid	
Albumen, veg.	4.35	Starch	
Karotin	0.34	Lime	
		Alumina	
		Oxide of iron.	

But as the scraped root of the carrot contains other matters than the above, especially a large quantity of ligneous substance, which cannot be digested, and which will remain often two or three days in the intestinal canal before being discharged, and be highly detrimental to the delicate mucous membrane of children, it becomes important to separate that which will be useful from that which will not. Gumprecht advises an ounce of finely-scraped full-grown carrot to be mixed with two cupfuls of cold soft water, and allowed to stand for twelve hours, being frequently stirred during this period. The fluid portion is then to be strained off, what remains being pressed to yield some more. This fluid is then to be mixed with the proper quantity of biscuit powder, or bruised crust of bread, or arrow-root, &c., and the pap placed over a slow fire, until it begins to bubble. Care must be taken that the heating be not pushed so far as to cause boiling, or the albumen will coagulate. After its removal from the fire, it is to be sweetened with a due amount of white sugar. Dr. Gumprecht states, that by mixing the carrot juice with biscuit, crust of bread, or arrow-root and sugar, we obtain all the farinative and nutritious elements required, viz., albumen, gluten, starch, sugar, fat, and the phosphates of lime and magnesia. This food is more particularly adapted for children who have been suckled and are being weaned; for those who are being brought up by hand the following preparation is deemed more advisable: an ounce of *very-finely* scraped yellow carrot, and two drachms of biscuit-powder, are to be mixed with two cupfuls of cold soft water. This must stand in a covered vessel, in a cool place, for twelve hours, and be frequently stirred during this time. It is then to be drawn off, or strained through a linen cloth. Some sugar-candy and a pinch of salt are to be added to the fluid, which may then be administered by means of the sucking-bottle; care being taken that the food is at the proper temperature. We may state, that Zeise of Altona, (Annal. v. Wöhler u. Liebig, 1847,) has given a scientific reason to support a practical belief common in Germany, viz., that the carrot is a good anthelmintic. He states the elementary composition of *Karotin* to be  $=C^5, H^8$ , and polymeric with the oil of turpentine and its usual contained matters. Upon the value of the carrot in fattening animals, we refer our readers to the little work of Dr. Lankester, 'On the Food of Man.' We must also observe that the above preparations of the carrot are contra-indicated as diet, when there is any tendency to diarrhoea; and that Mauthner and Münchmeyer, who employ them, believe that another drawback to their use is the trouble

which their preparation involves to the poorer class of mothers who are often careless and indolent.

For the nipple of the sucking-bottle, Dr. Churchill prefers the calf's teat to one made of chamois leather, or ivory made flexible and elastic by the removal of its earthy matter, or M. Dardo's nipple of thin elastic cork. The remarks on the subject of *dress* are judicious, and particularly so those on teaching children to walk. In the limited space which has been set apart for the consideration of *hygiène*, the subject of *light* perhaps could not have been treated at greater length than it has been; and we are glad to find it has been even cursorily noticed. Its influence, merits far more attention than it generally receives from writers on *hygiène*, who, though fully impressed with the necessity of enforcing proper diet, clothing, air, exercise, &c. rarely take sufficiently into their consideration the power which strong solar light exerts on the living organism, and its importance as a stimulant and promoter of its highest energies. Whilst air and exercise are recommended, of course the exposure to a brighter light is involved, if not implied by most people; but we consider the subject as meriting a distinct chapter, as much as *sleep* or *diet*. Its full appreciation, however, requires a more extended knowledge of comparative physiology than many such writers appear to evince. We can add our commendations of the account given of *nurseries* and *nurses*.

We pass now to the second and more important part of Dr. Churchill's work. This is divided into eight sections, each section embracing an account of the diseases affecting particular systems—nervous, respiratory, &c. In the first chapter of each section, the author notices the diseases by which the *fœtus* is more frequently attacked during intra-uterine life; he then treats of those which it presents to us at birth, whether of long-standing or acquired during childbirth, together with certain malformations which require treatment; and, lastly, enters at length upon the special affections of infancy and childhood. The diseases of the cerebro-spinal system occupy the first place. Amongst intra-uterine and congenital diseases, the following are alluded to—*convulsions*, *hydrocephalus*, *absence of brain or skull*, *encephalocele* and *hydrorachitis* or *spina-bifida*. We hold the same opinion with regard to the ability of properly treating these subjects in the same volume with the extra-uterine affections, as we do with regard to *hygiène*. The necessary consequence of attempting the combination is incompleteness and imperfection somewhere.

The following embraces the author's consideration of intra-uterine convulsions:

"Many authors have maintained, that the *fœtus in utero* is subject to epileptic or convulsive attacks. Duettel states 'Nullus autem affectus familiaris solet esse proli in utero quam epilepsia.' And Segerus relates a case of a pregnant woman who suffered severely from this disease, and in whom the *fœtus* exhibited similar convulsive movements. Lowenheim held that it was not uncommon, and Hoogveen and Feiler relate examples. Hufeland thinks that these convulsive movements are dependent upon or derived from the mother.

"There are few practitioners of any standing who have not been consulted on account of the distress caused by the violent movements of the *fœtus*; in many cases the annoyance arises from excess of uterine sensibility, but in others the movements appear to be excessive, irregular, and of temporary duration, subsiding after a time to return in moderate degree, or to recur again in paroxysms, or perhaps to cease altogether. Such cases I have several times observed, and in the



latter instances the child has been still-born, and the period of its death referred to the close of the violent convulsive movements. Whether these are cases of epilepsy, may of course admit of doubt; but it is of little consequence, as during intra-uterine life nothing remedial can be attempted." (p. 55.)

The following extracts are the only physiological observations which the author ventures upon, in relation to the origin of the several affections whose names we append; all further remarks relate chiefly to cases and to treatment when advisable:—

*Hydrocephalus.* "Some writers have attributed it to an arrest of development, but this appears quite inadequate; it appears more likely, as Rudolphi has observed, to be a special disease arising from excessive congestion of the membranes or from inflammation." (p. 56.)

*Absence of brain or skull.* "That these are cases of arrest of development and not the result of disease, can admit of no question." (p. 56.)

*Hernia Cerebri.* "This malformation is also due to an arrest of development in the ossification of the cranium, by which the fontanelles and sutures are left incomplete." (p. 57.)

*Spina-bifida.* "Depending as it does upon an arrest of development in some portion of the spinal canal." (p. 59.)

Upon some points connected with the origin of the above, we shall add a few more observations.

Cotunnus was, we believe, the first observer who drew attention to the fact of there being, in the normal condition of the nervous centres, a fluid present in the cavities of the brain, and beneath the subarachnoid covering both of it and of the spinal cord. Magendie elaborated the subject, and termed the fluid the cerebro-rachidian. Magendie considered two ounces to be its ordinary amount, Cotunnus four. According to the former, the fluid in the ventricles communicates with that in the cerebro-spinal subarachnoid space, and accumulates in the greatest quantity where that space affords greatest capacity. On these points, the reader will find full information in Dr. Todd's 'Cyclopædia of Anatomy,' &c. A few years ago it was stated by Breschet, after his examination of very many bodies at the Hôpital des Enfants Malades, that in the foetus, in infants of the full time, and in children of from six months to a year old, a certain though variable amount of fluid is to be found in the anterior or median ventricle, or the cavity of the septum lucidum. This cavity, well described by the brothers Wenzel and Tiedemann, is found to be larger, the younger the child. Morgagni gave the first hint that too great an effusion of the cerebro-rachidian fluid might produce an important disease; and Breschet particularly called attention to the existence of this natural hydrocephalus (as he terms it), as not being directly a disease in itself, but as a circumstance disposing more or less to pathological phenomena, and therefore to be regarded as an important point in reasoning upon the cause of intra-cranial effusions in general, and especially of congenital hydrocephalus. The way thus paved by Morgagni and Breschet, has lately been trodden by an able and acute observer—Dr. Behrend, one of the editors of the 'Journal für Kinder Krankheiten.' His paper will be found in the second volume of his journal for 1849. The design of it—founded upon several cases—is to show that spina-bifida, congenital hydrocephalus, and encephalocele, arise from one and the same cause; and a highly-interesting case that occurred to him, in which the two former affections were present with hyderencephalocele over the brow,

seems to our mind satisfactorily to support the point which is maintained in the paper in question. Our further remarks will bear reference to the three diseases just mentioned.

In *spina-bifida*, we find a greater or less deficiency of the bodies of the vertebral column, or a want of union between certain of them, accompanied by an external tumour over the part containing fluid, with (in rarer cases) a portion of the spinal cord. Dr. Churchill observes :—

“The different degrees have been grouped into three classes, by Fleischmann and others :

“1. When the entire vertebra is divided ; this is extremely rare. Ollivier recites three cases related by Tulpus, Malacarne, and Zuringer, in which it existed.

“2. An absence of a greater or less portion of the lateral arches of the canal ; this is the most common variety, and

“3. Where the arches are well developed, but without union posteriorly ; here, however, the separation can be but a few lines, resembling a groove rather than an aperture. Ruysch, Acrell, and Isenflamm have each described a case of this kind ; the former in the lumbar region, the second in the sacrum, and the latter in the first cervical vertebra.” (p. 59.)

The absence of the spinous processes does not necessarily imply a communication with the interior of the canal.

“Beclard has found them absent several times as a simple malformation, the bodies preserving their integrity.” (p. 60.)

In *encephalocele*, we also find a deficiency of the bones of the skull, and an external tumour over the part, containing either fluid alone (*hyderencephalocele*), or fluid with a greater or less amount of cerebral matter. In *congenital hydrocephalus*, we find a greater or less amount of fluid, either in the cavity of the arachnoid, in its subcellular tissue, or in the ventricles, and often accompanied with more or less distension and expansion of the cranium, &c., from the pressure of the fluid within. Now it may be asked, —is there not a common cause at the base of these affections, or at least between the former and the latter, and that variety of *encephalocele* termed *hyderencephalocele*? We agree with Behrend, that in all probability there is, —that, viz. hinted at by Morgagni, more prominently alluded to by Breschet, and further illustrated by Dr. Behrend himself. We think there is sufficient support for assuming, that during intra-uterine life, in consequence of an abnormal activity of the vessels of the meninges, a superabundant quantity of the cerebro-rachidian fluid is effused ; and that this fluid, normal when in proper quantity, presses unduly against the ossific walls, breaks through them where it finds least resistance, and pushes before it the membranes of the nervous centres, causing them with the integuments to protrude in the form of a tumour ; the particular affection which is found being determined by the particular seat and extent of this fluid, and by the point at which it meets with least resistance. In Behrend's case, when the fluid was evacuated from the tumour over the spine, the tumour of the forehead gradually diminished, and the distended and protuberant fontanelle fell. It was clear to the observer, that there was an intimate connexion between the three phenomena. It might be replied, that in *spina-bifida*, and those cases of *encephalocele* in which a solution of continuity, not sutural, or deficiency of bony walls exists, such conditions of the latter, and the permission of protrusion through them, are rather dependent, primarily, upon an incompleteness or want of progressive perfect per-

formance of the action of *osteosis*, than on any local influence exerted upon the cranial and vertebral walls, in perfect independence of whether the action formative of bone be normal or otherwise. But since, in Behrend's case, none of the bony tissues of the skeleton showed any signs of want of that amount of osteosis which should be expected to exist in relation to the age of the child, except in the particular spots of the cranium and spine where protrusion existed; and since in many other cases of various forms of encephalocele and congenital hydrocephalus, with deficiency of bony continuity, other than sutural, the like fact existed; and not only this, but as shown by Aurivill, Malacarne, Hartell, and others, that save in those localities immediately involved, the bones of the skull have been in many cases thicker than ordinary,—we believe that we have sufficient reason for assuming, that the protrusion of the external tumour does not primarily depend upon a low development of the power of ossification. On the contrary, we believe the power of forming bony matter to be quite in its normal intensity, and that if it were even much stronger, the pressure of the fluid would interrupt its action locally, absorption of the earthy matter would ensue, and a retrograde metamorphosis take place in the cartilaginous and ossific formations. We may observe, too, that it has been stated by Behrend, that in cases of *hemi-* and *anencephalia*, where originally intra-uterine hydrocephalus existed, and the calvarium became not only thinned, separated, or fissured, but quite disappeared, and the brain also, the rest of the skull was almost always unusually thick. Why in one case the pressure of the fluid should be mainly exerted at the top of the cranial vault, in another at the occiput, in a third at the brow, in a fourth at the temporal bones, or why the rachidian fluid should be more effused than the cerebral (*or vice versa*), and press in one instance on the lumbar, or in another on the cervical spine (Ollivier's case), we do not pretend to say. Sometimes the distension of the parts which ensues in congenital hydrocephalus is equal; in other instances, the direction of the pressure is much stronger towards one side. In many of these latter cases, it is evident that such direction of pressure in a particular line is dependent upon the greater amount of effusion on the one side than on the other. Klein found the left side of the encephalon widely distended, presenting the appearance of a mere bladder, whilst the right half had not departed from its natural form. In another case, the left ventricle was so distended as to reach the bulb of the olfactory nerve.

We do not, however, mean to deny that there do occur cases of 'vices' of conformation of the cranial bones, which are independent of a mere local pressure from fluid within, and which may even permit of the existence of some forms of encephalocele. But we believe, that in the varieties of intra-uterine and congenital disorder we have more especially alluded to, their theory of formation is such as we have cursorily hinted at. Even in cases like the one mentioned by Billard, in which there was complete absence of the petrous portion of the left temporal bone, and through the space thus formed, a considerable portion of the brain protruded in a sac formed by the meninges and skin, but void of true scalp, it is probable, as the relater observes, that there existed in the uterus some kind of projection, as a polypoid growth, &c.; which, coming into contact with the part of the cranium alluded to, had caused destruction of the scalp, &c.; or else that there was some vice of conformation of the pelvis

of the mother, which caused pressure to be exerted on a particular portion of the uterus as it enlarged.

On the treatment of *encephalocele*, Dr. Churchill remarks :—

“All writers, I believe, are agreed, that the best mode of treatment consists in the application of gentle and equable pressure. M. Salleneuve used a piece of thin sheet lead, softly padded, and fastened to the child's cap at the part corresponding to the tumour, and the pressure was increased or diminished by tightening or loosening the cap. By thus depressing the tumour gradually, and without injury, an opportunity is given for the growth of the bone, and the completion of the defective space, which of course is the radical cure of the hernia. M. Salleneuve related a case which was thus cured, to the Royal Academy of Surgery of France. Callisen and Sanson concur in the propriety and feasibility of this mode of treatment when the tumour is small; but when it is large, and at the occiput especially, little more can be done than some contrivance to protect it from injury.” (p. 59.)

As applicable to those cases in which very little or no fluid exists in the tumour, but chiefly brain, and to those in which the diagnosis may be difficult, we agree in the advice above given; but with respect to the treatment of others, in which a considerable amount of effusion exists, and very little cerebral matter, or fluid alone (*hyderencephalocele*), something further might be added. Of the value of compression applied indeed to any form of the disease, we have no great amount of *experiential* proof. When Salleneuve and Callisen wrote, sanguineous tumours of different parts of the cranial coverings were not unfrequently confounded with forms of *encephalocele*. This Cloquet was aware of. We admit, however, that one of Mr. Lyon's cases fully proves the value of compression in relation to *hyderencephalocele*. But we have some facts on record, which seem not entirely to abrogate a further trial of the treatment by puncture in the latter affection; they are not very strong in its favour certainly, but we think they are sufficient to show, that where the contents of the tumour are entirely fluid, or much fluid and a small portion of brain, and compression alone is unavailing in reducing them, the idea of puncturation is not entirely to be set aside. We particularly refer to accounts of cases so treated by Lyon, Adams, Earle, and Dendy; and to the deductions to be arrived at from reasoning by analogy on the case of *spina-bifida* punctured by Abernethy, one so treated by Sir Astley Cooper, and another more lately by Dr. Steevens of New York; as also from some of the instances in which chronic *hydrocephalus* has been favorably subjected to a like procedure. In Mr. Lyon's case, when the *dura-mater* and *pia-mater* lined the interior of the tumour, the latter was first punctured on the right side of the nose, five days after on the left side, three days later the child died. In Mr. Adams's example, the tumour underwent puncturation seven times with success, allowing the escape of fluid each time, but finally leaving a solid tumour apparently formed by a protruding portion of brain. Mr. Earle repeated the operation nine times, the child surviving nearly two months from the first puncturation. Mr. Dendy during the space of about nine days, punctured thrice with a grooved needle, allowing the escape of almost twelve ounces of fluid, the child dying on the tenth day. Mr. Abernethy punctured the swelling of a *spina-bifida* “every fourth day for six weeks. The wounds generally healed very well, but at last one of them became ulcerated, the discharge became purulent, and the child died.” (Churchill.) Sir Astley performed the operation in

the like affection six times, and applied pressure; the patient at twenty-eight years of age being in active and perfect health. Dr. Steevens's successful instance, with others, we shall notice presently. Certain of these cases are, we assume, sufficient to prevent us at present from coinciding with that opinion which would entirely limit the treatment of all forms of tumours connected with the interior of the cranium to that of compression, some of them, at least, proving that the coverings of the nervous centres may be punctured with impunity and benefit.

Our author, after noticing the various proposals of Richter, Forestus, Bell, and others, for the cure of spina-bifida, and the successful case which was punctured by Sir Astley Cooper, thus writes:

"The operation by puncture has been several times performed since, but without success. Dr. Sherwood tried it and failed. Otto punctured the tumour in a child also affected with hydrocephalus, and the tumour disappeared, but the child died three weeks afterwards. Pliny Hayes lost a patient in two days after a single puncture. In 1819 Dr. Berndt failed in three cases; the first died twelve days after the operation, the second after three weeks, and the third after three punctures. Benedict Trompei performed the operation upon a girl of six years old with a cataract needle, and she died comatose thirteen days after. I tried the same plan three or four years ago, and the tumour was becoming more solid, so that I began to have some hope of success, when the child was seized with convulsions and died.

"Still, small as the chance is, it would appear, that of all the methods proposed, the most feasible is compression alone, or combined with acupuncture.

"In the 'New York Journal of Medicine,' for September, 1843, a case is related by Dr. Steevens, of New York, successfully treated by puncture alone. The tumour was about three inches and a half broad from side to side, and it was punctured three different times, and more than nine ounces of fluid escaped. After the last operation the sac inflamed, and the child became irritable and restless; but these symptoms soon subsided, and a year after nothing remained of the sac but a small bunch of indurated and corrugated integument. The child was eight months old." (p. 65.)

We may add, that MM. Robert and Rosetti, and Professor Rugieri, have each successfully employed the combination of puncturation and pressure.

The author gives but slight consideration to the subject of the "absence of brain or skull," and leaves unnoticed the spinal cord. We could scarcely expect him to find room for much detail on these matters; but, as he systematically includes the general subject of intra-uterine disease in his work, we think he might have given a sketch of the more important "vices" of the nervous centres. We will supply a slight one. There is only one case on record that we are aware of, of *aneuria*, or complete absence of brain, spinal cord, and nerves, in a monstrous foetus. This is alluded to by Fabre as being recorded by Clarke in the 'Philosophical Transactions' for 1793. How far this case was truly what it was represented to have been, may admit of some doubt. From the time of Morgagni numerous instances have been given of *amyelencephalia*, or absence of the brain and spinal cord. Whether these are cases in which no development of central nervous matter has ever taken place, or whether it has, and this matter has afterwards become destroyed by increasing dropsy (if we may so call it) of the cerebro-rachidian fluid, must at present be considered a question *sub-judice*. We incline to the latter opinion. The acute remark of Fabre, that as yet no author has described the non-existence of the spinal

nervous centre in the embryo, but that all known examples refer to the fœtus when seven, eight, or nine months old, seems to support the view, that the causes determining the absence or destruction of this organ are only developed at a period more or less distant from its first formation, and that therefore its absence is never primitive. Complete absence of the brain, of particular parts of it, and limited vices of conformation, are not unfrequently met with. It is doubtful whether there is a sufficiently trustworthy example recorded of *amyelia*, or absence of the cord, the brain being present; Morgagni, copying Rayger, has given two such cases; but the opinion of one of the highest authorities on these matters, Ollivier, is, that the description of them is not complete and precise enough to warrant their acceptance. Illustrations of *atelomyelia*, or imperfect conformation of the spinal centre, are by no means wanting. Generally speaking, absence of any considerable portion of the encephalon is accompanied with that of the walls of the cranium; and when both are extreme, they constitute a case of *acephalia*, or headless condition. In *amyelencephalia*, separation of the parts of the vertebræ, either through the whole length of the spine, or in parts of it, always occurs; but not constantly with protrusion of a membranous sac, as in *spina-bifida*. It has been affirmed, that when the brain and spinal cord are both absent, neither cranial cavity nor spinal canal can exist; this assertion is opposed by Fabre, and is entirely unfounded.

The chapter on *cephalæmatoma* is a good one, and sufficiently complete. Since it was written, however, Nevermann has published a paper on its treatment, which we recommend as a useful addition to Dr. Churchill's remarks.

In allusion to the treatment of one form of "irritation of the nervous system," we meet with the following highly judicious advice:

"As this is the first time I have had occasion to mention leeches, let me recommend to my readers, that *in all cases where they are applied to infants or children, the bleeding should be arrested at once when they fall off*. By so doing we can estimate exactly the amount of blood lost, and we avoid the great mischief of continued draining. Of course it will be necessary to apply a greater number of leeches than usual, or to repeat them; but that is of no consequence, compared with the danger of the ordinary method." (p. 78.)

Whilst we agree with the main principle here inculcated, and always enforce it in private practice, yet we are constrained to acknowledge, that sometimes in public practice we are obliged to act differently. This arises from the fact, that those who have to *pay* for the leeches, will not, and often cannot, admit the full force of the latter part of Dr. Churchill's statement. If the author, like ourselves, has ever belonged to a public charity deficient in funds and regarding the expense of leeches with no very favorable eye, or has largely attended the poorer classes under circumstances when they had themselves to purchase the leeches, we think he will pardon us when we confess that occasionally we have ordered half the number otherwise advisable, and directed the "draining" to be allowed to continue for a short time after their removal from the part. The clumsiness and the impatience of the mother, and the uncleanness of the child, will frequently prevent some of those we do order from biting; and hence only half the business would be effected in some cases, were no draining to be allowed. True, it would be better that the mother should return to the charity for more leeches, or buy them herself; but where is the charity that fully

recognises such a rule, or where is the poor mother that can or would so readily purchase them? The practice we are too often forced to adopt, is, we admit, bad in its principles; but we often feel that the exigencies of the case compel it. We always urge upon the nurse not to put the child back into a warm bed immediately after leeching; or, if such is necessitated, to watch it carefully.

We are of opinion that the treatment advised in the "second variety" of nervous irritation,—or, as we would rather express it, the state of exhaustion after such irritation—which is the *hydro-* and *hydrencephaloid* disease of different writers, is hardly sufficiently forcible in relation to support and stimulants:

"Neither depletion nor any exhausting remedies are admissible; on the contrary, it is absolutely necessary to administer good though bland nourishment. Chicken-broth, or nicely made beef-tea must be given frequently, but in small quantities, and after a time a little wine whey or wine and water." (p. 78.)

We have found that *beef-gravy* with the fat skimmed off, and ammonia, its aromatic spirit, æther, or cinchona, are generally required in these cases.

We shall pass over the subjects of *trismus*, *chorea*, and *convulsions*, with a brief comment on the following passage, which we have noted in the chapter on chorea:

"In other cases, the paroxysm consists in hammering the knees with the hands, or of a constant series of bowing. Such cases as these latter, however, have one peculiarity not usual in chorea, i. e., the muscular movements appear independent of the will, neither excited by it, nor under its control; and it is almost certain that in some of these cases the patient is unconscious." (p. 87.)

We presume that Dr. Churchill has here had in view, when alluding to the "constant series of bowing," examples of what has been termed the "*salaam convulsion*" by Sir Charles Clarke, and *eclampsia nutans* by Mr. Newnham, who has ably illustrated this peculiar affection in a paper detailing four cases in the 'British Record of Obstetric Medicine.' Mr. West, of Tunbridge, was the first person, we believe, who drew attention (in the *Lancet* of 1841) to it, and it was exemplified in his own child. A paper was lately read upon the subject before the Westminster Medical Society, by Dr. Willschire, who added another case to the only four British examples on record, given by Mr. Newnham.\* If Dr. Churchill has had these latter cases in mind, or others analogous to them, we certainly coincide with him that they were attended with peculiarities not usually seen in choreic affections; and we are of opinion that they have no right to be placed under the head of chorea. The general convulsions which sooner or later accompany the "*salaam*" movement of the head, sometimes partake of an epileptiform, sometimes of a tetanoid character. Moreover, paralysis and complete idiocy have followed.

Our author's sixth chapter is headed—*acute meningitis, acute arachnitis, acute hydrocephalus*. We would add the following from Morgagni as its very fitting motto: "*Hydrocephali nomen etsi unum est; plures tamen inter se discrepantes affectiones significat.*" (*De Sedibus et causis Morborum* Sp. xii.) In this chapter are described four "phases of the

\* Some cases have been recently recorded in American journals, under the designation of *eclampsia nutans*, but the accounts of them are not sufficiently precise to satisfy us that they were really examples of this disease.

same disease." (p. 115.) The "symptoms and the post-mortem appearances indicate an affection of the membranes of the brain as the essential character of the disease, whether primary or secondary." The four phases of one and the same disease particularly dwelt on, are:—*First*. That phase which "corresponds to the acute hydrocephalus of Gölis and others, and the "meningite simple aigue" of Barrier and Rilliet and Barthez. It is not the most common, but is very far from being rare." (p. 117.) *Secondly*. That "phase or form," which, from Dr. Churchill quoting from Dr. West at the commencement of his description (p. 121) as illustrative of it, we of course assume to correspond to the disease which Dr. West is describing, and which is the "acute hydrocephalus" of this writer, considered by him as essentially the same with *tuberculous meningitis*. The following quotation from Dr. West's account of it clearly indicates this :

"We are thus led to the conclusion, that inflammation of the brain occurs in early life under two different conditions. It now and then comes on in previously healthy children, but occurs much oftener in connexion with the tuberculous cachexia, or as the result of tuberculous deposit in the brain or its membranes. The term encephalitis may be properly used to denote the cases of simple inflammation of the brain, while we may with advantage restrict the term *acute hydrocephalus* to cases of cerebral inflammation in scrofulous subjects, owing to the extreme rarity of the former affection, &c." (Lecture V.)

This phase, which also corresponds (Dr. Churchill tells us), "with Dr. Cheyne's first species, will, with some little modification, apply to those cases in which the meningitis occurs in the course of measles, scarlatina, or infantile remittent, or when it is secondary to disease of the bowels or liver." (p. 123.) *Thirdly*. The "next form" is the "*tubercular meningitis* of the French authors," (p. 124,) which, we may remind our readers, is identical with Dr. West's "acute hydrocephalus," the description of which has already been appealed to by Dr. Churchill to assist him in illustrating his previous variety. *Fourthly*. "The last form of the disease" is the *water-stroke*. (p. 126.) Under the head of *pathology*, after alluding to injection of the membranes, effusion of fluid, purulent and concrete matter, softening, &c., it is stated :

"These post-mortem appearances are more or less common to all the forms or varieties of hydrocephalus I have noticed ; but others are superadded in *tubercular meningitis*." (p. 128.)

Accordingly, we find the morbid anatomical phenomena met with in the different affections described, all massed together, the super-additions in tubercular meningitis then added to them, and the following *résumé* given :

"There appears but little difficulty in deciding upon the nature of the disease. In each form we find traces of inflammation in the membranes of the brain, with the results in the form of serum or pus ; in some we find a deposition of tubercular matter, but whether that be the result of inflammation or not is not as yet decided. M. Trousseau believes that they are [it is ?] so ; but Rilliet and Barthez incline to the opposite view. M. Bouchut considers them [it ?] a constitutional disease. In addition, we find the substance of the brain occasionally partaking of the inflammatory action." (p. 129.)

Although we accord with Dr. Churchill in the opinion that the various forms of encephalic affection in children included by him in his present chapter,—which forms, as described by him, we have had very great



difficulty in recognising, unravelling, and reducing to our own types,—have, as their primary and chief lesions, morbid conditions of the circulation in the meninges, yet we cannot but regard his general exposition of the important subjects embraced in this chapter as deficient in clearness and accuracy, even admitting his general views to be correct. From its commencement we became anxious as to what would follow :

“Modern writers, indeed, particularly the French, have drawn a marked distinction between acute meningitis and tubercular meningitis; but as the most intelligent of them confess that they differ only in the pathological condition of the membranes, and not essentially in the symptoms or the course of the disease, I have thought it better to include both under the one name.” (p. 115.)

“An attempt has been made to distinguish between simple acute meningitis and tubercular meningitis, but I think without success except in extreme cases.”——“In the majority of cases the course and symptoms are so similar, that unless we have some collateral circumstances to guide us (as for instance, a disposition to tubercularization in other localities, or a strongly marked scrofulous diathesis), I do not think any positive diagnosis possible.” (p. 133.)

We differ from the above judgments of our author most decidedly; and we are astonished how Dr. Churchill, conversant as he appears to be with the writings of Rilliet, Bouchut, West, and others, who afford ample proof of the propriety of the great stress they lay upon distinguishing between these affections, and (as we otherwise should have assumed) not himself practically ignorant of their symptoms, could have arrived at them. If it be replied that we cannot gainsay what practical experience has really taught Dr. Churchill, so far as he himself is concerned, we must still express an astonishment at finding that such experience is totally opposed to that of the best authorities in this country and on the continent. Our astonishment would not yield, were Dr. Churchill to arrive at the following conclusion after double the amount of evidence he affords of careful observation and power of analysis of symptomatic phenomena :

“Moreover, it must strike every one that between several of the forms here described, there is very little difference of symptoms, nor do I think that they will be found more unlike in practice.” (p. 126.)

The only methods by which we could arrive at such a belief, in relation to simple acute, and tubercular meningitis, must be one of these two; either to attach a different meaning to the term *symptoms* than we have hitherto attached to it, or else to rest satisfied with a knowledge of the diseases in question as drawn from our author's description of them. Dr. Churchill not unfrequently quotes from Mauthner; but as his references are always (we believe) to a review of his work in one of our fore-runners, we presume he has not had the advantage of referring to Dr. Mauthner directly. If he had, he would have found the following motto prefixed to this author's chapter on “acute hydrocephalus, or Whytt's disease,” and which we think not unworthy of attention.

“According to a universal law, science first appears to arrive at distinctiveness, by opposition and separation.” (Marcus und Schelling.)

Alluding to *tubercular meningitis*, we are told by Dr. Churchill that :

“From the researches of late years it would appear that this form of the disease is much more frequent (at least on the Continent) than any other.” (p. 124.)

"Hydrocephalus consists essentially in inflammation of the membranes of the brain, occasionally accompanied by a deposition of tubercular matter." (p. 134.)

Surely from this the reader would little expect, that in our own country at least 90 cases out of 100 of the so called "hydrocephalus" are cases of scrofulous meningitis, which would by those conversant with children's diseases be diagnosed as such easily; and that at least in eighty out of the ninety, after death, tubercular deposit would be found in the meninges, whilst, although no tubercular granulations might be apparent in the other ten, yet the symptoms during life, and the lesions which are found after death, would afford ample proof of the rectitude of the diagnosis arrived at.

The remaining ten of the 100 would be equally well declared to be cases of simple acute meningitis.

In connexion with the differential diagnosis of "hydrocephalus" the author writes:

"Infantile remittent or gastric fever seldom presents sufficiently marked head symptoms to be mistaken for hydrocephalus, until towards its termination." (p. 132.)

This does not accord with our own experience; on the contrary we have anxiously pondered at the bed-side, early on the invasion of the febrile affection, as it turned out to be, as to what should be our diagnosis.

With respect to prognosis:

"I have seen a considerable number of cases; and although when symptoms of effusion are present, the case is hopeless, yet at an earlier period I have succeeded in curing as large a proportion as in other diseases of equal importance and severity.....When attacked early, a considerable proportion of cases may be cured." (p. 134.)

The first statement seems to us very vague. What diseases are of equal importance and severity with tubercular meningitis, and what proportion of them is cured? Certainly neither pneumonia or croup are equal to that malady in its fatal tendency, nor in our opinion are peritonitis or gangrena oris. Phthisis *may* be, but we would much sooner have to deal with a not very far advanced case of it, if we had cod liver oil, country air, and good general hygienic arrangements at our disposal, than with tubercular meningitis in any stage.

To our minds the principles of treatment inculcated are as unsatisfactory as the pathology. We are first told:

"Many children are lost by the usual moderate remedies, who might be saved if more active measures were adopted. Let me illustrate what I mean by a case:—my friend Dr. M'Donnell's child, aged four months, strong and healthy, was suddenly attacked by acute meningitis of the most severe character. Six leeches were applied immediately to the forehead and the bleeding stopped; the convulsions became less frequent and the fever diminished; in about eight hours six leeches were again applied, and we found that the convulsions did not return, but the starting and crying and restlessness continued; and, consequently, after the lapse of six or eight hours, we repeated the six leeches, *i. e.*, eighteen in twenty-four hours, stopping the bleeding as soon as the leeches fell; and from that moment all the symptoms rapidly subsided, and the child recovered his health in two or three weeks." (p. 135.)

That this was a case of simple acute meningitis we believe, and that depletion was its proper treatment, we, of course, assent to; but it was

not ordinary acute hydrocephalus or tubercular meningitis. However, as a comment on the above, we are told that :

"Of course one rule will not apply to all cases ; many things must be taken into consideration : first, the constitution of the child ; secondly, the cause of the disease ; thirdly, whether the disease be primary or secondary ; and lastly, the period of the attack at which we are called to the child. These circumstances will necessarily modify the treatment. You cannot bleed a child of a weak constitution so extensively as one who is strong and healthy ; nor does the disease, when secondary or in an advanced stage, admit of such active treatment. Let us examine the principal remedial agents in use." (p. 135.)

Bloodletting is first examined ; and we find that :

"In all forms of the disease, whatever be the constitution of the child, whether the disease be primary or secondary, *if the attack be recent*, I believe bloodletting to be necessary, either by opening the jugular vein or the vein in the arm, by cupping or by leeching. And the quantity taken should be, in most cases, larger than in other diseases, or even large in proportion to the age of the child. Moreover, if the good effect be not produced, and the child able to bear it, it should be repeated three or four times." (Ib.)

Further on, as if tubercular meningitis had been forgotten, or not meant to be included in these directions for the treatment of "all forms of the disease," it is remarked :

"In tubercular meningitis, bloodletting is also necessary according to the age and strength of the patient, and the intensity of the disease ; but Rilliet and Barthez do not think it should be carried to so great an extent as in acute simple meningitis." (p. 136.)

We should think not either, and coincide with Dr. Churchill entirely in this, that :

"In the more advanced stage of either variety it is rarely of any use, and may, perhaps, do injury by reducing the strength of the child." (p. 136.)

That bleeding in simple acute meningitis is proper, we do not for a moment deny ; but we express a strong conviction, derived from experience as well as from reading, when we say that bleeding in acute hydrocephalus or tubercular meningitis, carried out to the extent indicated by Dr. Churchill, is by no means advisable, whether the disease be primary or secondary. We do not stand alone in this opinion. Dr. West, one of the highest authorities in this country, thus cautions his pupils :

"With reference to depletion you must not forget that the disease in which you are about to employ it, although of inflammatory nature, is inflammation in a scrofulous subject, and is in many cases grafted on previous organic disease..... You cannot therefore hope to cut short the affection by a large bleeding ; but your object must be to take blood enough to relieve the congested brain, and no more than is necessary for this purpose..... Though you have determined on the propriety of depletion, it will be seldom found even at the outset of the disease that the character of the pulse is such as to warrant venesection..... I will not say that this depletion is never to be repeated, but I believe that in far the greater number of cases you will do no good whatever by its repetition." (Lecture VI.)

Dr. Churchill in his *resumé* again urges on us that—

"In the first and second stages of meningitis, our remedies are : bloodletting in proportion to the age, strength, and constitution of the child and the intensity of the attack, but in greater proportion than in other diseases." (p. 139.)

All that we are told about Iodine and its combinations, is included in what follows :

"*Iodine* has been used and it is said with benefit. Dr. Evanson is favorable to its employment ; but its use has not become so general as to lead to a belief in its great efficacy." (p. 138.)

Of the *great efficacy* of *any* remedy, or rather form of treatment, in tubercular meningitis, we have *great doubts* ; but we think that a little more information concerning this agent, the iodide of potassium, the proto- and biniodide of mercury, frictions with the ung. iodin. c. &c., might have been given, considering how much they appear to have in late years been used.

Our author's observations on prophylactic treatment are more to be praised.

After discussing other diseases of the nervous centres, the section is terminated by the subject of *spinal apoplexy*, chiefly in order to supply an omission in the chapter on trismus nascentium. The omission supplied is a notice of the views of Barrier, Olivier, Billard, and Matuszinski, which teach that *trismus* is due to spinal apoplexy, or to the effusion of a semi-coagulated fluid in the spinal canal. Our author is of opinion that the testimony of Clarke, Labatt, and Collins, renders it necessary for us to have further information, ere we fully coincide in the above views. We may also add that, although trismus is undoubtedly a spinal affection, yet the researches of West offer some serious obstacles to associating the above frequently observed lesions with the symptomatic phenomena, in the light of essential or necessary cause. (West, Lecture XI.)

Section II is occupied with the consideration of the " Diseases of the Respiratory system." On those of intra-uterine life it is remarked :

"Considering that the respiratory apparatus is not used during fetal life, we might naturally expect that it would escape disease ; but it is not so. Billard and Cruveilhier observe that in the bronchial tubes we find concretions, polypous masses, and evidences of inflammation ; and the latter mentions a case of death immediately after birth, in which the bronchi were filled with a thick mucus apparently the result of chronic catarrh. . . .

"Examples of lobular pneumonia are recorded by Cruveilhier ; of sanguineous effusion by Mende, Wrisberg, Joerg ; of abscess in the lung by Koelpin, Mende, and Cruveilhier ; of pleurisy by Veron, Billard, Orfila, and Cruveilhier ; of tubercles by Husson, Chaussier, Cruveilhier, Lobstein, and Billard ; of scirrhus by Wrisberg ; of œdema of the lungs by Zierhold ; and of hydro-pneumonia." (p. 180.)

To have rendered the notice complete, certain "vices of conformation" of the thorax might have been added.

A good general view is given of various opinions, &c., relative to the pathology of *spasm of the glottis*.

"It has been suggested that fatal spasm of the glottis may be the cause of the sudden deaths, without any apparent cause, that are met with among children. Such cases are recorded by Maunsell and Evanson, Montgomery, and very many writers. The infant may be perfectly well, or, perhaps, only slightly indisposed, when in a moment it falls back dead, as happened to the infant of a friend of mine.

"I confess that I am inclined to believe that many of the deaths attributed to the nurse or mother overlaying the child, are in truth cases of sudden death from spasm of the glottis." (p. 187.)

We admit with Dr. Churchill that in this disease, when fully developed,

the "prognosis is grave, and in the severe cases very serious, because of the implication of the brain, the tendency to terminate in convulsions or in sudden death;" but we think that a general prognosis formed on the first part of the following statement would be far from true:

"One third of Dr. Cheyne's cases died; Dr. John Clarke says that the patient rarely recovers; Gervino and Gardien think it almost always fatal if remedies be not employed in the early stage, and this seems to be the general opinion; but on the other hand, if the complaint be recognised and the treatment early and prompt, the symptoms will in many cases yield to the remedies employed." (p. 195.)

Amongst the agents made use of in its treatment, our author mentions that in one case Sir Henry Marsh tried an infusion of tobacco-leaves (gr. v to 3vj) as an enema; and that Dr. Stewart speaks highly of a poultice sprinkled over with scotch snuff. In *croup*, too,

"Dr. Condie recommends a tobacco-poultice to the throat, composed of the moistened leaves of tobacco, mixed with the crumbs of stale-bread or ground flax-seed. The patient must be carefully watched, lest the depressing effects be excessive." (p. 255.)

We think that this great depressor—*Nicotiana*—should for the future be expunged from the pharmacopœia for children. In spasm of the glottis:

"It will be advisable in most cases to apply some form of counter-irritation; either some irritating liniment or a blister, or what I have found far better, a small seton in the arm. In one case under my care, the moment the seton discharged fairly, the spasms ceased; and whenever it was left out they returned." (p. 196.)

The chapter on *Pertussis* is one of the most complete chapters in the book, the subject appearing to have been a favorite with the author. We gladly make room for the following specimen of Dr. Churchill himself; the extract is about the longest which we could find devoid of proper names, or which is not a mere repetition of what has been said by other writers:

"The different views of the nature of the disease may be thus summed up:

"1. That it consists simply in inflammation of the mucous membrane lining the air-passages, the glottis, larynx, trachea, bronchial tubes, and air-cells.

"2. That this inflammation is of a specific character.

"3. That it is an affection either of the pneumogastric nerves, spinal nerves, medulla spinalis, the brain, or the nervous system generally; either of a nervous or inflammatory character, or a reflex irritation.

"4. That it is a compound affection: in the beginning an inflammation of the air-tubes, and subsequently a spasmodic or nervous affection.

"5. That it is a nervous affection, having its seat in the bronchial mucous membrane, and in the pneumogastric and other nerves.

"Now if we are to decide the question by the results of *post-mortem* investigations, we must necessarily conclude that none of these theories can be the true one, because the facts upon which they are based are by no means sufficiently general; some indeed are so rare that it is evident they are additions to the primitive disease, and others so very uncommon, that one must conclude that they have nothing at all to do with it.

"Again, if we analyse minutely the history of the disease, and compare many cases together, we must arrive at the conclusion that they are divisible into two great classes, the simple and the complicated, and these differ not merely in degree, but in kind; that the former present, upon the whole, a very uniform appearance, with similar stages, symptoms, and course, but that the latter possess additional symptoms of different kinds, by which their history is altogether modified; they

are, in short, whooping-cough, *plus* the peculiar complication of each. This is so evident, that the best modern authorities have based their description of the disease upon it.

"On this ground we must reject those *post-mortem* evidences of extensive bronchitis, pneumonia, arachnitis, congestion of the brain and spinal marrow, redness and swelling of the pneumogastric nerves, &c., as being foreign to cases of simple whooping-cough; and if we then proceed to the consideration of the question of the nature of the disease, we find very little assistance to be obtained from morbid anatomy, for in the majority of cases of death from other affections during whooping-cough, the air-passages exhibited little or no trace of disease.

"If we turn to the history of the disorder, we find that it generally commences by a catarrhal affection of the mucous membrane of the eyes, nose, and air-passages, amounting in some cases to actual bronchitis; but it may be doubted how far this must be considered essential to the disease, inasmuch as many cases occur in which it is altogether absent. And as this affection subsides, in its place we have a peculiar spasmodic cough, consisting of a series of forcible succussions during expiration, with an impossibility for a time of making a complete inspiration. This impediment to inspiration evidently arises from spasmodic action of the muscles of the larynx, trachea, and bronchial tubes, extending probably to the smallest, as it comes on quite suddenly and subsides as suddenly. And although the cough is excited by the presence of mucus, and has for its object its removal, yet its character is peculiarly spasmodic, and unlike any ordinary cough.

"Now without attributing it to organic disease of the brain or spinal marrow, we cannot but refer the peculiarity of this cough and whoop to a state of the nervous system analogous (shall I say) to that which gives rise to spasm of the glottis. In other words, that whooping-cough is also a case of reflex irritation of the nervous system, excited, no doubt, by other and different causes, but exhibiting a similar transference of effects.

"We are at present, I believe, quite ignorant of the nature of the peculiar exciting cause; we know that it exists, and that when it is applied, the primary irritation of the mucous membrane arises, followed by the reflected nervous irritation which gives rise to the peculiar phenomena of the disease." (p. 215.)

We add the results of the author's experience of æther and chloroform in its treatment:

"Soon after the discovery of the anæsthetic effects of *sulphuric æther*, it struck me that it would be likely to modify or suspend the spasm in whooping-cough; and having a case under my care, I directed that a little (I suppose about half a drachm) should be spilled upon the nurse's hands, and held before the child's nose and mouth at the commencement of a fit of coughing. I preferred this simple mode of administration (and do so still), because of the impossibility of thereby giving an overdose. The effect surpassed my expectation; most generally the paroxysm was shortened more than one half—often stopped immediately, and the duration of the disease unquestionably considerably diminished. Since then, I have tried the æther in twelve or fourteen cases, and *chloroform* in six. In one or two cases no benefit accrued, in others great mitigation of the spasm, and in three or four almost complete relief when the æther was applied at the beginning of a fit of coughing. Decidedly also in two thirds of the cases the course of the disease was much shortened, so that I look upon this as a valuable addition to our remedies. In no instance was insensibility or the least inconvenience occasioned." (p. 223.)

Where convulsions complicate the affection:

"All our treatment will be in vain, unless we can contrive to lessen the frequency and violence of the cough; the reiterated arrest of the circulation will shortly reproduce the convulsion. For this purpose I have found the hydrocyanic acid of great value; if anything will check the cough it will be either that or the belladonna. I should think it probable that the same effects would follow the chloroform or

ether, but as yet I have had no opportunity of trying either. I am not prepared to say whether the convulsion ought to prohibit their use, but I rather think not." (p. 227.)

The subject of *croup* is ably treated; the article upon it presenting a good epitome of the more important points in its pathology and treatment. As remedial measures for its early stage, the author places his chief reliance on bleeding and antimony. The question as to the utility of the performance of tracheotomy in its more advanced period, is minutely gone into:

"From the slight sketch I have given, the reader will perceive that the weight of authority, especially in Great Britain and America, is against the operation in croup; and also that the results of the cases in which it has been performed exhibit no very encouraging success. As an argument this is not worth much, however, to those who regard the operation as a '*dernier ressort*,' to be adopted in no case where there is hope from the ordinary method of treatment." (p. 259.)

If the operation be only resorted to when all hope from the use of our ordinary measures is entirely gone, then we believe it will be of no avail. But let the position be considered in another light,—are we or are we not justified in performing the operation earlier in the disease than it is, at least in Britain and America, usually had recourse to, considering the great chances there are that the disease will progress from bad to worse, when not arrested almost at the outset? To perform it when the disease has passed on to its extreme stage, when the powers of the patient are completely prostrated, and death absolutely impending, is indeed striving after a forlorn hope. But we think not so in the earlier period, after giving a fair chance to bleeding and antimony. But who would like so early to urge it on the parents' attention? who would willingly take the responsibility? for if the child should die soon after its performance, the operator would be regarded in no favorable aspect. We think the principle taught by Dr. Watson as regards acute laryngitis very applicable here; and if we were a child, and had croup, and could give advice as to our treatment, we feel that we should speak in the following spirit:—

"In my own case I should choose to be operated on early, the moment I found that early bloodletting was not telling upon the local distress, and that any shade of duskiness became perceptible in the skin, just as I should choose to be operated on at once for strangulated hernia, after one fair attempt had been made by a skilful hand to return the bowel; without waiting till inflammation had set in, or had been caused by the taxis." (Watson's Lectures, vol. i, p. 819.)

Out of 150 cases noted by M. Trousseau, 39 recovered, and 111 died after the operation:

"He mentions that there are also living in Paris about fifteen children saved in croup by tracheotomy, performed by Gerdy, Robert, Guersent, jun., Boniface, Déprés, Blandin, &c.; but he is unable to communicate particulars." (p. 259.)

The subjects of *bronchitis* and *pneumonia* follow croup. The attention of our readers has been before directed (see our sixth Number, p. 424) to the unsettled state of opinion which exists in regard to some points of the pathology of infantile pneumonia; and the views lately promulgated by MM. Bailly and Legendre have been brought before their notice. In order to render our further observations the more pertinent, it will be necessary to recapitulate here, in a few words, the general tenor of their doctrine. These writers maintain that the anatomic lesions of the so-

called lobular pneumonia of children are due to a cause quite different from inflammation of the cells of the pulmonary parenchyma; they affirm that the "hepatization," "consolidation," and "carnification" met with in this disease are states analogous to the condition of the foetal lung which has never admitted air (atelectasis of some authors); and they consider that the lung, although it has admitted air, yet from becoming forcibly collapsed through the congested or distended state of the pulmonary capillaries, assumes a condition like that above mentioned. Lobular pneumonia is accordingly to be viewed as pulmonary catarrh, or bronchitis, conjoined with an *état foetal simple* or an *état foetal congestif*. The work of Dr. Fuchs at the head of our article endeavours to carry out, to an extreme extent, the more essential points of MM. Bailly and Legendre's theory; at the same time enforcing other doctrines peculiar to the author himself. He is already known to the profession by a 'Treatise on Emphysema;' and the present monograph is one which, like the other, will amply repay perusal. We have derived much pleasure and instruction from it, and although it bears the title simply of "Bronchitis," yet its field of inquiry will be found more extensive than from this might be supposed. We cannot say that we entirely coincide in the picture drawn by the author of the symptomatic phenomena of what he considers alone as bronchitis, as representing only one disease, that of the lower air-tubes with a *non-inflammatory* condition of the pulmonary parenchyma; and we are still less willing to surrender our judgment to his opinion, relative to the causes of the morbid changes found after death in the latter, until more extensive investigations have been made,—the more especially so, considering how this opinion runs counter to that of so many competent observers. We shall lay the main points of Dr. Fuchs' theory before our readers, leaving out of view all collateral subjects:

"The differential diagnosis between pneumonia and bronchitis is sought for with great anxiety by most authors, and laid down accordingly. This I regard as superfluous; for in the numerous *post-mortem* examinations which I have made of children, I have never found a condition of the pulmonary parenchyma which could be regarded as analogous in the remotest manner, with that observed in adults, who have died from pneumonia. Abundant signs of distinctive character exist also, which lead to the opinion that true pneumonia does not appear to attack children." (p. 114.)

"The result of investigations hitherto made, clearly proves that the changes in the pulmonary parenchyma are not due to inflammation of it, but arise from the cells becoming devoid of air, and as a consequence incapable of performing their function. Atrophy of them ensues." (p. 81.)

From the above, and the details given in the work, it is clear that the "pneumonia" of Valleix and of others, Rokitsky's "catarrhal pneumonia," "atelectasia" of many writers (not of Jörg), and the "foetal conditions" of MM. Bailly and Legendre, are considered by Dr. Fuchs as synonymous with the contingent pulmonary lesion of his infantile *bronchitis*. Dr. Fuchs stands in the most complete opposition with Friedleben ('Archiv für Physiolog. Heilkunde,' 2 Hft. 1847), who asserts that true lobar pneumonia is one of the most frequent of the acute diseases of early life, that it exhibits the same anatomical changes as the genuine pneumonia of grown people, and that the opinion that a special (so-called) pulmonary catarrh is the ruling form in children, is one not



founded in truth. It is further maintained by Friedleben, that the pneumonia of children runs through the same course as that of adults, but that it evinces in the former a greater tendency to suppuration than it does in more advanced years. Opposed as these pathologists are to each other, they are equally opposed to the views generally prevalent on the subject in question; although in this country some of the better authorities believe that true lobar pneumonia is more frequent in children than might be supposed from the investigations carried on in the large hospitals on the Continent (West). The *bronchitis* of Dr. Fuchs "is one of the more frequent and dangerous of the diseases affecting children, and therefore one of the most important to be considered." (p. 8.)

"I have described it as consisting of two stages; the first is associated with the changes taking place in the ramifications of the bronchia, the second with those ensuing in the pulmonary parenchyma; and since these changes differ in their fundamental nature, it is easily explainable why their symptoms appear to stand in opposition, and to possess so different a character from each other." (p. 83.)

"A question to be considered is—in what consists the changes in the air-tubes?—is simply catarrh present, or a state of inflammation? On this point much difference of opinion exists, since it is presumed that such severe symptoms can scarcely be dependent only on catarrh. According to my view of the matter, the discussion is quite unnecessary and of no importance, since the danger originates elsewhere." (p. 55.)

"The first stage may be called the catarrhal or inflammatory one, since in very young children it may come on, as we shall afterwards see, as a bronchial catarrh, whilst in older ones it will have more the character of a determinate inflammation. The second stage is based on the changes ensuing in the pulmonary parenchyma, and begins when the air no longer penetrates into the cells through the minute bronchial ramifications." (p. 10.)

The danger is dependent on the second stage. The chief morbid states of the lung met with in this stage are the following, placed in order of their advancement:—1st. "Glandular- or nodular-like induration." 2d. "Red induration." 3d. "Blue-gray induration." The fundamental cause of these changes we have already given in our quotation from page 81. It must be borne in mind, that Dr. Fuchs's opinion of their *cause* exhibits to the reader one of the most important and leading principles of his treatise:

"Under the term *atelectasia*, that condition of the lung is implied which exists before birth and before commencing respiration, and after birth and after respiration. In the former case it is normal; but in the latter, where air should and must penetrate the lung, it becomes an abnormal, even a diseased condition. The cause of this latter condition does not lie in the pulmonary parenchyma itself. The term *atelectasia*, as applied to it, is therefore not properly applicable; that of "*fœtal condition*" is better, as it sufficiently answers to the whole events. Still as the former term appears to have become naturalised, and is applicable to the second stage, we may retain it. That which Legendre has lately denominated "*fœtal condition*" of the lung is not *atelectasia*, and does not belong here; the denomination is false and leads to error, since the state he describes is not a fœtal state, but the state of a lung which has become empty of air." (p. 93.)

"I designate with the term *apneumatois* that condition which ensues when a lung previously containing air becomes devoid of it. A definite condition demands a definite name; I have therefore chosen the above one. A lung in which the respiratory process has commenced, and the minor circulation begun, into which air has penetrated, in which the vessels of the minor circulation have become enlarged and contain more blood, does not retrograde into the fœtal condition, nor does it again assume the quality of the fœtal lung. I have therefore not retained the term

bestowed upon it by Legendre. Legendre has rightly comprehended the subject, but his term leads to confusion, since it is synonymous with atelectasia." (p. 104.)

"To search out the distinctions between atelectasia, apneumotosis, and bronchitis, is a subject attended with great difficulty. Touching the *causes* of the former two, it may be said, they are congenital in atelectasia, superinduced in apneumotosis, whilst bronchitis consists in a diseased condition of the lining of the bronchia." "In respect to anatomical characters it may be remarked, that the atelectic lung never contains air, sinks in water, is of small compass, of even surface on section, from which venous blood exudes. It possesses a certain cellularity and softness, and easily admits of inflation. In its second stage it retains its small compass, becomes firmer, brown-red in colour, and does not admit of inflation. The apneumatic lung is, in its first stage, of a dark colour, contains air, swims and crepitates; its compass is less than that of a collapsed lung, and hence single lobules in the apneumatic condition appear as if sunk in the normal tissue. Inflation can be performed. In the second stage the tissue becomes firm, compact, and void of air; it assumes an uniform character, a small compass, has an even surface on division, and no longer permits of inflation. In the third it appears as a blue-gray coloured tissue, with white streaks.

"The bronchitic lung contains air, even in large bubbles, is at first bright-red, then dark-red, is increased in size, and the diseased lobules are much larger than normal, the surface is unequal; the enlarged intercellular tissue forms furrows into which the pleura sinks; the parenchyma is very firm and hard, crepitates strongly, no blood exudes from its division or that of its vessels, inflation is difficult, and air penetrates into the intercellular tissue. In the second stage it becomes compact, firm, and brown-red in colour, is larger than a healthy collapsed lung, the separate lobules form firm hard nodules not to be inflated. Mucus is collected in the bronchia and cells. In the third stage, blue-gray induration is seen in connexion with emphysema anteriorly. The differential conditions of the first stages are the most determinate, those of the second depend chiefly on *size* as distinctive, those of the third are nearly similar. Bronchitis is most surely determined when a portion of the lung is seen in the first stage, and the changes in the mucous lining are plainly apparent." (p. 113.)

The chapter on pneumonia in Dr. Churchill's work is merely a condensation of Rilliet and Barthez, Barrier and West. We pass on to the third section, which treats of "Diseases of the Heart." When alluding, in the article on the 'Diseases of Children' in our sixth Number, to affections of the central organ of the circulation, we commented on the facility with which inflammation of the exocardial and endocardial membranes in children might be overlooked; the symptoms to which such inflammations give rise being considered as indicative of severe and acute disease of the pulmonary organs. To such an error in diagnosis we felt that those are particularly liable, who are not daily conversant with the diseases of children, or who have not acquired that *clinical eye*—as we have termed it—which is so essential to the practitioner who has much to do with these affections. The truth of the above fact was forced upon us by our own experience; we gained it not from books; we revoked not our own erroneous judgment in diagnosis from what others taught us, but we became aware of it from the errors we at first made, and kept ourselves on the alert to avoid them for the future. In this liability to error which we believe to exist, we do not mean to refer to acute cardiac affections supervening in the course of rheumatism, or when the heart has been previously diseased. In these cases, we are led both by circumstantial and substantive reasons to look at once to the great organ of the circulatory apparatus. We have in our mind's eye such cases as the following:—A child is

brought to us and described simply as being "so stuffed at his chest," or as having "so much difficulty in breathing," or merely as being "very ill," the mother not locating the malady in any particular spot, nor even in some instances thinking there is much the matter with the child at all. But it not unfrequently happens that a glance at that child will tell us that the heart is affected, or, at any rate, that we have discovered by that glance sufficient reason to direct our attention to it; and having made the necessary examination, we discover we have proceeded to the right locality. But what is it that this glance has taught us? What has passed to us through the inlet of the eye? We wish for the sake of those who may be placed as we have been, that we could sufficiently describe it. We fear that by words it is unteachable; or if not so, that we have not the power to make it plain—"esse aliquid in medico, quod neque dici neque scribi posse." The diagnosis lies between bronchi-pneumonia and cardiac inflammation. The signs of the former we know to be the peculiar respiration, action of the *alæ nasi*, the marked facial expression of distress, of great general *malaise*; and in severe and more advanced cases, the puffiness of the countenance, and change of coloration in the lips and cheeks. By the combination of several of these characters, we were at first always led to the pulmonary affection; but experience gradually taught us, that though very often right, we were sometimes wrong; that the lungs, &c., were not affected, but that the heart was. The broad outline of the visible symptomatic phenomena, seized by the eye at first, was identical in both; but a deeper analysis of that embraced in it proved it might involve a double meaning. The fact of this analysis not being made by many observers, and their conclusion being that the pulmonary organs are the parts affected, and treatment adopted accordingly which fortunately in many cases applies equally to both the affections we are alluding to, are, we believe, the causes of the prevalent opinion that *idiopathic* inflammation of the exocardial and endocardial membranes is not common in childhood. We believe it to be more so than is usually supposed, and it is to this form of cardiac disorder that the error in diagnosis we are dwelling on so especially applies. Dr. Churchill states of *Pericarditis*:

"The disease may be either *primary* or *secondary*, the former being exceedingly rare. It is seldom met with in adults according to Dr. Latham, and still more rarely in children." (p. 338.)

"Endocarditis, or inflammation of the lining membrane of the heart, seems more common than pericarditis both in adults and children, though they are frequently combined. . . . The attack may be either *acute* or *chronic*, the latter being fully as frequent as the former; and either *primary* or *secondary*, the latter being, as in the case of pericarditis, much more common than the former, and more frequent than primary pericarditis according to Dr. West." (p. 350.)

Our experience certainly will not permit us to gainsay, that simple idiopathic endocarditis is more frequent than the exocardial inflammation; but we believe, as we have before said, that a combination of the two is more frequently presented to us, as a disease affecting children from two years of age to eight or ten, than is generally admitted as likely to occur. It assumes a character not at first so urgent as that of the secondary forms of the malady, and is very amenable to treatment; and from that treatment being generally put in force (though the true nature of the disease is constantly mistaken, and therapeutic measures directed to

another and assumed malady), and being applicable to it, less attention has been directed to the actual existence of the cardiac affection to which we have thus drawn the notice of our readers. But to return to its diagnosis; Dr. Churchill, speaking of pericarditis, remarks:

"Probably the earliest symptom we shall notice of the disease in question will be uneasiness or pain in the left side of the chest, in the præcordial region, near the left mamma; this pain will be expressed if the child be old enough, or if not, we may detect it by the position in bed, the restrained inspiration, the suffering on percussion, or on being moved. In Constant's, Mayne's, and Billard's cases, it was pretty severe; in Puchelt's, not very acute; and in Rilliet and Barther's cases, it occurred but rarely, and was not severe." (p. 339.)

"The respiration, too, has a peculiar character in general; it is not the dyspnoea of obstructed lungs, nor is it any form of cerebral respiration; but it is high, rapid, yet restrained and suffocating, with quick movement of the *alæ nasi*, and a difficulty of speaking sentences, as though the interruption to the short, quick inspirations necessary in speaking, were intolerable. . . . If there be no disease of the lungs, there will be but little cough, if any. . . . Palpitation, owing to irregular action of the heart, is seldom troublesome; but the violent action of the organ is sometimes felt in a distressing manner.

"The pulse is very quick, strong, and wiry. The face has an anxious, drawn, distressed, almost frightened expression; in two cases, Billard observed spasmodic movements of the limbs; the child cries often as if suffering extremely, and generally objects to lying flat down in bed.

"But all these signs would only excite our suspicions that some grave lesion existed: they afford us no precise information as to its nature. This we can only obtain by a careful estimate of the physical signs; but then it is satisfactory to know that these are amply sufficient." (p. 340.)

"The phenomena which indicate the commencement of endocarditis are very slight and obscure; a slight febrile movement which subsides in a little time, the respiration somewhat accelerated and possessing the peculiar character I noticed in pericarditis; obscure pain in the præcordial region, and some difficulty in lying upon the left side, may be all the symptoms developed." (p. 350.)

The picture thus drawn by Dr. Churchill is in the main correct; and though referable in some points to more urgent and advanced cases than we ourselves are discussing, and meant to apply, we assume, to *secondary* forms of the malady, yet it indicates the source and nature of the signs by which we are governed in our diagnosis between bronchi-pneumonia and idiopathic cardiac disorder, so far as the primary diagnosis is gleaned by the first sight of the patient. But, without asserting that in these cases *precise*—meaning by this term, sure and unmistakable—information can be absolutely obtained without applying the ear, yet from what we have said, our readers will readily expect that we go much further than the belief, that without the ear all we could arrive at would be "that some grave lesion existed," and that we could derive "no precise information as to its nature." If this were all, we should feel that we had made but slight progress in clinical observation, or in the culture of that upon which we have already placed such stress. On the contrary, we feel assured that in the character of the peculiar respiration well adverted to by our author, its perceivable difference from that with which we have become familiar as indicative of bronchi-pneumonia, its more rapid performance, the quicker but less dilated action of the *alæ nasi*, the face not so congested and more pallid, the want of that intense expression of *malaise*, except when the case is more advanced and the symptoms urgent, the absence of

that great *abattement* of the intellect and feelings which we find in the pulmonary disorder, and the great and rapid increase of the positive symptoms on the slightest movement, are ample reasons for the immediate direction of special inquiry towards the heart. That inquiry has usually established the rectitude of our diagnosis.

At the commencement of the fourth section, which embraces the "Diseases of the Digestive System," we have an example of what we regard as an imperfect selection on the part of the author of those diseases he has considered himself bound to touch upon. *Hare-lip, cleft palate and imperforate anus* are discussed, and accounts given of the various surgical operations put in force by Cooper, Liston, Fergusson, Hutchinson, and others, for their relief. Now, in a work by a physician professedly on those diseases of children which come within the sphere of his own special line of practice, and in which work, professedly systematic also, such diseases as scrofula, phthisis, &c., are altogether omitted, we perceive great inconsistency in the above affections being dilated on, since these, in this country at least, fall to the province of the surgical practitioner. Why has not Dr. Churchill treated of congenital hernia, cataract, nævi, &c., &c.? This at least would have tended to make the inconsistency somewhat more consistent. We have no doubt but that he would have discovered help in the same place to which he confesses he has resorted to for much of the surgical information on hare-lip, &c.

"For most of the information I have been indebted to Mr. Cooper's invaluable Dictionary." (p. 359.)

After having read through the author's observations on *follicular and ulcerous stomatitis*, we were surprised to have found no mention made of the value of the chlorate of potassa in their treatment. This we regard as a serious omission in therapeutics; since we view the introduction of this agent into practice as one of great importance. Although the chapters on *muguet, aphthæ*, &c., are entirely composed from home and foreign writers, yet we find no allusion to the investigations of Dr. Berg of Stockholm ('Brit. and For. Med. Review,' vol. xxiv). We assume Dr. Churchill to be almost entirely ignorant from observation of *gangrene of the mouth*, or he would scarcely have allowed even his method of copying and extracting *verbatim* from other writers, to have led him into the mistakes which his account of this affection testifies he has fallen into. We even doubt his experience of the worse forms of ulcerous stomatitis. The first description he gives of the former malady is copied from Mr. S. Cooper; in which it is stated to be "a perfect specimen of phagedenic ulceration, and in its worst forms not unlike hospital gangrene." (p. 402.) Then follows that of a "variety," described by Dr. Cumming, which, in our judgment, is not gangrene of the mouth, but a bad form of ulcerous stomatitis; and at length, after more than three pages of extract from Rilliet and Barthez, the author proceeds to Underwood for an account of the "causes," the following extract from him being given in illustration by Dr. Churchill.

"The worst species of the complaint that I have happened to see," observes Dr. Underwood, "has been during the second period of dentition, when the child has been shedding a number of teeth together, leaving the rotten stumps behind,

which have been neglected to be drawn out. The whole gums will then sometimes be spongy, or dissolve into foul spreading ulcers." (p. 410.)

On referring to Dr. Underwood for further enlightenment, we read three lines further as follows :

"If the stumps of the decayed teeth can be got at they ought to be extracted; after which some such applications as the following will soon brace the loose gums, and heal up the ulcers, however foul or numerous they may be." (Underwood, Tenth Edition, p. 505.)

The applications alluded to are lime-water, myrrh, cinchona, &c.

Does Dr. Churchill really imagine that this is illustrative of gangrene of the mouth? Why Underwood himself explicitly states that he is talking of the "common canker," which "is rarely troublesome to cure." Notwithstanding that the author gives the differential diagnosis of the gangrenous disorder from ulcerous stomatitis (p. 412), and—so far as it goes—correctly too, yet it is evident that in his description of the former disease, of which he has "preferred the old appellation of *cancrum oris* as involving no hypothesis" (p. 402), he has confounded the two disorders together, extracting from other writers descriptions of bad forms of ulcerous stomatitis as illustrative of that which he makes synonymous with *stomatite gangreneuse*, *cancer aqueux des enfans*, &c., &c. If Dr. Churchill had had an hypothesis, and had preferred the use of a definite scientific term to that of an old woman, we should have had somewhat less confusion, and Dr. Churchill himself would have been saved from seeking refuge under his premonitory admonition to the reader,—“There appear to be several phases of the disease, differing in degree, if not more essentially,” (p. 402),—indicative of the entanglement into which the author himself seems to have had some foreshadowings that he was getting.

Of the treatment of *tonsillitis* it is observed :

"When the tonsils remain permanently enlarged, we must make some efforts to reduce them, not only on account of the liability to repeated attacks of inflammation, but because they involve a disagreeable change of voice and discomfort in swallowing. Dr. Coudie advises the repeated application of nitrate of silver; others, repeated small blisters externally; others, their removal by operation. Professor Hess, of Copenhagen, states that he has employed compression, by means of the index-finger applied to the indurated tonsil, with success. This is to be repeated three or four times a day; and when the gland becomes softer and absorption commences, gargles may be used. Each of these plans may succeed, and we may try any or all of them; but I would also suggest that the internal application of the caustic tincture of iodine, as well as the external use of the ointment, should have a fair trial previous to any operation. I have seen it very successful in several cases." (p. 420.)

We have tried all these measures except Hess's, and others too, in chronic enlargement of the tonsils; and have arrived at the conclusion that in nine cases out of ten they are of no avail in affording any considerable temporary, much less permanent, relief. In cases of shorter duration, the above measures, combined with proper constitutional treatment, afford relief; but in those of longer standing, we generally advise excision so soon as difficulty of breathing, trouble in swallowing, or "speaking through the nose," are shown by the patient. We do not go the length of those who recommend the use of the knife in almost all cases not acute; but on the other hand, we cannot coincide with that experience which in cases of old standing finds benefit to be derived from the temporising measures usually put in force, or sees danger and detriment

to the patient in undergoing the removal of the enlarged and indurated glands. There is one more therapeutic agent, however, to which we intend giving a trial. This is *gallic acid*, which, taken up by a moistened camel's hair pencil, may be smeared over the affected parts twice or thrice a day. We are led to make trial of it, from the benefit which a patient suffering from a varicose state of the veins of the side of the tongue received from its use. We had tried everything we could think of, locally and constitutionally, without advantage; when we recommended the patient to smear this acid along the tongue, and relief was almost immediately experienced.

After the discussion of the subjects of *parotitis*, *pharyngitis*, &c., we have a short chapter on *abscess between the pharynx and the spine*, founded on Dr. Fleming's paper, which appeared in the 'Dublin Medical Journal.' As it is a subject of much interest, and it is possible that the paper of Dr. Fleming may not have come before the notice of some of our readers whom it might greatly interest, we shall give the following extract from Dr. Churchill:

"The symptoms are very characteristic, although at first one might attribute them to some affection of the nervous system. They may be divided into the premonitory and the essential. The *premonitory*, indicative of *local* uneasiness, but yet common to all affections of the throat, complained of or otherwise according to the age of the child, and on examination not accompanied with proportionate visible lesion. The *essential*, often very suddenly supervening, and indicated by derangement of the cerebral, circulating, and respiratory symptoms, alternating with the comparatively healthy condition of these systems according to the alteration in the position of the individual; fixed and retracted state of the head, with rigidity of the muscles at the back of the neck, and more or less locked state of the jaws; painful deglutition, impossibility of swallowing, solids and fluids convulsively darted forward through the mouth and nose; repeated acts of deglutition without the presence of any fluid in the mouth, and on examination of the fauces, a firm projecting tumour felt beyond the base of the tongue, and if seen, presenting a smooth, rounded, highly vascular appearance behind the soft palate, usually occupying the median line, but occasionally inclining to either side. These essential symptoms are accompanied with the ordinary characteristics of suppurative fever. . . . Fever more or less sthenic in its character, according to the peculiarity of constitution of the child, is always present, and I think precedes the development of the local symptoms." . . . (p. 440.)

"Dr. Fleming's experience has proved that surgical interference is as effectual as it appears to be essential from the symptoms; and 'not alone from the fact of certain fatal results from mechanical pressure on and interference with vital organs, but also from the situation of the abscess being particularly favorable to extensive diffusion.' In one case only has Dr. Fleming seen a spontaneous opening occur; the abscess was situated high up, and the matter passed through the nose. . . .

"For increasing the facility of the operation, Dr. Fleming has contrived an instrument consisting of a trochar about four inches long, one extremity of the canula being slightly curved, the other with a ring on its upper surface to receive the forefinger; into this canula was passed a jointed stiletto, with, at its opposite extremity, a ring for the thumb, and a moveable screw, to graduate the projection of its point (p. 441).

"The greatest caution must be observed; an assistant must hold the head firmly, and be ready to throw it forward when the puncture is made. The operator should pass his left forefinger to the back of the pharynx, and fixing the point of it upon the tumour, use it as a guide to the trochar, so as to place it on the most prominent part of the tumour, when pressure on the stiletto will effect the object in a moment."

“Dr. O’Ferrall recommends that the operator should stand behind the patient, and pass the ‘forefinger of his left hand between the palate and the tongue, carefully avoiding the latter until it reach the abscess; the trochar is thus readily guided to the point intended to be punctured, and thus all danger to the surrounding parts is avoided.’ Dr. O’Ferrall, in similar cases, would in future prefer a straight bistoury having the cutting part short, as the density of the carving [covering?] of such abscesses renders the plunge of a trochar unsafe.” (p. 442.)

The history of the various forms of intestinal affection, included between *diarrhoea*, p. 466, and *dysentery*, p. 495, might, we think, have been simplified:

“There is no complaint so common in infancy and childhood as disordered bowels; and this we can easily understand on account of the delicacy of the mucous membrane, and the novelty, so to speak, of the functions it is called upon to fulfil.” (p. 466.)

We cannot perceive any greater “novelty” in these functions, in relation to the structures naturally intended to perform them, than we can in those which the eye, ear, brain, or lungs are severally called upon after birth to fulfil.

Dr. Churchill is, we presume, unacquainted practically with the value of administering internally the *nitrate of silver* in *entero-colitis*, as he merely alludes to the employment of it as an enema by M. Trousseau. If our author will make trial of it in the former method, as well as *per anum*, we believe he will find it to be one of the most valuable agents in arresting the exhausting intestinal discharge. In those instances only where it fails, we continue its employment in the form of injection, and administer the sulphate of copper, combined with small doses of the tincture of opium, in lieu of the mixture of the nitrate of silver.

The following remarks are worthy of being kept in mind:

“One of the occasional symptoms I have already noticed, the prolapsus ani, demands a word as to its treatment. So far as it depends upon relaxation resulting from frequent discharges, the relief of the diarrhoea will cure it at the same time; but it does often remain, because of the habit of sitting long at stool and forcing, which the child acquires during the course of the disease. Now, in order to remedy this effectually, all we have to do is to place a board with a small perforation across the chair or vessel the child uses, and to place the latter so that the child cannot touch the ground with its feet. So circumstanced, no excessive forcing can be used, and I have repeatedly found the plan successful. I am indebted for the suggestion to my friend Dr. Corrigan of this city.” (p. 484.)

“Diseases of the skin” follow those of the digestive system. The account given of them is but slight; and except where constituted of extracts from Burgess, Neligan, &c., by no means *au courant* with the dermatology of the present day. The terminology and arrangement of Bateman are chiefly followed.

In the next or sixth section, on the “Eruptive Fevers,” succinct and good accounts are given of *measles* and *scarlatina*, with their complications. We would not desire to be thought hypercritical; but we rather object to the terms “florid” and “vivid” red (pp. 552, 554), as applied to the ordinary colour of the eruption of measles. It is true that the colour varies in different cases, as Dr. Churchill observes (p. 554); but we do not think that we ever saw it approach in brightness and clearness of hue to that degree which we assume is implied in “florid” and “vivid.” We should rather call it a dingy, dusky, or dirty red, often of unequal



tone in parts, so as not unfrequently to cause a mottled appearance. We were curious as to the terms which would be employed by the author to designate the colour of the eruption in scarlatina, considering those applied to measles. The surface in the former is stated to be of a "vivid scarlet colour, like that of a boiled lobster." (p. 572.)

"The eruption may vary its seat; it may commence and continue most marked, as in a case I lately saw, upon the hands, shoulders, or back, or upon the cicatrix of a blister. In the morbillous fever of Sydenham (1674), it was principally on the neck and shoulders; in an epidemic at the College de Vendome in 1826, M. Gendrin observed it was confined to the face. In a case mentioned in 'Rust's Journal,' the eruption occupied one half of the body only." (p. 554.)

Dr. Churchill thinks it extremely doubtful whether cases of Willan's *rubeola sine catarrho* are those of measles; they rather appearing "to belong to the order *roseola*" (p. 555). It is well known that the late Dr. Home of Edinburgh succeeded in producing measles by inoculation. Several others, up to the time of Speranza in 1822, have done so likewise. Our author notices some later experiments by Katona of Hungary, who, during a severe epidemic in the winter of 1841, inoculated no fewer than 1122 persons "with a drop of fluid from a vesicle, or with a drop of the tears of a patient affected with the disease; the fluid being inserted in the same manner as the virus in vaccination" (p. 564). The operation is stated to have been successful in the proportion of about 93 per cent.; the attack of the disease which followed being a mild one, and no fatal case occurring. In Home's cases, the fever commenced on the sixth day; in Katona's on the seventh. Dr. Churchill does not venture an opinion of his own as to how soon in the course of the affection zymotic particles are given off, capable of propagating the malady in another. He states, however, that Dr. Williams considers that the blood throws off infectious emanations during the fever which occurs prior to any eruption. Upon this point we would observe, that a case is related in the 'Gazette des Hôpitaux' for May 1849, which appears fully to confirm the truth of Dr. Williams's opinion. A little girl became infected from her sister (previously and afterwards separated from her), at least twenty-four hours before any eruption showed itself on the latter. The period of incubation in the former was eight days.

Dr. Churchill thinks there is good evidence for believing, that a child may have *scarlatina* without any rash upon the skin; and is disposed to agree with Dr. Copland, that "true scarlatina, though *latent*," may be unequivocally present without either sore throat or eruption. The first hint given as to the existence of this *latent scarlatina* is to be found, we believe, in Dr. Graves's 'Clinical Medicine,' Ed. 1843, p. 536. Dr. Copland has more lately and fully worked out the subject in the second volume of his Dictionary, p. 672. The presence of the scarlatinal poison in the body is assumed from the morbid effects of it, exerted primarily and chiefly on the kidneys and serous membranes, in persons who have been exposed to infection during an epidemic, and who yet do not evince the faucial and cutaneous phenomena of the perfectly-developed form of the disorder. We cannot see our way clearly through the following statement of Dr. Churchill:

"*Causes*.—Among the predisposing causes we may mention *age*, children being more frequently subjects of the disease than adults, and adults than old people.

I have already stated that I do not believe that children are more liable, but that they are exposed to the infection whilst children, and therefore take the disease before they grow up." (p. 593.)

If children are not more liable to have scarlatina, how can their age be a predisposing cause to contract it? If a certain age be a greater predisponent than is another, surely a person of the former age must be more liable than one of the latter to yield to the influence of zymosis. Admitting that a particular *age* is not a predisponent, but that persons of all ages are equally liable, yet it is easily seen, that children may be more frequently the subjects of the disease than adults are. But we do not believe that this opinion holds good; on the contrary, we are confident, that if fifty children of five years of age, and the same number of adults of five-and-thirty, who had never had scarlatina, were alike exposed to its infection, a greater proportion of the children than of the adults would succumb to the influence of the poison. We presume that Dr. Churchill does not think so. The successful trials of Sir B. Harwood on the inoculation of this affection are alluded to, as are also those of M. Miquel de l'Amboise. The author does not give us any information concerning an exanthematous disorder, which seems to be the osculant species between *morbilli* and *scarlatina*; and in the existence of which we have no doubt. This is the *rubeola* and *rötheln* of some writers. At the end of the last century, Selle, in his '*Medicina Clinica*,' showed that under the denomination of measles, two distinct affections were included; one being true measles, or *morbilli*, and another *rubeola*. Since his time, many of the German writers have described this latter affection, even epidemics of it. Some have regarded it as a variety of scarlatina; some as one of measles; others have viewed it as a distinct disease, a special and specific contagious malady; whilst Hildenbrand and Copland look upon rubeola not as a disease *sui generis*, nor as a mere modification of measles, or of scarlatina, but as a sort of "hybrid of these two fevers, presenting sometimes a predominance of the symptoms characteristic of the one, at other times of those distinguishing the other, and not unfrequently an equal combination of the features of both." (Copland.)

The French pathologists, if we may take Rilliet as their exponent, consider that *roseola* and some forms of *abnormal scarlatina*, in which the eruption is benign and pale, have been included under, or made to constitute, *rötheln* and *rubeola*. We coincide in the views of Hildenbrand and Copland in the determinate existence of an affection neither measles or scarlatina; and we think there is great probability of the truth of their opinion, that it is a hybrid or bastard species generated by the two. Of one thing we are certain, viz., that although we have never witnessed what might be termed an epidemic of *rubeola*, yet we have frequently met with cases which we could neither resolve into measles or scarlatina, but which answered in all points\* to the descriptions given by the Germans and Dr. Copland, of their so-called *rötheln* and *rubeola*. We speak not from a limited experience, but from one which a public institution can afford. The fullest information in our language upon the subject is, we believe, given by Dr. Copland in his erudite and comprehensive Dictionary.

The seventh, and concluding part, containing *ten* pages, embraces the consideration of "Infantile Remittent Fever, Worm Fever, Gastric Fever." No pretence is made of developing the important subject of *fever* generally

in the child ; this, in its comprehensive sense, is excluded from the book. The exposition of *remittent*, chiefly composed from Condie, Butter, Joy, and Pemberton, we look upon as below the standard of the present day ; nor do we think that the observation of Dr. Churchill, that "in practice, various modifications, both of the symptoms and course of the disease, are observed, all of which cannot be easily enumerated," (p. 641,) should shield him from our animadversions, seeing that he has given up more than *ten* pages to "Porrigo, or Scald Head," and found opportunity for the relation of the surgical procedures of MM. Dupuytren and Roux in cases of "cleft palate."

In bringing to a conclusion our lengthened review of Dr. Churchill's work, we should have been glad to have given it a less qualified recommendation, than, as will be evident from our previous criticisms upon it, we feel justified in doing. We have done our best to discharge our duty, not merely with impartiality, but with that kindly and respectful feeling to which Dr. Churchill's previous contributions to professional knowledge fully entitle him. We believe that he *could* write a most excellent book on the Diseases of Children, if he would only take the trouble to do so ; and that the deficiencies of his present treatise are chiefly to be attributed to his having allowed himself to be too easily satisfied in the performance of a task, which he should have scarcely undertaken, except with the determination to execute it in the best possible manner.

The Seven Essays comprised in Dr. Beck's little work have before appeared in some of the American Medical Journals ; and one of them we remember to have seen republished in a periodical of this country. Under these circumstances, we shall refrain from any detailed analysis or review of them, simply indicating the more important deductions arrived at by the author, and recommending their attentive perusal to all engaged in the treatment of the affections of children. We do not coincide entirely in all of Dr. Beck's remarks, but the general character of his work is such as to merit strong recommendation. The intention of the author will be shown by the following extract from his fourth essay :

"It has frequently struck me, that a treatise describing with the necessary precision the peculiarities of the effects of medicinal agents on the young subject, as distinguished from their effects on the adult, has long been needed in our profession. As yet I know of no such work. The systems of *materia medica*, valuable and elaborate as they are, and in which we should naturally look for the necessary information, are confessedly deficient on this subject. The consequence is, that the young practitioner who depends upon them, finds himself continually embarrassed in the treatment of the diseases of children ; and he is obliged after all to rely upon the incidental observations gathered from works on general practice, or upon the slow accumulations of his own experience. Even works professedly on the diseases of children do not supply the want. They indeed specify doses suitable to the age, and now and then give cautions in relation to the use of certain medicines ; but they do not enter into the philosophy of the subject as it ought to be engaged upon. It is treated by them more as a matter of enlightened empiricism, than as one founded on sound and rational physiological and pathological principles. In some previous papers, I have endeavoured to offer some contributions on this subject ; and should they be the means of inducing some experienced hand properly to elaborate it, it appears to me that a greater practical benefit could not be conferred on the profession." (p. 54.)

The subjects embraced by Dr. Beck's Essays are,—the effects of Opium, Emetics, Mercury, Blisters, and Bloodletting, on the child; appended to which are some observations on Ergot, and an account of the origin of the use of Mercury in inflammatory complaints.

The more important axioms laid down by the author, may be said to be the following, in respect to—

*Opium.* 1. That it acts with much greater energy on the infant than it does on the adult. 2. That its action on the former is more uncertain than on the latter. 3. That its use should be avoided, as much as possible, in the young subject. 4. That when given, either the tincture of opium, or Dover's powder, should be employed, and administered, at first, in *very small* doses. 5. That these doses be not repeated at *too short* intervals, as is often the case.

*Emetics.* 1. That as a general rule we need not be afraid of causing the youngest child to vomit, provided the means employed to induce emesis be mild in their nature. 2. That the preparations of antimony should be used with great caution, and only when acute inflammation is present, and the patient able to bear the sedative effects they produce. 3. That the *continued use* of the potassio-tartrate of antimony, in young subjects, cannot be too specially guarded against. 4. That the agent to be employed—as a general one—to produce vomiting, is ipecacuanha.

*Mercury.* 1. That although mercury so seldom salivates infants, yet, notwithstanding this, it cannot be doubted that it affects the system profoundly, and even more so proportionally than it does the adult. 2. That although salivation may not ensue from its employment, yet by the latter a young child may be prostrated or destroyed. 3. That its employment in the young subject requires much greater caution than in the adult.

*Blisters.* 1. That they produce their effects on children in a shorter time than on adults. 2. That their local effects are greater in the former, and more likely to be followed by injurious consequences. 3. That nevertheless they may be rendered more efficient as a means of cure in the diseases of children than in those of adults, if the requisite care and knowledge direct their employment.

*Bloodletting.* 1. That the child does not support the loss of considerable quantities of blood so well as the adult does, the nervous system being especially affected. 2. That *repetition* of bloodletting is not so well borne. 3. That *local* bleeding produces greater effects on children than on adults. 4. That children ought never to be bled to syncope.

The whole of the above subjects receive able illustration, as do also other points than those we have alluded to. In the essay on 'Ergot,' Dr. Beck arrives at the conclusion, that its use has in many cases proved injurious to the child; that it exerts its action on the uterus anterior to the full term of pregnancy, and that it should "never be used except in cases where nature is incompetent to a safe delivery."—In the seventh and concluding essay, the author awards to Dr. William Douglass, of Boston, the credit of first using mercury in the treatment of inflammatory complaints. The date of this employment was 1735; whilst it was not until 1783 that Dr. Hamilton, of Lyme Regis, published his celebrated letter to Dr. Duncan, giving an account of "a successful method of treating

inflammatory diseases by mercury and opium." Dr. Hamilton states that his attention was called to the powers of mercury in inflammation in 1764 by a British Navy Surgeon, who had become acquainted with its use in the treatment of hepatitis in the East Indies. But before that time it seems to have been generally employed by medical practitioners in the American Colonies.

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#### ART. IX.

*Abnormal Nutrition in the Human Articular Cartilages, with Experimental Researches on the Lower Animals.* By P. REDFERN, M.D. London; Lecturer on Anatomy and Physiology, and Examiner at the University and King's College, Aberdeen, &c. &c.—*Edinburgh*, 1850. 8vo, pp. 86. With Fifty-two Wood Engravings.

A PECULIAR interest attaches to the subject of the growth and nutrition of Cartilage. It is the first tissue in regard to which it was clearly proved, that the series of changes in which these processes consist is not dependent upon any "action of the blood-vessels," but is effected by the vital powers of the elements of the tissue itself; being related to the blood-vessels in no other way, than as they are the conduits through which the nutrient pabulum is supplied. It is the first tissue in regard to which the same was proved to be true of the abnormal forms of the nutritive process usually designated as inflammation, ulceration, &c.; phenomena still more closely connected, in the minds of most pathologists, with some undefined "action of the blood-vessels." And it is the first tissue in which the phenomena, both of normal and abnormal nutrition, were clearly shown to consist essentially in the development and multiplication of nucleated cells; a discovery which has been more fertile in important consequences to histology and physiology than any other that has been made within the century, since on it has been built the whole cell-doctrine of animal nutrition, and the identification of this process with what had been previously determined as to the nutrition of plants. It is a peculiarity of cartilage, that operations which are elsewhere restricted to the embryonic condition of the animal tissues, are carried on with little variation in its state of full development, and through the whole period of life; and this, under circumstances that render them peculiarly adapted for attentive and accurate study.

The notion of the vascularity of cartilage was currently entertained at no distant period. In the article "Cartilage," in the 'Cyclopædia of Anatomy and Physiology,' written, we believe, about the year 1835, we find the following statements, which were in harmony with the doctrines then generally taught on the subject. "In healthy cartilage, it is true, no red vessels can be demonstrated; neither can the finest injection be made to penetrate it, nor will madder used in food colour it. But disease sometimes shows red vessels ramifying through its substance; and several other phenomena lead us to the conviction that it is at all times permeated with vessels, though they may be too fine to admit the red globules. For instance, we find cartilage assume a yellow tinge in jaundice. If we slice off a bit, the dry substance is soon moistened with a serous fluid,

which, doubtless, comes from its colourless vessels. Exposed cartilages have been known to granulate, which implies the presence of vessels, and perhaps of cellular substance. And we know that in the old and laborious, there is often not the least sign of wear, although the enamel of the teeth be quite worn away. Where a perichondrium is present, we may suppose the vessels first ramify in it before they enter the joint." Dr. W. Hunter's description of the peculiar arrangement of vessels which supply diarthrodial cartilage is then cited, and the writer continues: "It does not appear that nerves or absorbents have ever been traced into cartilages; but the phenomena of disease, pain, ulceration, &c., convince us that they are supplied with both." It is really quite amusing to turn back to such descriptions as these, which were thought satisfactory enough no more than fifteen years ago; and it is only by doing so, that we can appreciate the full import of the change which has taken place in the science of histology since that time.

Before we inquire into the objects and results of Dr. Redfern's inquiries, we shall cast a brief retrospective glance over the progressive steps by which our present knowledge respecting the structure and growth of cartilage has been attained. The *corpuscular* nature of this substance had been recognised by Purkinje, Miescher, Müller, Wagner, Valentin, Arnold, and other German histologists; but it was not until Schwann published his immortal '*Untersuchungen*' in 1839, that the import of these corpuscles as true *cells*, having a distinct and independent vitality, growing and multiplying by powers inherent in themselves, and constituting the proper organic elements of the tissue, was at all understood. The chapter "On the Structure and Growth of the Chorda Dorsalis and of Cartilage," precedes that "On Cells as the Basis of all Tissues of the Animal Body;" it being obviously the intention of the author to pave the way, by the exposition of his observations upon the first of these topics, for the reception of those more comprehensive doctrines of which he could not afford such satisfactory evidence. The following extract from the conclusion of the chapter on Cartilage will indicate this:—

"The above-detailed investigation of the chorda dorsalis and cartilage has conducted us to this result,—that the most important phenomena of their structure and development accord with corresponding processes in plants; that some anomalies and differences may indeed still remain unexplained; but that they are not of sufficient importance to disturb the main conclusion, viz.—that these tissues originate from cells, which must be considered to correspond in every respect to the elementary cells of vegetables. Thus, then, are we furnished with the first of the proofs required in the introduction; that is to say, we have shown with regard to a certain tissue that it not only originates from cells, but that these cells in the progress of their development manifest phenomena analogous to those of the cells of plants. We have now thrown down a grand barrier of separation between the animal and vegetable kingdoms, viz.—diversity of structure. We have become acquainted with the signification of the individual parts of the animal tissues as compared with the vegetable cells, and know that cells, cell-membrane, cell-contents, nuclei, and nucleoli in the former are in every respect analogous to the parts having similar names in the cells of plants. . . . The other proof for the accordance of animal and vegetable structure yet remains to be supplied, viz.—that most or all animal tissues are developed from cells. If this proof only were furnished, the analogy of such cells to the elementary cells of plants would at once become extremely probable; we may now assert that analogy so much the more firmly, since the cells of two distinct tissues have been proved in detail to cor-

respond with those of plants." (Sydenham Society's Translation of 'Schwann's Microscopical Researches,' p. 35.)

To the question of the vascularity of cartilage, it does not appear that Schwann gave much attention. He obviously perceived that the development of the tissue is perfectly independent of the penetration of blood-vessels into its substance; for he remarks:

"The formative processes of cartilage hitherto described proceed, as it appears, without the presence of vessels in the structure; such at least is the case in thin cartilages, to which probably the fluid parts of the blood can penetrate from the vessels of the neighbouring tissues. In the branchial rays of the fish, for example, I could not find any space in which vessels could have existed; throughout the structure masses of cartilage and cartilage-corpuscles were to be seen, but no canals which could have been traversed by vessels." (Op. cit., p. 27.)

He says, however, at the commencement of this chapter:

"The accordance of the structure of cartilage with the tissue of plants is of more importance [than that of the chorda dorsalis] in reference to animal organization. We have here to do not only with a more widely extended form of animal tissue, but also one which, at least in its subsequent stages of development, contains vessels, and therefore bears more decidedly the character of an animal tissue." (Op. cit., p. 15.)

In the year 1841, Mr. Toynbee presented to the Royal Society a series of 'Researches, tending to prove the Non-vascularity and peculiar uniform mode of Organization and Nutrition of certain Animal Tissues,' in which he clearly proved the true relation of the substance of cartilage to its nutrient vessels, by the special study of the course and distribution of the latter, as ascertained by careful injections. He shows that healthy cellular cartilage, such as that of the ordinary articulations, is not penetrated by blood-vessels, except when it is of unusual thickness, but that it derives its nutrition from blood-vessels which come into contact with its surface; and he altogether negatives the idea that any colourless vessels are given off into the substance of the cartilage, showing that the ordinary capillaries return by distinct loops, which often form large ampullæ, or vascular dilatations, on the surface or at the edge of the cartilage, so as to afford a larger extent of mutual contact. The thicker cartilages are traversed by vascular canals, into which blood-vessels enter; but these are at wide distances from each other, and are lined by a continuation of the perichondrium, which always forms a limit between the blood-vessels in the canals and the proper substance of the cartilage, which is never penetrated by them. Mr. Toynbee remarks, however, that "the investigations of Sir B. Brodie, Mr. Mayo, and more recently Mr. Liston, leave no doubt, that in some diseases to which articular cartilage is subject, blood-vessels are distributed throughout its substance;" and he also adverts to researches which he has himself made upon "those morbid states of articular cartilage, in which blood-vessels are prolonged into its substance, and upon the manner in which the vessels are introduced into it." In Mr. Toynbee's opinion, an articular cartilage is chiefly nourished through that surface of it, which is in contact with the surface of the bone to which it is applied, although separated from the blood-vessels of that bone by an osseous lamella composed of two layers; and he maintains that during the whole of life, the articular cartilages are undergoing gradual conversion into bone, at their attached surfaces, whereby

they become progressively thinner. The supposed vascularity of diseased cartilage was subsequently explained by Professor Goodsir (in his 'Anatomical and Pathological Observations,') as depending on the formation of a new tissue, occupying the place from which the cartilage has been removed by absorption. Of the correctness of this explanation we believe that there is no doubt whatever. Among the latest researches on the structure and growth of articular cartilage, are those of Dr. Leidy, of which we have given a short account elsewhere (vol. IV, p. 277). It is supposed by Dr. Leidy that articular cartilage derives its nourishment not only from the blood in the vessels of the bone to which it is attached, and from the *circulus vasculosus* at its circumference, but also from the synovial fluids, — a doctrine which had been previously propounded by Henle. We agree with Dr. Redfern in thinking this improbable; and looking at the structure of Cartilage in comparison with that of the *Algæ*, which are similarly non-vascular, and taking also into consideration their very slow rate of growth, we see no difficulty in the idea that, like all other tissues, they derive their nourishment directly from the blood. The observations of Dr. Leidy on the filamentary arrangement of the molecules of the intercellular substance, are of peculiar interest in connexion with the researches of Dr. Redfern, of which we shall presently give an account.

Of the various doctrines propounded by writers upon the diseases of cartilages, with regard to the changes concerned in inflammation, ulceration, &c., it would be a waste of time to dwell at any length; since these have mostly rested upon observations made with the naked eye, and have generally been based upon the notion that cartilage is vascular. The idea that vital action can go on without blood-vessels, scarcely seems to have been entertained by Surgical writers until a very recent period; but it is one with which those Physiologists who took a broad and comprehensive survey of organized nature, have been long familiar. The chief point under discussion has been, whether the ulceration of cartilage proceeds from changes in its own texture, or from some agency external to it, or from both causes, either separately or in combination. Sir B. Brodie maintained that ulceration or absorption of cartilage might take place from the action of their own vessels, or from inflammation of the subjacent bone, or from the synovial membrane. Mr. Key, denying the vascularity of cartilage, found it difficult to imagine that ulceration could take place by any action of its own, though he saw evidence that absorption of cartilage might occur without the development of a vascular membrane. Mr. Liston laid great stress upon the supposed vascularity of inflamed cartilage; but recognised the tumefaction and softening of cartilage, as a change often antecedent to ulceration. By Henle, Mandl, and other foreign histologists, it is supposed that many of the morbid alterations in articular cartilages depend upon maceration in the pus, altered synovia, &c. of the articular cavity; a doctrine whose truth is linked with that of the ordinary mode of cartilage-nutrition through the synovial fluid. Professor Goodsir was the first, we believe, to institute careful microscopic observations upon diseased cartilages, with the view of ascertaining the essential nature of the morbid actions which take place in them; and these observations led him to the conclusion, that in ordinary ulceration the action is almost entirely superficial, the agents concerned in the destruction and removal of the tissue not being the



blood-vessels, but the cells of the false membranes developed over the ulcerating surface. "A layer of nucleated particles," he states, "always exists between the loops of capillaries and the ulcerated surface;" and it is of these nucleated particles, intermixed with fibres and blood-vessels, and in scrofulous joints with tubercular matter, that the false membrane seems essentially to consist. He was not, however, unobservant of the fact, that the cells of the cartilage themselves undergo a change:

"Instead of being of their usual form," he says, "they are larger, rounded, or oviform; and instead of two or three nucleated cells in their interior, contain a mass of them. At the very edge of the ulcerated cartilage, the cellular contents of the enlarged cartilage-corpuscles communicate with the diseased membrane by openings more or less extended. Some of the ovoidal masses in the enlarged corpuscles may be seen half released from their cavities by the removal of the cartilage; and others of them may be observed in the substance of the false membrane, close to the cartilage, where they have been left by the entire removal of the cartilage which originally surrounded them. . . . The cavities of the enlarged corpuscles of the cartilage open on the ulcerated surface by orifices of a size proportional to the extent of absorption of the walls of the corpuscle, and of the free surface of the cartilage." (Anatomical and Pathological Observations, p. 18.)

Professor Goodsir seems, however, to regard these changes in the cartilaginous structure itself as secondary to the generation of the false membrane, to whose agency he attributes the ulcerative action; for he says, "The cartilage, where it is not covered by the false membrane, is unchanged in structure."—Mr. Rainey, however, states as the result of his observations, that the absorption of cartilage is preceded by conversion of its nuclei into drops of oil, which are sufficiently characteristic, and capable of being removed by the addition of ether. The intercellular substance also, he affirms, is sometimes converted into granules of oil, arranged in irregular lines running in all directions, grooving and excavating the cartilage.—By Mr. Birkett, who communicated to the 'Guy's Hospital Reports' in 1848 the results of his examination of a diseased knee-joint, it is maintained "that the destruction of articular cartilage depends chiefly upon a diseased state of the capillary vessels of the bone, whereby its nutrition is suspended, and that their disintegration simply, and not ulceration, takes place;" and further, "that this disintegration is mainly attributable to a metamorphosis or conversion, first of the nuclei, and then of the intercellular or hyaline substance, into fat." Mr. Birkett regards bone and cartilage as bearing the same relation to one another as true skin and epithelium; a notion on which Dr. Redfern makes some very pertinent observations (p. 56). When we add to the foregoing summary, that Sir B. Brodie and other surgical writers have noticed an occasional "degeneration" of cartilage into fibrous structure; and that it has been remarked by Henle and Mandl, that the nuclei of the cells of fibro-cartilages contain more oil than do those of simple cellular cartilages, and that this increase of oily matter takes place *pari passu* with the conversion of the intercellular substance into fibres, as if the two processes were mutually related; we believe that we have sufficiently indicated the state of knowledge on the subject, previously to Dr. Redfern's researches.

The treatise before us first appeared in the form of a series of detached papers in the 'Edinburgh Monthly Journal;' and having fully appreciated the value of these during the course of their publication, we are glad to meet with them in a collected form. The number of cases examined by

Dr. Redfern is scarcely as great as could be wished, not more than thirteen being here recorded. His investigation of them, however, bears the marks of great care and intelligence; and as in several of them the morbid actions presented themselves under a variety of phases in different parts, apparently in consequence of differences in their stages of progress, the actual observations are as numerous as if a large number of subjects, each furnishing but a single group of facts, had passed under Dr. Redfern's scrutiny. In several of these cases the disease affected many joints; and in some it had not been indicated by any symptoms during life, notwithstanding that it had proceeded to the extent of almost the entire destruction of one or more of the articular cartilages. The details of each case, and of the microscopical observations made upon the diseased parts (illustrated by very good figures), are related pretty fully; and this method, although somewhat tedious, is very advantageous in the investigation of a subject like the present, as affording to every reader who may be competent to form a judgment upon it, the opportunity of drawing his own conclusions from the facts set down. We must restrict ourselves, however, to a summary of Dr. Redfern's results, and of the deductions he draws from them; using his own language with some abridgment.

True or simple *hypertrophy* of cartilage occasionally occurs without any obvious change in the texture; though it is generally accompanied by a soft, spongy, and decidedly diseased condition. *Atrophy* is seen in old people, affecting a whole surface or particular parts; in the former case, it is usually healthy; whilst in the latter, there are generally traces of the former existence of other disease. *Softening* is generally very evident to the naked eye, the texture of the part so diseased being pulpy, and resembling a portion of soft fibro-cartilage. For microscopic examination, sections are difficult to be obtained. The hyaline substance is dark and indistinctly granular or fibrous; the cells are enlarged, and contain a number of other cells or nuclei; and in many parts the dark fibrous mass contains corpuscles or small cells, which appear to have been set free by the bursting of the larger ones. This change is obviously preliminary to *ulceration*; the general external characters of which are thus described by Dr. Redfern:

"Ulceration is found involving whole surfaces, the cartilage being soft, velvety, and abraded;—more frequently, such changes are found in the central parts of the cartilages alone, the circumferential part being white, dense, and generally split by numerous small fissures;—or a small spot of the size of a split pea may be the only part affected, and may present an irregularly-rounded and excavated spot, the margins of which are fringed by a multitude of projecting bands and fibres, the deepest part being also covered with similar processes. Again, limited depressions occasionally exist, and appear as smooth as if portions of cartilage had been scooped out:—in more active forms of disease, the cartilage is removed very rapidly; and, on examination, a few patches here and there may be the only traces of its former existence. Such patches are generally reddened;—their surface, though not perfectly smooth, may yet be glistening, and their general appearance is often such as might lead to the impression that they are portions of tolerably healthy cartilage, which have been left after the absorption of the other parts. The whole surfaces entering into the formation of a joint, or particular parts of the same, are not unfrequently found covered with fibrous and projecting fringes in advanced life. These are best seen on immersing the surface in water, when the fibrous processes float out, and may be carefully examined. They are of all sizes and lengths, attached at one end to the surface of the cartilage, and free at the other. They

are found most frequently on the cartilages of the patella and trochlear surface of the femur, the substance of these cartilages, at the same time, being apparently healthy, as far as can be ascertained by the naked eye.

"The process thus spoken of as ulceration may commence at any part of the cartilage,—on either of its surfaces, or in its very substance, though its favorite seat is the free surface, from which it gradually extends to the deeper parts, destroying them as it meets with them, and, at last, leaving the bone completely bare." (pp. 57-8.)

Whatever external appearances, however, the disease presents, the structural changes are constantly found, on microscopic examination, to be of a uniform character; consisting of certain changes in the structure and arrangement of the *cells*, and of others in the *hyaline substance*; and the chief variations being dependent upon the rate of progress of the disease.

"The first changes which can be noticed in the cells are their enlargement and irregular arrangement in the tissue. Some of the cells, in the foregoing observations, were found to measure  $\frac{1}{100}$ th of an inch in their long diameter, and others were five or six times their usual size, over large portions of many diseased surfaces. They become rounded, oval, or oblong, and very generally isolated and irregularly distributed, instead of being elongated and found in groups which form columns in the deeper parts. If only slightly enlarged, their nuclei remain in small number, and are irregular;—if considerably enlarged, their contents differ entirely from those of healthy cells, and consist of a mass of corpuscles, which are irregularly rounded, oblong or triangular, glistening on the surface, and varying in diameter from  $\frac{1}{100}$ th to  $\frac{1}{50}$ th of an inch, the larger ones being finely granular, or containing a small nucleolus. On tracing sections, containing such cells, towards the surface or diseased part, the cell-walls become indistinct,—patches of corpuscles are met with without cell-walls enclosing them, and the corpuscles are found at length freely mixed with the tissue. These changes are best marked in the deep cells, though they occur also in the superficial ones. The cells of the free surface enlarge and become much further separated from each other, losing their regularity of arrangement. Their nuclei generally disappear, and the whole cell often becomes finely granular, and is reduced to a mere shred of membrane, which is gradually lost. In some places the diseased cells enclose others, of about  $\frac{1}{100}$ th of an inch in diameter, which are shortly released by the bursting of the containing cells. These smaller cells are full of dark and irregular granules,  $\frac{1}{150}$ th to  $\frac{1}{100}$ th of an inch in diameter. They are often found mixed with fibrous tissue, to which they give a dark and indistinct character, as they become gradually lost amongst its fibres. The diseased cells, nuclei, or corpuscles, contain granules under many other circumstances. Indeed, the granules appear of all intermediate sizes up to  $\frac{1}{100}$ th of an inch, when they are evidently formed of drops of oil, into which the nuclei have probably been converted.

"When the disease advances with great rapidity, scarcely any changes can be seen but those in the cells. These become rapidly distended,—their nuclei are converted into granules, and the cells afterwards form a number of cavities in the tissue, the walls of which appear finely mottled—or the cells burst upon the surface, causing it to present a series of cavities; and, having discharged their contents, these enter into the formation of a fibro-nucleated membrane, with nipple-like processes which fill up the opened cells." (pp. 58-9.)

The alterations in the hyaline substance consist of a splitting into bands and fibres of all sizes; the smallest fibres being indistinguishable from those of white fibrous tissue. The bands constitute the fringed processes projecting into the joint, loose at one end, and attached to the cartilage at the other, which were just now described. This change is not so often seen in acute as in chronic cases; partly because, in the former, the rapid enlargement of the cells destroys the hyaline substance

before it has time to break up into bands and fibres; whilst, in the chronic disease, the structure is not always wholly removed, but is converted into the fibrous membrane which covers the bone when ulcers have healed. Wherever the surface presents the velvety appearance, this is found to be due to the splitting of the hyaline substance into fibres or bands. The direction of this splitting is in accordance with the natural disposition of the cartilage-cells and of the molecules of the hyaline substance, as pointed out by Dr. Leidy; that is, horizontally near the original free surface of the cartilage, and vertically in its deeper parts. Amongst the fibres or bands, rounded gelatinous masses are sometimes found, which have every appearance of being the changed contents of cartilage-cells; and when they are much subdivided, nuclei, granular cells, and ordinary cells in various stages of development,—all apparently proceeding from ruptured cartilage-cells,—are found among them. The fibrous membrane covering the bone at the bottom of old ulcerations which have healed, is of the same character as that which covers the free surfaces of cartilages in a state of chronic disease; the components of both being apparently the degenerated elements of the cartilage, namely, fibres resulting from the splitting-up of the hyaline substance, and nuclei and other corpuscles discharged from cartilage-cells. All these changes may occur, it is positively affirmed by Dr. Redfern, without the implication of any other tissue:

“As long as the cartilage is the only tissue affected, no other structures are found than those which have been named, and no pus is formed. But, whenever the bone or synovial membrane becomes diseased, these parts throw out the usual exudation, in which new structures grow. Upon the synovial membrane, the exudation becomes organized into lymph or converted into pus, or it may be mixed with tubercular granular matter if it be in a scrofulous individual; in the tissue of the bone, plastic and fusiform cells and primitive filaments are formed in the exuded matters. In all such cases, the new elements just named appear, and have a few changed cartilage-cells mixed amongst them.” (p. 60.)

During the occurrence of these morphological changes, important alterations are taking place in the chemical composition of the cartilage-substance. In proportion as the hyaline substance assumes a fibrous character, in that proportion do its characters change from those of chondrin to those of gelatin, thus approximating the white fibrous tissue. It is stated by Dr. Redfern, that similar chemical and morphological changes are found to occur in conjunction, during the formation of white fibrous tissue from coagulated blood-plasma. The nuclei of the cartilage-cells appear to be frequently resolved into fat. In one instance, a number of masses of carbonate of lime were found in the very substance of the cartilage; and, in another case, perfect octohedral crystals of oxalate of lime were found in the midst of the fibrous processes of its surface, although no such crystals could be discovered in any texture in which the diseased parts had been enveloped.

It is clearly proved, then, by Dr. Redfern's observations, that the essential nature of all the known diseases of articular cartilages is a change in the nutrition of their texture; that this may occur without any disease in the synovial membrane, or in the bone; and that all the morbid actions go on independently of any peculiar action in the blood-vessels. “That these processes do not *depend* upon the conversion of the nuclei and

hyaline substance into fat, is shown by such changes being only occasional ones; the whole substance of the cartilage being frequently destroyed, without the production of fatty granules in a single cell." The fibrous metamorphosis, or degeneration of the hyaline substance, and the abnormal development of the cells, appear to be the two elements of the morbid process; and, though usually concurrent, each may take place independently of the other. It is well worthy of remark, that in the greater number of instances where "granular" cells were found, these had all the characters of the compound granular bodies, or "exudation-cells" of other tissues, although they are clearly proved to be generated by the metamorphosis of the ordinary cartilage-cells. This, of course, suggests the question, whether exudation cells are not elsewhere generated by a metamorphosis of the normal elements of the tissue in or upon which they lie, instead of *de novo* from the blood. That ulceration of cartilage is not dependent upon the action of a membrane upon its surface (as supposed by Mr. Key and Prof. Goodsir), is affirmed by Dr. Redfern on two grounds, on which he lays a by no means unwarranted stress,—namely, "that in many, if not the majority, of cases of ulceration of cartilages, the process commences without the presence of any membrane whatever; whilst, on the other hand, there can be little doubt that cartilages are very frequently covered by a thickened synovial, or a newly-formed vascular membrane, without an ulceration resulting." We do not doubt the accuracy of Prof. Goodsir's observation; but it by no means follows, that because he found a vascular and cellular membrane dipping down into the honey-combed surface of the cartilage, this membrane is the agent in producing the destruction of its substance; and we think that Dr. Redfern's idea of its formation, as partly consequent upon the degeneration of the cartilage-structure, is much more consistent with the facts he has adduced.

Although Dr. Redfern is quite opposed, and we think with justice, to the notion that the changes in the texture of the cartilages can be produced mechanically, yet he fully recognises the influence of mechanical causes in the induction of the morbid state of nutrition on which these changes immediately depend. "Injuries, as a blow or a fall, may be the exciting causes, as well as a constitutional disposition to disease, the latter being the cause where a great number of joints are affected at the same time." On this last point he does not dwell as we should have expected him to do, considering the very curious phenomena which he has detailed in the former part of his paper. Thus in Case VII, which was one of "Cancer of the Liver, Brain, Kidney, and of every other organ except the Heart," there was also very general disease of the cartilages; those of all the articulating processes of the lumbar, and several of the dorsal vertebræ, of the right sacro-iliac synchondrosis, of the right hip and knee-joint, of both elbow-joints, and of the carpal articulations, being affected, in various degrees, with the usual forms of abnormal nutrition, and being found in some parts almost entirely wanting. In Case IX, there was "Disease of nearly the whole Articular Cartilages in the Body, with Porcellaneous Formations in a few spots;" and although the affection had proceeded in some parts to the almost entire destruction of the cartilages, yet there had been no symptoms indicative of diseased action in the joints:

"On many of the cartilaginous surfaces there was a dense fibrous membrane, which was evidently produced by splitting of the hyaline substance of the cartilage

into fibres, and by changes in the cells, which had reduced them to granular nuclei, or isolated patches of dark granules. In the elbow-joint an apparent conversion of cartilage-cells into osseous lacunæ, by calcification of their walls, was observed." (p. 44.)

The nature of the disease which led to the death of the patient was not very positively made out; but it seemed to be of a tuberculous nature, especially affecting the nervous centres. In Case XII, again, in which death took place from fever, there was "General Disease of the Articular Cartilages." These cases seem clearly to indicate that abnormal nutrition of the cartilages may proceed from a "constitutional" cause; in other words, may be dependent upon an abnormal condition of the blood. And we think it not improbable that, as Dr. Redfern suggests (p. 47), in many cases in which the disease appears to be limited to one articulation, it really affects several joints in a minor degree, although it may not manifest itself by any of the symptoms usually regarded as characteristic of it. For, as many of the observations recorded by him prove, the disease *may* proceed to the extent of the entire destruction of the cartilage, without the patient's attention being particularly attracted to it. And the hypothesis of its constitutional origin, as Dr. Redfern justly remarks, explains the ill success of merely local treatment, especially in hospitals; whilst it accounts for the benefit occasionally derived from the administration of cod-liver oil, with a good diet and a pure atmosphere.

Dr. Redfern ventures to advance what will to many appear a most improbable supposition,—namely, that the *pain* which is commonly accounted an indication of disease in articular cartilage, is not to be so regarded; but that it is to be attributed to the implication of some other texture. This is a very important point in diagnosis; and we shall, therefore, do Dr. Redfern the justice of quoting his remarks in full:

"Now, if it be true, that disease having the same essential structural characters in all cases, at times goes on to the complete destruction of the texture, without the patient or his attendant being aware of its existence; and in others, causes such violent pain and hectic irritation in the system as to require amputation, the fact is of extreme importance, as, so far as I know, no similar occurrence takes place in any other texture whatever.

"I shall not presume to deny, that uncomplicated disease of articular cartilages produces excessive pain, since this is stated very positively on the high authority of Sir B. Brodie, and by most surgical writers. And yet, on looking over the numerous cases which have been related by Sir B. Brodie and other authors, I find but few which will aid in the determination of the question. Uncomplicated disease of cartilage is of very rare occurrence, if we exclude the cases of fibrous degeneration; and carefully collected reports of numerous cases, with histological examinations, are necessary to establish a fact so decidedly at variance with everything we know of disease and its characters in other textures. This will appear more evident from the following considerations:

"1st. Nerves have never been found in cartilages, and there is not the slightest reason for believing that they contain any, for they give no evidence of sensibility in man or in animals on being exposed or sliced away. The cuticle, enamel, and the substance of the brain are in a similar position. They contain no nerves, and give no indications of sensibility, when parts are removed, or otherwise destroyed. Moreover, it is well known to every pathologist, that an immense portion of the whole bulk of the cerebrum may be utterly destroyed by disease, without a single recognised symptom or other mark of the existence of the lesion. Hence the value of pain.

"2d. It is certain that disease of cartilages may go on for years, and even be cured by the efforts of nature, without the knowledge of the patient, and that the essential characters of such disease are similar to those of other diseases of the same tissue, which are supposed to be the cause of excessive and exhausting pain.

"3d. The symptoms supposed to indicate disease in the cartilages are so closely allied to those produced by affections of the bones, that a positive diagnosis is very difficult to make until the bone becomes exposed, and then the symptoms are, at least, as easily attributable to the disease in the bone as to that in the cartilage. Indeed, M. Richet, in the work before named, when entirely ignorant of the real nature of the changes which take place in diseased articular cartilages, gives several good anatomical and other reasons why the symptoms supposed to arise from disease in them should be referred to the bones.

"4th. Pus has never been shown to be formed from the substance of diseased articular cartilages, nor from effusions into their texture. At the time when pus is formed in joints, the pain becomes very severe, though there may have been but little previously, the very formation of pus indicating that other textures than the cartilage are affected, and that the pain proceeds from the disease in them." (pp. 63-4.)

We cannot but think that our author has succeeded in establishing a strong case. The absence of nerves in cartilage is a fact just as fully established as the absence of vessels; and it does not seem possible to understand how the cartilage-substance itself can ever become sensible. The case is a very different one from that of other parts which are ordinarily insensible, but which become the seat of severe pain in disease,—such as the bones and ligaments, the abdominal viscera, &c.; for *there* nerves are always present, and it is only because the usual actions of the parts do not produce any impression upon them, that we are not conscious of those actions. We can understand how pain should be experienced, when the soft vascular tissues of the interior of a bone are tumefied by inflammation so as to cause pressure on their nerves, or when a ligament has been unduly stretched, or when the determination of blood to an internal mucous membrane renders its nerves unusually susceptible of impressions; but we cannot conceive how even the slightest pain can be felt, as the direct consequence of an abnormal condition of a tissue in which there are no nerves whatever, either in the healthy or the diseased state. It does not do to beg the question, and to affirm that *because* pain is felt in joints whose articular cartilages are undergoing ulceration, *therefore* there must be nerves in those cartilages, although they have escaped observation. Precisely the same kind of argument has been used about the vascularity of cartilage; and, as we have seen, it has turned out to be entirely fallacious. There is not a single tissue, not even the epidermis itself, of which the absence of nerves as well as blood-vessels from its substance may be more certainly predicated, than it may be of cartilage; and it appears to us that this should be taken as an axiomatic fact, in any attempt to account for the pain and other symptoms of disease in the joints.

With regard to the natural termination of disease in the cartilages, Dr. Redfern gives us less information than we could wish; the following short extract containing all that he has to say upon the subject:

"After ulceration has gone on for a considerable time, the part may be healed without any evident exudation. The structure which is formed, is precisely of the same character as the fibro-nucleated membrane found on the surface of cartilages in a chronic state of disease, and it is probably formed in a similar way to the latter, by such a new arrangement of the particles of the cartilage as converts it into fibrous tissue." (p. 64.)

This, of course, refers to morbid alterations of structure entirely limited to the cartilages, such as may take place, according to Dr. Redfern's observations, without any external indication of disease. In cases where pain is felt in the joint, Dr. Redfern would conclude that the bone or synovial membrane, or both, are more or less implicated in the disease; and it may either happen that they are secondarily affected, or that the morbid action commenced in them, and has subsequently extended itself to the cartilage. Ankylosis, it is obvious, cannot occur without a diseased state of the surface of the bone; and this is by no means necessarily consequent upon the almost entire destruction of the cartilages (as is shown by some of the observations narrated), the ends of the bones being still covered by a fibro-nucleated membrane, which seems to be the result of their degeneration.

In order to test and extend the information derived from the observation of disease in man, Dr. Redfern has performed a series of experiments upon the cartilages of the lower animals; these experiments were continued during a whole year, and were nearly ninety in number; the progress of each one was carefully watched and recorded from day to day; and on the death of the animal, a lengthened microscopical examination was made of every texture in the neighbourhood of the parts operated on. The results of similar experiments were always found to correspond, when performed upon animals of the same species, in all cases in which some accidental occurrence had not taken place, such as the supervention of acute inflammation in the joints, pressure on the articular surfaces resulting from dislocations, &c.; and thus they admit of being generalised with considerable precision. We must forbear entering, however, into the minuter differences which distinguished the results obtained on the cartilages of the ribs, the sternum, the ear, the nasal septum, and the joints, when these were subjected to irritations of various kinds, such as the making of incisions into them, the insertion of setons, the application of ligatures, actual cautery, acids, or alkalis; but shall cite Dr. Redfern's account of the phenomena common to all:

"When changes are induced in the costal, articular, or nasal cartilages, by any cause whatever, they are essentially similar in all cases, and affect both the cells and inter-cellular substance. The cells become enlarged, rounded, or oblong, and irregularly arranged; their nuclei giving place to a multitude of corpuscles, or to a series of granules. The walls of the cells next become identified with the hyaline substance, when the corpuscles are set free, and begin to undergo other changes, elongating and becoming very evidently converted into the nuclear fibres of fibrous tissue, or reduced to small patches of granules, which may only become visible on acting upon the fibrous mass by acetic acid. During the enlargement of the cells, and especially at the period of discharge of their contents, the hyaline matrix becomes softened, and allows the corpuscles to be freely moved in it, or pressed out altogether; whilst, at a subsequent period, fibres appear in the interspaces of the corpuscles, and take on the characters of those of the white fibrous tissue. In this way, both elements of the cartilage are concerned in the formation of the fibrous membrane, which is destined to heal the breach in the tissue, for the hyaline substance is converted into the white fibres, and the corpuscles of the cells into nuclear or elastic fibres.—Or, an altogether different result may take place, especially in the case of articular cartilages, the whole thickness being converted into a calcareous mass; the cartilage cells becoming lacunæ, and calcareous matter being also deposited in the intercellular substance, rendering it dark and incapable of transmitting the light, the surface all the while remaining perfectly smooth and on its former level." (p. 84.)



This production of a dense fibrous membrane, or of a smooth calcareous lamina, appears to be Nature's method of effecting a spontaneous cure; the tendency to which is very decided in uncomplicated lesions of cartilage, especially in such as have been artificially induced. In no case do these changes seem to extend further into the healthy cartilage surrounding an injured spot, than appears necessary for the formation of sufficient fibrous texture to constitute the cicatrix. It is a remarkable result of Dr. Redfern's inquiries, that these changes occur much less rapidly after incisions, than they do after great irritations; so that even after seven weeks, the texture of incised cartilages presented very trifling alterations. Hence the belief has arisen in the minds of many observers, even Dr. Leidy being among them, that the two surfaces of an incised cartilage never unite. In Dr. Redfern's experience, however, it was always found that the changes consequent upon incision were of the same kind with those following upon irritation, although more tardy in their rate of progress; and that, if sufficient time was given, they would go on to the production of perfect fibrous tissue, and thus to the reunion of the divided surfaces. The fibrous tissue, which heals up breaches in the texture of cartilages, may contain (according to Dr. Redfern) the yellow as well as the white fibrous elements; the latter being formed by the elongation of the discharged corpuscles of diseased cells into nuclear fibres; whilst the former originate in the altered hyaline substance. All these results are in precise accordance with the inferences drawn from the study of the human articular cartilage in states of disease; and each set of observations thus affords a valuable corroboration to the other.

From the account which we have given of Dr. Redfern's researches, our readers will have gathered our high appreciation of their value. We consider them to have been conceived and carried on in the true spirit of scientific investigation; and regard their results as important, not merely from their going far to establish the true nature of the changes concerned in the abnormal nutrition of cartilage, but also from their bearing upon those fundamental questions of general pathology, which modern physiological research has brought into such prominent view and placed in such a novel aspect.

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#### ART. X.

1. *Sheriff Court, Dumfries. Brieve of Furiosity, Russell versus Russell.*—*Dumfries*, 1849. 12mo., pp. 48.
2. *Commission of Lunacy on Mr. Vicars, of Liverpool, August 21 and 22, 1849.*—(*Sun Newspaper*, August 22, 1849.)
3. *Case of Hayward versus Coombs, tried before Mr. Justice Erle and a Special Jury, December 20, 21, 22, 1849.*—(*Daily News*, December 22 and 24, 1847.)

IN pursuance of the intention which we expressed in our last number, we proceed to notice some cases which have occurred subsequently to that of Miss Nottidge, in which the erroneous *dictum* of the Lord Chief Baron appears to have exerted a prejudicial influence. Before proceeding to

them, however, we may stop to remark, that, by a recent judgment in the Vice-Chancellor's Court, one of the brethren of the Agapemone, who had married a sister of Miss Nottidge, has been ruled to be unfit for the guardianship of his own child—a decision which is strangely inconsistent with the view of the character and proceedings of the brotherhood, which seems to have been taken by both judge and jury on that remarkable trial.

The next case, *Russell versus Russell*, which we shall notice as briefly as possible, occurred in Scotland, and was marked by the same circumstances as the last—an erroneous charge by the judge, and an erroneous verdict by the jury. It was an inquisition proceeding under a "brieve of furiosity," as it is called, granted at the instance of the patient's brother, and directed to the Sheriff of the County, commanding "diligent and faithful inquisition to be made, if William Russell, sometime watchmaker in Longholm, thereafter in the United States of America, and now, and for some time past, an inmate in the Crichton Royal Institution, Dumfries, be *incompos mentis*, prodigal, and furious; viz., that he neither observes time nor measure in his expenses, but squanders and dissipates his possessions; and how long that state of furiosity has continued; and if so, then, who is nearest kinsman to the said William Russell on the father's side, and if that nearest kinsman be provident of his own affairs, and properly qualified to administer the affairs of another." The inquisition lasted two days, and the case for the promoter of the inquisition was first entered on. There were, in the terms of the brieve, three questions for the jury to determine: 1st. Was William Russell insane, and how long had he been in that state? 2d, who was his nearest kinsman by the father's side? 3d, was that kinsman competent to manage the patient's affairs? We have, however, only to discuss the first question.

As is necessary in such cases, the alleged lunatic was presented to the jury, for the purpose of being examined, and the following scene took place:

"His lordship beckoned Mr. Russell forward to a position immediately below the bench, and addressing him, asked him if he knew what he had been called into court for? After he had repeated the question, Mr. Russell said in a loud tone: My lord, I am rather dull of hearing. Most certainly I understand what I am here for; am I a fool?"

"Mr. Gordon—Have you any property in Moffat?"

"Mr. Russell seemed to apprehend the nature of the question perfectly, but his answer was by no means a pertinent one, nor very intelligible in itself. It referred to (as far as we could understand it) the settlement made by his grandmother; and, intermingled with the statement, there was a large modicum of law terms, of which 'life-rents,' 'fee simple,' &c., formed a portion.

"Mr. Gordon addressed a number of similar questions to Mr. Russell; such as, What is the value of your property? Have you any houses in Moffat? Were there any disputes about them? Did you get your property when you came from America? and repeated some of them several times. The statements elicited were, in almost every case, so far as our impression went, very loosely connected with the questions asked, and were generally discursive, and occasionally, to all appearance, quite irrelevant. The remarks were sometimes shrewd, however, and even witty, so as to convulse the court with laughter. [We hope that by the word court, the reporter means the idle audience, and not the judge and lawyers.]

"Mr. Gordon repeated for the fourth or fifth time—Did you get possession of your property?"

"Mr. Russell—When I arrived at Greenock from America, and being ill with

my leg—my leg, not my head, recollect—(laughter); for you see I was asked to clean a gun-lock, and, in doing so, one of the slugs lodged in my leg, and made me lame. Well, I came here for the benefit of my leg—not my head, recollect—and when I arrived at Greenock, Mr. Wightman said he would pay my passage home. Well, I reached Moffat, and commenced to work in my garden, but Dr. Smith's swine sadly annoyed me by breaking into it. I complained of it to Dr. Smith, and also to Mr. Stewart, of Hillside, and wanted to get the swine shut up in a stable—

“Mr. Gordon—Did you get possession of your property?”

“Mr. Russell—Permit me to explain. My brother and family all received me kindly, and did all they could to make me comfortable. Hugh asked me to clean a watch for him, and I said I would do it mathematically (laughter). I told Hugh he had never been from Moffat to get a knowledge of mathematics, and I said, if he did not know how to fix a pendulum, I would point out to him the place of suspension, and centre of gravity. He did not seem to take it up properly; and the Apostle Paul has said, better speak a few words to a man of understanding, than a thousand to those who cannot comprehend them. Throw a stone into the sea, and it will stand as soon as it reaches the centre of gravity—

“Mr. Gordon—What became of the property?”

“Mr. Russell (apparently in great surprise at the question)—The property! it was in the same place as when I left it; the property did not run away. (Laughter.)

“Mr. Gordon—Had you any disputes with your brother?”

“Mr. Russell—When I came home bad with my leg—not my head, recollect—(great laughter).

“It would serve no good purpose to attempt to follow out Mr. Russell's statements further, as, in a rambling way, he related several instances connected with himself and his family affairs, which, though scarcely intelligible at first, must have been considerably cleared up in the course of the prolonged inquiry which ensued after his examination, first by Mr. Gordon, and subsequently by his lordship. What we have given above is, we think, a fair specimen of his answers. Before being dismissed, his lordship, at the request of Mr. Hallard [the counsel who opposed the brief], asked him, Do you believe in God Almighty?—Most undoubtedly. Do you believe in our Saviour?—Certainly; do you think I am a fool? Do you know what Deism is?—A disbeliever, one who does not believe in the Holy Saviour; such a man I despise.” (pp. 7-9.)

The narrative given above clearly shows that Russell's mind was unsound, and the subsequent evidence distinctly proved that he laboured under various delusions, and was dangerous. Before, however, giving a brief abstract of this, we may be permitted to express our regret that such a scene took place in open court. Here was a poor lunatic subjected to an examination before a mixed audience, who seem to have looked on the matter as an excellent farce, at which they were convulsed with laughter. On the first manifestation of such unseemly behaviour, it ought to have been checked by the judge, and if persisted in, the court ought to have been cleared. There is no wonder that in Scotland inquisition is so seldom had recourse to. Though the scene does not appear to have affected the patient, many under the circumstances would have been violently excited. Russell, however, was probably delighted with enacting a prominent part in the scene. If this were so, it would be another index to the state of his mind.

It appears from the evidence that the patient had gone to America many years ago, and returned about ten years before the date of the present inquiry. His head seems to have been injured, as a cicatrix on the scalp was visible. When an attempt was made to examine this, he always endeavoured to divert attention to some other part, as his leg or arm, and

this accounts for his repeatedly saying in his examination, "my leg, recollect." On his return he was lame, and his mind was observed not to be the same as it used to be. When he began to move about, after being detained at home for about six weeks by his lameness, he used to go to the street, and talk in a most excited manner respecting his relatives and his property, the large sums which he had sent home from America, and his determination of turning out of a property, which once belonged to the family, an individual who had bought and paid for it. He collected crowds about him, haranguing them on the same subjects, and his brother's conduct to him. He not only threatened, but violently attacked his mother and brothers, striking and throwing articles, such as a candlestick or washhand-basin, at them, &c. At last he was sent to Dumfries asylum on the 4th of February, 1840, and remained there for about a month. How or why he was liberated does not appear. On his release from the asylum he continued to act in the same manner, and was sent back on the 30th of December, 1841. The subsequent history of the case we give in the following extracts from the evidence of Dr. W. A. F. Browne, the accomplished medical superintendent of the Crichton Royal Institution, Dumfries, under whose care the patient was placed :

"The state of his mind, when first received, was such that witness regarded him as insane, and still regards him in a state of insanity. He was so divested of reason when brought first, that he was seen to uncover his person, even in the presence of females. He is essentially the same still; but now, under discipline, could not say that he entertained any hopes of his recovery, or even improvement. He has frequently been heard to threaten violence to parties both connected with the house and others. From what witness had seen of him, has no doubt that he would commit violence to persons to whom he entertains antipathy, in circumstances of excitement. Has no doubt of this whatever. Thinks that he is not a person, who might, with safety to the public, be set at large. If contradicted as to the delusions he entertains, becomes excited. The delusions are very numerous, and may be specified as referring to property to which he lays claim; and that various individuals are suborned and bribed to confine, calumniate, and do him evil. He has delusions also as to seeing people whom he never sees, repeatedly announcing to witness that he has had interviews with persons from Moffat, who gave him information about his property there, though he (the witness) knows for a certainty, that such individuals had not been at the institution. Then there are delusions as to hearing rumours, which are manifestly absurd in themselves. As an instance, he often says that he hears rumours of respectable persons in Moffat and Dumfries being intoxicated—persons who it was extremely improbable had ever been in that state, and regarding whom the patient could not possibly have heard any reports to that effect. Mr. Russell has also another delusion in regard to the American Minister—the ambassador of the United States to Great Britain—whom he expects to interfere for the vindication of his rights. These were the most prominent delusions, but there are many others besides. Sometimes the patient is cunning, plausible, and gentle; but repeating over and over again his ideas with regard to property, and his other delusions, without reference to previous statements to the same effect; at other times, when contradicted, or when his word is disputed, he becomes irritated, excited, and uses threats. . . . If no reference was made to his delusions, a considerable time might elapse without their existence being detected. With a previous knowledge of the case, a person could scarcely be any great while in detecting them; but without that knowledge an hour or two might elapse.

"*Cross-examined.*—He is neither frantic nor furious just now. Old age has to do, in all probability, with a certain impairment of memory which witness noticed, but not with the delusions. If a party, whom Mr. Russell disliked, were to con-

tradict him in regard to his property, that would render him excited and violent. Has no knowledge in regard to the validity of his claims to property, but the grounds on which he makes his claims vary very much. This is in regard to the property at Moffat. Has heard Russell say that he had sworn his allegiance as an American citizen. Is not aware that his reference to the American ambassador was associated with his American citizenship. He has not the liberty of the grounds at the institution without the presence of an attendant. When conversing with persons to whom he has no antipathy, and not in circumstances of excitement, he speaks coherently and soberly.

*"By the Sheriff.*—Mr. Russell's insanity consists in his delusions chiefly, but not entirely. The other grounds are, that his memory is impaired, that his mind is confined to one subject—the assertion of his fancied right to some property; a circumstance which would not infer insanity, were it not that the patient's mind is insensible to new impressions. He has been eight years in the institution, and has scarcely talked of anything else, and witness has great doubts if he could tell anything about what occurs in the institution, or about any person there. There cannot be grounds for supposing that the property should amount in value to £30 at one time, and £2000 at another. One of witness's reasons, therefore, for thinking his idea on that point a delusion is, that he varies his account of its value so much. He has stated that he has a right over property at Selkirk, as well as at Moffat and the neighbourhood. Another evidence of delusion is, that statements have been supplied to witness by parties who know the real state of affairs, which show that those of Mr. Russell are unfounded. Another is, that the claims of Russell were never asserted in any proper way, but always with violence. There is still another, which is the existence of other delusions with reference to the same subject, viz., the bribing and suborning exercised towards him, which witness knows to be false. As an instance which may be adduced of the delusions which Russell labours under, he frequently told witness that a person named Greene, from Moffat, had called upon him; when it was certain, from the evidence of keepers and others, that no such person had called at the institution. From the fact of his mind being engrossed with one subject, and labouring under delusions, thinks Russell is incapable of managing his own affairs. Thinks that his reason generally must be vitiated in its application to ordinary matters by the delusion under which it habitually labours. He talks coherently sometimes, but at the same time not rationally. He talks rationally on some points. He has a share of reason more or less; but that must be affected by his delusions; and as he would proceed on false principles, it would not guide him in the common affairs of life.

*"By the Chancellor of the Jury.*—Thinks Russell conducts himself more violently than a sane individual would do, if really deprived of his property in the manner supposed by him. Does not find him quiet and rational in all other subjects disconnected with the property; but on some he talks rationally—in regard to his garden operations, for instance, and some parts of his reminiscences of America. Talks seldom of the passing events of the day, though, like other patients, liberally supplied with newspapers and periodicals. Does not keep up a continuous knowledge of affairs in America, though he refers to that country more than to Britain.

*"By the Sheriff.*—Does not now uncover his person, as at the time when he was first placed under restraint. The patient is not naturally cleanly in his habits." (pp. 14-18.)

This clear and straightforward evidence, sifted in every way, was corroborated by Dr. Smith, formerly surgeon in the Royal Navy, who knew the patient and his family well; Dr. Grieve, consulting physician to the institution; Mr. Wingett, apothecary to the same; and Dr. Blacklock, a practitioner in Dumfries. Taken in conjunction with the appearance which the patient made in court, one would suppose that it would have

been impossible for the jury to have come to any other conclusion than that he was unfit to be at large, and incapable of managing his affairs. His insanity was confirmed. He was garrulous, plausible when he chose, excitable, and occasionally violent. He had delusions, which led him to claim, not only property which *might* be his, but also property which *could not possibly* be his. He saw people who were not in the place, and heard rumours which were never uttered. Under the enlightened government of the asylum, he was so far quiet, orderly, and decent in his habits; but it is quite clear, that if freed from control, he would be filthy, indecent, troublesome, and dangerous. Dr. Brown was the most competent man to guide the jury. He is well known to have devoted himself, with great success, to the study and treatment of insanity. His experience has been ample; he has been many years in daily intercourse with the insane, and knows all their habits, and modes of thinking and acting; and the institution under his care is one of the best in the country. Thus, familiar with insanity, and having the patient for eight years under his observation, his statements ought to have outweighed all those brought forward on the opposite side. The case-book of the asylum, too, was produced and read, in which the history of the case was from time to time entered, thus precluding all possibility of error respecting the facts.

To rebut this strong and overwhelming evidence, a witness was produced, who declared that, eight years previously, she did not consider the patient either a lunatic or furious, though she admitted that he came to her house in a passion, and accounted for his state by saying, that he had been "angered by his brothers and sisters, because they would not give him his property." Another declared, that Russell had lived for four months in his house, in 1841, and that he thought him a very rational and sensible man,—that he frequently saw him at the asylum, and always found him very rational and sane,—that he saw him the day before the inquiry,—and that he knew no difference in him since 1841, except that he was a little deafer. A third witness was adduced, to prove that he did not consider him to be insane in 1840,—that "he was always quite civil, though he sometimes got elevated with spirits, like any other man." On cross-examination, he stated that he had not seen him since 1840 till the day before, and that he saw no difference in him mentally, but thought that he was improved in body. Another witness swore, that he did not think that the patient, who lived in his house for a month, in 1841, was a furious lunatic; but, on the contrary, that "he was civil and rational all the time he was with him." He had not seen him since. Two other witnesses gave similar opinions. One "never considered him a furious lunatic, but thought him an intelligent talking old gentleman." He had seen him the day before, and conversed with him for a few minutes; and his opinion of him was the same as he had formed before he was sent to the asylum. He considered him to be of sound mind, and that he was an intelligent man. The other never heard of any incoherency, such as he would have expected from an insane person.—This is all the non-medical evidence brought forward to prove that Russell was sane, and requires no comment.

Two medical men were called on the same side,—Mr. Burnside, a surgeon, and Dr. McCulloch, a physician, in Dumfries, the most important parts of whose evidence we shall extract.

**Mr. Burnside—**

"Saw William Russell for the first time along with Dr. Barker, on Thursday last, and conversed with him for about an hour and a half. It was not his impression that he was a furious lunatic, but, on the contrary, he thought him a rational and intelligent man.

"*Cross-examined.*—Visited him only once. Knew that lunatics had frequently lucid intervals, and it was quite possible that Mr. Russell might be in such a state when he saw him. Had not directed his mind to cases of insanity more than other medical practitioners. Had not been examined in court before in a similar case. Was quite satisfied of Russell's sanity at the time he visited him.

"*By a Jurymen.*—He put questions to him, and he did not evade one of them. There was no error in articulation, and no want of fluency.

"*By the Court.*—The questions he put to him were upon American jurisprudence, his journeyings in the country, as to how long he had been there, and his opinion about American manners and customs. In talking of American jurisprudence, Mr. Russell repeated to witness the substance of a letter he said he had written to an acquaintance, a strong democrat, which, for latent observation and acuteness, surpassed many a sane man's production.

"*By a Jurymen.*—He did not wander in his conversation, but seemed quite disposed to talk upon any subject.

"*By the Court.*—The substance of the letter which Mr. Russell repeated, was about American manners and laws, the superiority of English government to American, and the facilities afforded to American criminals to evade punishment, even for taking the life of a neighbour. Thought no man that did not possess a healthy and vigorous mind could pen such a letter. Spoke also about his confinement in the Crichton institution. Answered him to a question, how long he had been in that place. Could not recollect how many years he said he had been there, but the answer was just in the way in which a sane person would have given it. He spoke, also, of the way he was taken, and at whose instance he had been placed there. Did not speak of Dr. Smith that witness recollects of. Spoke of Dr. Russell, the historian, and said he was a relation of his; spoke about Mr. Gordon, also. Was quite sure that these statements were made in answer to questions. Witness's conversation with Mr. Russell, during their intercourse, was not exactly such as would take place with ordinary, intelligent men, casually meeting; witness's object being principally to put questions, in order to test Mr. Russell's sanity by his answers. Had no difficulty in getting a word in, in conversation. From the opportunity witness had of judging, Mr. Russell appears perfectly harmless and sane." (pp. 29-30.)

The statements of this witness appear to us to be so extraordinary, that one can hardly conceive that he was referring to the same patient as had been produced in court, and it therefore becomes necessary to examine them in detail. It may be suggested, that he had examined the patient during a *lucid interval*. This, however, is impossible; as, from the nature of Russell's case, it was impossible that he could have a lucid interval. Remissions and exacerbations no doubt occur in his case, as in most others; but certainly no remission could take place to such a degree, and for such a length of time, as would constitute a lucid interval, and render the patient for the time sane. Erroneous and very confused views are entertained on this subject by non-professional, and, we may say, by not a few professional, men. Lucid intervals occur in very few cases indeed, and that only in certain forms of recurrent insanity. This explanation must therefore be abandoned. A more plausible solution is to be found in the inexperience of the witness in such cases. "He had not directed his mind to insanity more than other medical practitioners;" in

other words, he knew little or nothing more on the subject than non-professional men in general do. "He had not been examined in court before on a similar case;" and, we suspect very much, had never examined a case of the kind before. It requires some practice before a man can conduct the examination of a suspected patient, so as to decide whether he is sane or insane; and we have seen experienced men fail for hours to explicate the delusions of a lunatic, and occasionally require to have a second interview. A great deal depends on tact and a previous knowledge of the patient's history; but, even with these, much difficulty is often experienced. The witness saw Russell only once, and for the first time, for an hour and a half; he had no experience in such cases, and, at the most, a very imperfect knowledge of the subject; and nothing is more likely than that he would be deceived. He is not, however, the less an incompetent witness. Laying aside, however, his opinions and conclusions, we have still to examine the facts stated by him. Questions were put to the patient, respecting what is called American jurisprudence, in talking of which he repeated the substance of a letter about the American manners and laws, superiority of the English to the American government, and the facilities which criminals in America had for evading punishment, which could not, in the witness's opinion, have been written, except by a man possessed of a sound and vigorous mind. Now, we question how far the witness himself was capable of entering on the subject of American jurisprudence, and suspect that, what is thus so pompously designated, was only some common remarks on the mode in which the law is administered in the United States. We have no doubt that the patient wrote the letter which he quoted. Be it remarked, however, that this letter was written by him when he was in America, many years ago, before he was insane,—that his notions respecting the manners, customs, and government of the country were formed when his mind was sound,—and that, with the exception of his property and his supposed wrongs, he was more inclined to talk on that subject than any other.

Nor let it be thought wonderful, that he should be able to repeat the substance of a sensible letter after so long an interval, or that his information respecting what passed before him in America should be correct and unmixed with delusion. His memory was stated to be impaired; but, as very frequently happens, it was in respect to recent occurrences, and not to those more remote. We know several instances in point, of a much more marked kind. An old gentleman, who had been more than thirty years insane, was able to repeat a shrewd business letter of some length, which he had written before his malady overtook him, and explain all the circumstances connected with it, and yet he could not recollect the occurrences of the day before. As long as he spoke on this subject, he was quite amiable and gentlemanly; but, nevertheless, he was dangerous, and inclined to be filthy and indecent. He had a delusion, that he could communicate with people at any distance, by various absurd means, and receive answers from them instantly, whatever they were doing. When this was doubted, he became furious, and assailed those near him; and it required the greatest care and attention to keep him cleanly and correct in his habits. From a stranger he could conceal his delusions very effectually. In conversation, he managed always to revert to the letter, and the circumstances connected with it. In another case, a lady,



who had been attached to one of the smaller courts of Europe, became insane shortly after her return to this country, and continued in that state many years. Though she had full access to the newspapers of the day, she did not keep pace with the events, either of this country or of that in which she had spent so long a time. Still, she delighted to talk of those whom she had met abroad, their manners and customs, mode of government, &c., and told many interesting anecdotes in a very pleasing manner. To strangers, she appeared to be calm and rational. Even to those around her she seldom broached her delusions, and carefully concealed them if an attempt was made to draw them out. This patient, however, was under the influence of various delusions. She believed that there were evil spirits in some cats, and that these cats she could discover,—that certain infants had demons for souls,—that it was her duty to extirpate these cats and babies,—and that a good spirit always attended her, who pointed out the particular cats and babies to be destroyed,—that her friends had conspired to ill-use her and deprive her of her property,—and that a sign would be given her, when she was to signal vengeance on her foes. She was several times in confinement, but never recovered, though she improved. Whenever she was liberated, she became worse; and, we need hardly say, that she was too dangerous to be left long at liberty. We believe that she is now dead.

We can easily conceive, that Mr. Burnside, or any other medical man, unaccustomed to examine cases of insanity, would have been deceived by any of these two patients, especially if he saw them only once, and for no great length of time. We do not see any other fact in the evidence of this witness requiring remark. His evidence, in our opinion, does not weaken the force of that of Dr. Browne, and of those who corroborated him.

Dr. McCulloch stated, that he had often to inquire into cases of insanity, and had frequently granted certificates in such cases; had seen Russell eight or nine years before, and again the day preceding, and the first day of, the trial. He did not consider him a furious lunatic, and had granted a certificate, to the effect that he could not see anything in the state of Russell's mind rendering his confinement in an asylum necessary. He candidly admitted, that Russell might possibly be a lunatic, and yet seem not to be one when he saw him; but qualified his admission by the following statement:

"Usually, though not invariably, when the attention of lunatics, even in a lucid interval, is brought to the particular subject on which they are insane, they exhibit insanity."

The witness here evidently confounds a remission of the symptoms of insanity with a lucid interval. "By a lucid interval," says Shelford, in his treatise on the Law of Lunacy, "is to be understood, not a remission of the complaint, but a temporary and total cessation of it, and complete restoration to the perfect enjoyment of reason upon every subject upon which the mind was previously cognisant;" and this is the view taken by all medical authorities. It is impossible, therefore, that any patient could have a lucid interval, if the touching on any subject, on which he had been insane, or was supposed to be insane, produced a manifestation of the disease. We have already stated that, in cases like that of Russell's, no lucid interval ever occurs; and that it is only in certain forms of

recurrent insanity that such an event takes place. To return to Dr. M'Culloch's evidence:

"Witness entered upon the subject, on which his friend, Dr. Brown, supposes Russell to be insane—his property, namely, and other collateral points. Of course he had not the same means of ascertaining Russell's state of mind, as if he had been in the habit of daily visiting him for years."

Now, it is a mistake to suppose that Russell was insane only on one subject,—his property, and points connected with it. He was insane in general, and had many delusions, some unconnected with his property. But it must be borne in mind, that delusions do not constitute insanity, but are only among the symptoms of the malady. The existence of the disease may be inferred from other circumstances than the manifestation of delusions. We hear often, that delusion is the test for insanity; and then, when insanity is disputed, the great object is to show, that what are brought forward as delusions, are not delusions, but realities. The moment that it can be shown, that the supposed delusion is founded on fact, as when a man raves about his property being wrongfully kept from him, and it is proved that he is entitled to some share, about the amount of which there is a dispute, it is immediately inferred that he is not insane. Though, however, it could be proved that all a patient's delusions were founded on fact, (and such cases we meet with,) he might, nevertheless, be shown to be insane by the exaggerated view he took of the facts, by the extravagant way in which he attempted to accomplish a laudable object, by his habits and conduct, and by his general conversation. We do not allude here to what may be called purely moral or impulsive insanity, which, though frequently connected with unsound intellect, is not always so. Delusions, no doubt, are a good test of insanity, and most satisfactory proof, but they are not the only test.

Dr. M'Culloch then described the conversation which he had with Russell respecting America—his misfortunes there—his return home—his disagreement with his relatives, &c., in the course of which it appeared that, though he conceived himself to have been very ill-used by his brothers, he nevertheless applied to one of them for a gig, to proceed to Langholm on some business. This apparent inconsistency he explained by saying, that he was in a state of destitution, and had no other way of getting his gig, except by applying to his brother. In conclusion, Dr. M'Culloch could discover nothing in his manner and appearance indicative of insanity, and nothing peculiar about him, except that he spoke loud, which might be accounted for, either by supposing that he felt himself wronged, or by his deafness.

On reviewing the whole evidence, it appears to us that there can be no doubt that William Russell was insane, a fit patient for an asylum, and quite incompetent to manage his affairs. The evidence adduced by the promoter of the brief, is clear, explicit, and every way satisfactory. That of Dr. Browne, and the medical men who corroborated him, is so full on every point, as to leave no room for hesitation. The history of the case for years was detailed, and five medical men, after careful examination, were unanimously of opinion that the patient was insane. Dr. Browne had observed and managed the patient for eight years; Mr. Wingett, his assistant, had also daily opportunities of satisfying himself regarding his state; Dr. Grieve, the consulting physician to the asylum, had ample

means of coming to a correct conclusion ; Dr. Smith knew the case from the beginning ; and Dr. Blacklock, though his opportunities were limited, saw enough to convince him that the patient was not sane. The account which all these witnesses gave of the conduct, conversation, appearance, habits, and delusions of the patient, tallies completely with what was observed when he appeared in court.

The evidence of the two medical men cited by the opposer of the *briefe*, sinks into insignificance before that to which we have just referred. They had not proper opportunities for coming to a satisfactory conclusion. Dr. M'Culloch admits that he might have been mistaken ; and Mr. Barnside seems either not to have had sufficient experience to conduct the examination, or to have kept clear of the points on which the insanity of the patient might have betrayed itself. His statement of what occurred during his interview with Russell, is so irreconcilable with other evidence of an unexceptionable kind, and with the exhibition which the patient himself made in court, that we can account for it only in one or other of these ways.

We pay no attention to the non-medical evidence, which in this case, like many others, was most contradictory in its nature. One class swore to the insanity and violence of the patient, while another swore, not only that he was quiet, harmless, and sane, but that he was rational, sensible, and intelligent.

We pass over the speech for the pursuer. The counsel who opposed the *briefe* endeavoured to show that because Russell had undergone the ordeal of examination in court without being excited or violent, Dr. Browne's opinion that he was dangerous must be erroneous. We have already stated that we believe the patient to have been too well pleased at appearing as chief actor in the scene, to be at all inclined to be violent. It appears too, from a part of the evidence which we did not think it worth while to quote, that he was aware of the necessity of restraining himself, and had resolved to keep quiet for fear it should harm his case, —a degree of prudence by no means unfrequently exhibited by the insane in similar circumstances. The counsel then stated that what had been called delusions, had in a great measure been shown to be realities ; but supposing that not to be the case, they did not justify his confinement, in corroboration of which he quoted the opinion of the Lord Chief Baron, on which we have already commented. He endeavoured to special-plead the medical evidence against him, and to throw the whole blame of the patient's excitement on his friends ; adducing the patient's generally orderly conduct in the asylum, and inferring that if he were not ill used by his friends, he might with perfect safety be set at liberty. The remainder of his speech was confined to questions respecting the property alleged to belong to the patient, and its management or attempted management, into which it is unnecessary to enter, as we are not called upon to discuss that part of the case at all.

The sheriff then summed up in a long speech, legal, critical, and meta-physical, in which Erskine's '*Institutes of the Law of Scotland*,' Dr. Conolly's '*Indications of Insanity*,' Milton's '*Paradise Lost*,' Spenser's '*Faery Queen*,' Shakespeare's '*Caliban*,' Mrs Shelley's '*Frankenstein*,' are all pressed into his service. It was adorned with everything save a knowledge of insanity. After stating that ideas the most vague and loose prevailed in regard to the nature of insanity, of which the results of Miss

Nottidge's case, and of this, afford ample proof, he told the jury that *it was in vain for them to refer to medical authorities for the nature of insanity; for what they said upon it presented a confused, troubled, perplexed, and contradictory testimony*, and quoted as an instance, Dr. Conolly's definition of insanity, which he professed himself incapable of understanding, and which he thought they would be as little able to comprehend. Now, whether Dr. Conolly's definition be correct or not, is a question on which we do not wish to enter at present; but that it is intelligible enough, few will deny. Having thus summarily disposed of the medical authorities, he proceeded to criticise the definition given by Erskine, in his 'Institutes of the Law of Scotland,' a work held in the same veneration in that country, as Coke in this. Let us see whether we shall be much the wiser for it, and the comments of the learned judge.

"Mr. Erskine's definition of insanity was before him, and he prayed the attention of the jury to it, seeing that what Mr. Erskine said was law. He enumerates three kinds of insanity: the first consisting of fatuity or absolute idiocy; the second being furiosity, when the subjects of it are furious persons, incapable of taking care of themselves; and the third, where the mind is only partially affected, and where the individuals are, in minor matters, able, to a certain extent, to manage their own affairs. Insane persons coming under the first two classes, are wholly unable to take care of their own affairs, and for them the law allows a brieve of idiocy or furiosity. Those of the third class can take charge of them, and for them the law allows the appointment of a curator, through the Court of Session. The case of fatuous persons is hopeless from the beginning, but it is different with what are called furious lunatics. Mr. Erskine's definition was of more importance than that of medical men. They all knew what furious meant, in the common acceptance of the term; but its legal meaning was not quite the same as its ordinary meaning. 'Furious persons,' says Erskine, 'cannot be said to be deprived of judgment,'—the judgment here meaning the mind and mental faculties in general, and not, in particular, the faculty of judging; 'for,' Erskine adds, 'they are frequently known to reason with acuteness.' And what else, he (the Sheriff) might ask, had they to characterise a sane man, but that he reasons with acuteness? 'But an excess of spirits and an overheated imagination obstruct the application of reason to the ordinary affairs of life.' This phrase, 'excess of spirits,' is loose, and not in Mr. Erskine's exact style. Why, a man might, when in liquor, or elevated with joy, be said to have an 'excess of spirits.' Lower down, however, Mr. Erskine makes his definition more accurate: 'an overheated imagination,' which obstructs the application of the reason to the ordinary affairs of life. Here, then, lies the main secret. The furious lunatic is a man who reasons with acuteness; but an overheated imagination or fancy impedes the application of reason to common purposes. It must be obvious, that, if a man cannot use his faculties, it is just the same as if he had none."

This, no doubt, is obvious; but it is not so very obvious to us, as it may be to the learned judge, how a man, who cannot use his faculties, can reason acutely. The whole of the passage appears to us to be confused and contradictory.

The sheriff goes on to say, that however heated or excited the imagination may be, provided there be no obstructions to the application of the reason, there is no disease; that it is only when the creations of fancy are considered to be real, that they are evidence of mental disease;\* and that there is nothing else at the bottom of madness of the kind under

\* This was illustrated by referring to Spenser, Shakespeare, Milton, Mrs. Shelley's Frankenstein, &c.

consideration, but a disordered fancy. He then gives an enumeration of the faculties of the mind, which he states to be "perception—memory—the power of combining, comparing, and abstracting the ideas which the perception takes in and the memory stores up; and the faculty of imagination, which is separate from all the rest." After this, he informs us that man does not call in the imagination to assist the application of the reasoning faculties. We leave the reader to make what he can of these statements, and content ourselves with stating that a disordered imagination is no more at the bottom of insanity than any other disordered faculty of the mind; that we consider his enumeration of the mental faculties to be incomplete; and that man may, and does, on many occasions, call in the imagination to assist in the application of the reasoning faculties. We are precluded, by the length of this paper, from entering into a discussion of this interesting question, but we may be permitted to ask, whether imagination can act alone—whether it be not always, in a healthy state of mind, directed and controlled by the other faculties—and whether the want of this direction and control may not be the cause of its morbid action.

We quote the following, merely for the purpose of protesting against the doctrines laid down, and referring the reader to what we have before stated on the subject in this and our former article:

"Other mental delusions there no doubt are, which arise from defective processes of reasoning, but these, strictly speaking, are not insanity; as, for instance, when a man, by dint of reasoning, gets himself into the belief that there is no God. There had been examples of men, with great mental powers, professing to have reasoned thus, though the strange result to which they arrived was not owing to a diseased fancy. In the same way delusions of a religious nature arose; those who were subjects of them may be wrong in their belief, but were not, on that account, to be considered mad.\* As an illustration of this, he might adduce the Nottidge case, to which his learned friend, the counsel for the respondent, had already referred. Miss Nottidge believed that one Prince was God Almighty, and as a proof of her sincerity, granted him a check for £6000; but the jury, for all that, found that she was not mad. The court was right in England, and he thought that a Scotch court would have given the same decision. True enough, reason was given to keep us right, but it did not follow, that if we used it improperly or wrong, that we were therefore mad."

We cannot help remarking here, that there is no analogy whatever between the supposed case of an atheist and that of Miss Nottidge; and that, even according to the sheriff's own doctrines, Miss Nottidge was insane, seeing that in her, "an overheated imagination obstructed the application of reason to the ordinary affairs of life."

When the sheriff intimated his opinion that a lady, who believed a man

\* While these sheets were in the hands of the printer, a circumstance occurred which illustrates the absurdity of the doctrine laid down by the Lord Chief Baron, and adopted by the Sheriff of Dumfries.

On the night of Tuesday, the 30th of April, cries of "help" were heard proceeding from one of the basins in Trafalgar Square. It appeared that the assistance was required by a young man who was standing up to his middle in the water, holding up the seemingly lifeless body of a female. He stated, that as he and his wife were passing, he noticed a woman clamber up the side of the basin and deliberately pitch herself in, head foremost. He ran to the spot, and not seeing her rise, supposed that she had been stunned. He went in and picked her up. She was removed to the Charing Cross Hospital, and in two hours recovered so far as to be able to speak, when she began to call her God to witness how she had performed the rite of baptism according to her belief, viz., by immersion, she being a baptist. *When asked why she attempted to commit suicide, she seemed amazed, and said that she had been merely baptizing herself, but had no recollection of the pool she went into for the purpose.*

to be God Almighty, and in proof of her sincerity gave him £6000, was not insane, we were so far prepared for what was to follow, when he came to the consideration of the case before him. Accordingly, he told the jury that they were not to place much dependence on the medical evidence, as it was of no use, compared to what they themselves had seen and heard; but that if they did depend on any of it, it should be that of one who had made the subject his particular study. He explained the patient's rambling statements in court, and the fact, stated by some of the witnesses, that they could not get an answer from him, by observing that he had a great mass of things mixed up in his mind with the question, and in order to include them all, he was obliged to start a long way off, but never got to the end, and soon forgot all about the question. This, one would think, should have led the sheriff to conclude that Russell was of unsound mind. On the contrary, he thought that there was no reason to suppose that he did not possess the ordinary faculties of his mind, though he might not possess them in strength and vigour. Then the question was, were his delusions so strong as to obstruct their exercise. He did not think that Russell was uttering delusions. He stated that his complaints were well founded, and that his tale was true, and there was no proof to the contrary. All that could be said was, that he was too much taken up about himself and his sufferings, which rankled in his heart; but they were not, therefore, entitled to consider him mad. His eye had no unsteady glare; it was not, perhaps, indicative of superior wisdom, but it did not indicate insanity. Except when excited by the pursuer's agent, there was no manifestation of anger, though in the circumstances in which he was placed, it was scarcely to be expected that he would be calm, however sane; and when dismissed, he quitted the court in the quietest way possible. He attributed the violence shown by the patient to anger. His assaulting his mother proved him to be a bad son, but not a madman. He advised them to throw aside all the medical evidence except that of Dr. Browne; and after paying him a high compliment, proceeded to dispose of it as follows. Russell's uncovering his person in the presence of females, though it proved a morbid feeling, was no evidence of insanity. Dr. Browne's other reasons for considering him insane, he did not think satisfactory. Dr. Browne considered that he was labouring under a delusion respecting his property, because he valued it at one time at £20, and at another at £2000; because he had been told by the patient's brothers that his claim was a delusion; and because he had not urged his claims in a legal manner. The sheriff was of opinion, that there was nothing in the difference of the value which Russell put on his property; as the sums appeared to bear such a relation to one another, that the first might be the annual, and the second the full value, and this might have been his meaning. No reliance was to be placed on the statement of his brothers, because they were the men who, he complained, persecuted him. His not urging his claims in a legal way, was explained by the fact, that he was poor, and had not the means. The alleged delusion respecting the American ambassador had not been shown to be so in reality. Two kinds of delusions he thought proved—those as to seeing people, and hearing rumours. This portion of the charge he concluded as follows:

"It might be, that the respondent labours under them all, both these, and the others he had before noticed; but looking at them with regard to legal proof, he

thought only two kinds of delusions had been proved. If William Russell really laboured under all the delusions named, his mind would be so engrossed by them, that he would be unable to attend to the common business of life, and keeping Erskine's definition before them, the jury would have no difficulty in finding for the pursuer. The diseased fancy, in such a case, would carry its unhappy victim into a maze, a mystery, an unfathomable sea; but if his delusions were confined to seeing people whom he does not actually see, and hearing rumours which he cannot in reality hear, it would be for the jury to say if these were of such a character as to incapacitate him from managing his ordinary affairs."

Thus, bit by bit, was the whole evidence of the unfortunate man's insanity special-pleaded away. By the same process, half of the inmates of our asylums might be proved to be sane, and set at liberty. Looking at the whole charge, we cannot compliment the sheriff on his possessing any accurate knowledge of the subject, or even sufficient knowledge of insanity to enable him to appreciate the value of the evidence placed before him. It is part of his official duty to visit the asylums within his county, and to liberate such of the inmates as he may determine to be improperly confined. Of this he is the sole judge, and from his judgment there is no appeal. We counsel him, on such occasions, for his own sake, not to follow the course which he adopted at the trial, but to be guided by men who are thoroughly acquainted with the subject of which he is at present profoundly ignorant. Although the judge ordinary of the district, let him not disdain to avail himself of the opportunities afforded him in his official visits to the Crichton Royal Institution, but endeavour to make himself acquainted with the various forms which insanity assumes, and to profit by the instructions which Dr. Browne is so able to give him, and which, we have no doubt, he will be as ready to bestow.

The jury, confused with law, metaphysics, and special pleading, retired, and, after an hour and a quarter's deliberation, returned, with an unanimous verdict, that the first point of the brief was not proven; but that *William Russell laboured under certain delusions*, which, however, did not *totally* incapacitate him from attending to the ordinary affairs of life. According to the direction of the court, the question was put to each jurymen, *cognosce or not*, when it was unanimously answered, *not cognosce*. The sheriff stated, that he entirely agreed in the verdict. The effect of this is, to set the patient at liberty. The verdict was received by many persons in court with a burst of applause, which was quickly checked.

We have now to state an extraordinary circumstance, which has come to our knowledge, that the *soi-disant* friends of the patient seem to have been puzzled with their victory; for the patient remained for nearly a fortnight in the asylum, and was at last removed by his brothers, his supposed persecutors. We have heard, but cannot vouch for the fact, that he has been since confined. Be this as it may, we suspect that both judge and jury are by this time convinced that Russell was insane.

We are in possession of the secret history of this case; but, as it throws no light on the subject of insanity, we do not think it necessary to detail it.

The next case is that of Mr. Vicars, of Liverpool, to which we refer more for the purpose of offering a few remarks on a particular form of insanity, than on account of anything interesting in the case itself.

A commission having been directed by the Lord Chancellor to inquire into the state of mind of Mr. Edward Vicars, who had been a traveller in the wine and spirit trade, and was stated to be possessed of £6000, the inquiry took place before Mr. Commissioner Barlow, on the 21st and 22d of August last. Three counsel appeared for the supposed lunatic, and as many for his sister, on whose application the commission had been issued. The facts of the case may be very briefly stated.

The alleged lunatic was 46 years of age; and though a widower at the time of the inquiry, had been rendered unhappy, according to the statements of some of the witnesses, by his wife's intemperance. He was at one time a good man of business, but for some time had been addicted to drinking, and at one time was certainly insane, as will appear from the following extracts:—Mr. M. Vicars, the defendant's brother, stated that, "He first saw something strange in him in 1847. On one occasion he lost his luggage, and told him he knew he should do so. On one occasion his brother told him it was no use eating, as he had no bowels, and all that he eat went into his legs; he also said that he had no brains. He then went to an asylum, from which he escaped in October, 1848. Saw him afterwards at his sister's house. He conversed a great deal on electricity, said that it was the moving power of the earth, that people might be made mad by it, but that *he* could not be played on by it, as he was in the secret. He said that he could prove from the Scriptures, that people could be made mad by electricity, that they could converse by means of it at any distance; and that, by it, he had heard voices from heaven. While his brother was in confinement, he had his case investigated by Dr. Thornby, who reported in writing upon it; the result of which was, that he did not like to advise his sister to release his brother. The witness then proved, that several letters which had been read were in the handwriting of his brother. In these he alluded to the power of electricity to make people mad, and stated that madhouse keepers were a set of thieves and rogues; that they would soon all be put an end to; and that all the gold of California should not prevent him from putting an end to them. . . . . His brother on one occasion, after he had escaped from the asylum, said that he had pledged himself before God and man to do away with lunatic asylums, and that he would conquer or die."

Another witness who knew Mr. Vicars well, stated that, "In 1846, he was called to see him, the servant telling him that his master was going mad. Mrs. Vicars told him, that Mr. Vicars had run down a steep place into the river. When witness saw Mr. Vicars, he was in bed praying, and scratches were on his body. Mr. Atkinson, the surgeon, was sent for; and on his coming said, let him get out of his bed. Mr. Vicars then got out of bed, and attempted to get out of the window, but witness and Mr. Atkinson prevented him. He then got into bed again and began to pray; and he continued in that state for a week. On one occasion he told witness that he had been to hell; that he had had a very pleasant journey; that he touched at a very nice island; that they found that the old hell was full; but there were two new ones, which were very pleasant. On another occasion, when witness called on Mr. Vicars, he said that he was very glad to see him, as he was going to die at six o'clock that evening, and he wanted to tell him what to do about his funeral. He took out his watch, and said that he had only an hour or two to live. Witness said he must go, when



Mr. Vicars said that he had better stop till he was dead. Witness went away; and when he returned, he found Mr. Vicars making a good supper. The witness then alluded to other acts of singularity committed by Mr. Vicars, particularly his refusal to take food, in which he persevered for upwards of a week, and thereby reduced himself almost to a skeleton. On one occasion witness went with him to Liverpool. He did not speak for the first twenty miles; but on the occasion of witness getting out of the carriage, Mr. Vicars said that no earthly power should move him from the carriage, and it was with the greatest difficulty that he was got out. He was afterwards taken to a lunatic asylum in the neighbourhood of Liverpool. On one occasion he told witness, that a whole workhouse might be made mad by electricity. On another occasion he said that he would fight for the lunatics as long as he lived, and would spend all the money he had for them."

The evidence quoted above was fully corroborated, and there can be no doubt that Mr. Vicars had been insane, and was very properly placed under restraint. The question, however, for the jury to consider was, whether he was *then* insane, and of this no evidence was adduced. They had, therefore, no alternative, but to find that the defendant was not proved to be insane. It was contended that he was always sane except when he was excited by spirits, and that therefore it would be unjust to deprive him of freedom of action, both in respect to his person and property. Now to us that admission is sufficient to prove that he was really insane; although, as the case was placed before them, the jury were warranted in giving the verdict which they did. The truth is, that the case was ill got up; and we have heard it rumoured, that some previous steps taken in reference to the patient were irregular. While, therefore, we must acknowledge that the verdict of the jury was justified, we must at the same time state that the friends of the patient, for such we venture to designate him, were fully warranted in placing him under restraint in the first instance; and afterwards, when he escaped, in applying for a commission. To make good our position, it is necessary to consider,—first, the form of the disease under which the patient laboured,—and next, the necessity for restraint on one or more of the grounds adverted to in a former portion of this paper. The form of the disease to which we allude has been denominated *dipsomania*, and has been recognised by Esquirol, Marc, and other competent authorities, which therefore renders it unnecessary for us to prove its existence. To the term *dipsomania* we object, as it does not correctly describe the disease, which consists not in thirst-mania, but in an irresistible propensity to indulge in intoxicating liquors, or stimulants which produce the same effect. We therefore prefer the term *oinomania*, by which it has already been designated by a writer who has given a short account of the disease.

*Oinomania* then consists in an irresistible impulse to indulge in intoxicating substances, whenever and wherever they can be procured. It is quite different from drunkenness, which however may induce it. Many men at the festive board invariably become excited or intoxicated, who in general are sober, or even abstemious, and whose consumption of wine and other stimulating beverages is, in the course of a year, much less than that of those who are never seen under their influence. Others take their daily allowance, and consume a larger quantity of alcoholic drinks than is consistent either with health or sobriety. Others again become daily drunk

after dinner. All these, however, possess self-control, and can at any time when it suits their convenience abstain from stimulants, though placed before them and even urged upon them. On the contrary, those affected with the disease cannot do so; and however convinced they may be of the impropriety of their conduct, or however anxious they may be to resist, they feel themselves to be, and in reality are, impelled by an overpowering propensity to do that which they know to be wrong, and from which they derive no pleasure. The case of a young man of fortune, 26 years of age, related by Macnish, affords a good illustration:

"Every morning before breakfast he drank a bottle of brandy; another he consumed between breakfast and dinner, and a third shortly before going to bed. Independently of this he indulged in wine, and whatever liquor came within his reach. Even during the hours usually appropriated to sleep, the same system was pursued—brandy being placed at his bed-side for his use in the night-time. To this destructive vice he had been addicted\* from his 16th year, and it had gone on increasing from day to day till it had acquired its then alarming and incredible magnitude. In vain did he try to resist the insidious poison. *With the perfect consciousness that he was destroying himself, and with every desire to struggle against the insatiable cravings of his diseased appetite, he found it utterly impossible to offer the slightest opposition to them.*"

The same author mentions another instance of a patient, who in reply to the remonstrances of one who pointed out, in a clear and forcible manner, the distresses of his family, the loss of business and character, and the ruin of health entailed upon him by his conduct, replied, "My good friend, your remarks are just; they are indeed too true, but I can no longer resist temptation. *If a bottle of brandy stood at one hand, and the pit of hell yawned at the other, and I were convinced that I should be pushed in as sure as I took one glass, I could not refrain.* You are very kind; I ought to be grateful for so many kind, good friends, but you may spare yourselves the trouble of trying to reform me; the thing is out of the question."

We repeat, that the disease does not consist in the habit of becoming intoxicated, but in the irresistible impulse which drives the unhappy being to do that which he knows to be pernicious and wrong, and against which he makes many a vain struggle. He derives no pleasure from taste, for he drains the cup, of whatever liquor it may be, at a draught; or from society, for he generally avoids it. His only gratification is momentary, and consists merely in his being freed from the overwhelming misery, mental and bodily, which the non-gratification of his insane impulse inflicts upon him.

This form of disease is hereditary, and frequently occurs in individuals in whom there is a predisposition to other varieties of insanity. We have met with several very marked instances. We may mention one. The grandmother was insane. Her two sons manifested a predisposition to the disease, one by engaging in all political excitement and public affairs to the neglect of his business, and the other by suffering from several attacks of suspicious melancholy, and one or two of *oinomania*. It is a remarkable fact, that all the sons of both brothers were liable to *oinomania*, and all have died but two, while all the daughters have manifested a tendency to

\* That is, under this disease he had laboured.

high religious excitement, and occasionally showed some indications of *erotomania*.

We have had ample means of observing the phenomena of *Oinomania*, and have found that there are three varieties of the disease; the acute, the recurrent, and the chronic.

The *acute* is the rarest of the three, and the most easily treated. We have seen it occur after hemorrhage in the puerperal state, in nursing prolonged beyond the strength of the patient, on recovery from fevers, after excessive venereal indulgence, in some cases of masturbation, and in some forms of dyspepsia. When it proceeds from any of the first four causes, it is easily cured by restoring the patient's strength, and there is every probability that disease will not recur. When it arises from the two last, it is not so easily removed, and is very apt to assume the chronic form. In the treatment of this variety of *oinomania*, the most modified form of restraint, delicate surveillance, referred to in a former article, is all that is necessary; and it would therefore be quite unjustifiable to remove the patient to an asylum. Change of scene, cheerful society, and some interesting occupation, will be found useful adjuncts to other means of treatment.

The *recurrent* form of *Oinomania* is much more frequent than the acute, though less frequent than the chronic, and comes on in paroxysms. Patients so affected may abstain for weeks or months from all stimulants, and may even loathe them. By degrees, however, they become uneasy, listless, depressed, and irritable, and feel restless and incapable of exertion. They are aware of the impending paroxysm, and struggle against it till the impulse becomes irresistible, and then they drink to an extent which to those unacquainted with such cases would appear to be impossible, and which would destroy any ordinary man. During the paroxysm, there appears to be a greater tolerance of stimulus than the constitution exhibits in its normal state. We may give one case which came under our notice. An educated gentleman of good property had spent some time in a tropical climate, and returned in bad health. In a few months he seemed to have recovered, and appeared to be even in robust health. At uncertain times he became dyspeptic, and occasionally bilious, when, after dosing himself with blue pill and black draught, he would betake himself to some watering place for a few weeks, whence he would return in good health and spirits. He married and had a family, and he seemed to be in the enjoyment of as great happiness as usually falls to the lot of man. He lived generously, but not too freely, and he never exceeded in his potations. At last, when he was about forty years of age, he was observed every two or three months to become restless, irritable, and depressed, and when spoken to on the subject attributed it to his being bilious. Instead of going to a watering place, however, he absented himself from home on the pretence of going to fish, and nothing was heard of him for a fortnight; when he returned, he looked jaded and listless. He then abstained altogether from wine, or even small beer, and became cheerful and vigorous as ever. It was afterwards discovered, that on none of these occasions he had been more than fifteen miles distant from his house, and that he had spent the whole time in drinking by himself in his bedroom, occasionally sleeping for an hour or two. Soon, however, he adopted another plan, and did not leave home during his paroxysms. On some pretext or another he sent his family

away, and then made his arrangements. He went to his wine merchant's, and bought porter, ale, whisky, brandy, and sherry. These he paid for at the time, that they might not appear in his accounts; and ordered them to be packed in a box with a lock and key, and to be addressed to his care for some one else. He desired that it should be ready by a certain hour, when he called with a hackney coach and received it. He then drove home, and had the box carried to his room. He next provided himself with salt beef and biscuits, told his servant that he was to be very busy, and could not be interrupted, therefore that he was to tell any one who called that he was not at home. He now shut himself up in his room with his store and a few books, and was not seen for eight days. During these eight days he had consumed—

12	quart	bottles	of	sherry,
6	"	"		whiskey,
6	"	"		brandy,
12	pint	bottles	of	porter,
12	"	"		ale,

giving an average of a bottle and a half of wine, the same quantity of spirit, and three pints of strong beer daily. At the end of the time, he took a warm bath, had himself shaved and his hair dressed, and drove out in an open carriage into the country. He left not a single trace of his operations in his rooms. He became as abstemious as formerly, went to his club, visited his friends, and no one for a moment could have suspected that he had spent eight days in the way he had done. The paroxysms, however, became more frequent, and his wife became acquainted with the whole. It unfortunately happened that it was at the commencement of a paroxysm that she made the discovery; and her attempts then to prevent him from obeying his insane impulse produced in him the most furious excitement, and she was compelled to allow him to follow his own course. When he recovered, he told her everything, lamented all that had taken place, but declared to her, as well as to his confidential medical friend, that it was utterly impossible to avoid the evil. Accordingly, he persevered in the same course, and in his paroxysms was occasionally so excited, that it became necessary to adopt some stringent measures, both for his own sake and that of others. He was told that if another attack occurred, it would be absolutely necessary to confine him; that, according to his own statement, he was insane at these times; and that his medical adviser and another were prepared to grant the necessary certificates. He replied that he believed that they were right,—that they might do what they thought best,—but that nothing would be of any use. Accordingly, he was sent to an asylum, where, by judicious treatment, he was cured in about two years. He has now been free from his malady for nearly eight years, and, what is more remarkable, can take a moderate quantity of wine, without feeling a desire for more. Such cases are by no means rare, though they seldom end so favorably as that now described. The disease is, in most cases, allowed to go on, till the patient falls a victim to the physical effects of intemperance, or becomes maniacal or imbecile, or, as more frequently happens, is affected with the chronic form of the malady.

The recurrent form of oinomania is observed in those who have suffered from injuries of the head, in some women during pregnancy, at the

catamenial periods, on the approach of the critical period and afterwards, in individuals whose health has suffered by living in tropical climates, and in men whose brains are over-worked. When it occurs after injury of the head, the case is hopeless; but as such patients are in general very violent, it is necessary for the safety of the community that they should be secluded. In other cases it admits of cure, but only after long treatment, of which seclusion is a necessary part; and, with the single exception of pregnant women, this should never be attempted at home. It would be very advisable to have an hospital adapted for such cases; as they are pests in an asylum, either public or private, and require particular arrangements for themselves. As such an institution does not exist, there is no alternative but sending them to an asylum; where, however, they ought to be kept completely distinct from other patients, to whom they seldom do any good, and from mixing with whom they could derive no benefit. They require the strictest surveillance, and if kept at home or boarded in a private house, there is no possibility of preventing their obtaining stimulants, unless they be strictly immured in a room, and no one allowed to approach them who has not been diligently searched. This strict confinement would be incompatible with health, and though it might prevent the propensity from being gratified, would not eradicate it. An asylum, therefore, is at present the best place for treating such cases. We have said that the disease requires long continued treatment, and that this treatment, to render it effectual, must take place in an asylum or hospital specially devoted to such cases. This, then, itself justifies seclusion. But it may be shown to be necessary on other grounds. Patients affected with this variety of oinomania, are very frequently outrageous during their paroxysms, and not unfrequently suicidal before or after them. On all three grounds on which we have stated seclusion to be justifiable, the confinement of such patients may be defended.

The patient ought always to be confined at the beginning of a paroxysm, and the seclusion ought not to be for less than two years. We have seen shorter periods tried, but without permanent success. It may be said that it is hard to confine them when they are free from a paroxysm, and appear to be perfectly rational. It must, however, be borne in mind, that the disease is not cured,—that there is only a lull,—and that it must be looked on precisely in the same light as recurrent mania, no patient suffering under which would any one be fool-hardy enough to set at liberty during the period of quiescence which occurs between the paroxysms. Patients labouring under recurrent oinomania are very plausible, and, what is a lamentable fact, are often men endowed with good intellectual powers, generous feelings, and many loveable qualities. They will represent to their friends and to the medical man in charge, that they feel and know that they have quite recovered,—that they are perfectly satisfied and grateful for what has been done for them,—that they feel themselves to be quite different men, and are anxious to take their place in the world, and perform their duties. All this will be urged the more anxiously and pertinaciously, the nearer a fresh paroxysm is approaching; and if they be unfortunately set at liberty, ample proof will be afforded in a few days of the impropriety of yielding to their wishes. Should their appeals to their friends and the medical officers

prove ineffectual, they will then demand their liberation as a matter of right,—appeal to their conduct since they came to the asylum,—write long letters to their acquaintances and friends, to magistrates, commissioners, &c., which shall appear in all respects rational and conclusive, and, in all probability, induce some one to interfere to put an end to what he will consider a piece of monstrous injustice. Meantime the officers will be annoyed in various ways, and the commissioners or visiting magistrates will be beset with applications for the patient's liberty. The motives of the friends will be questioned, the opinion of the medical officers will be undervalued, and, in all probability, the patient will effect his object to his own detriment. If not set at liberty, he will attempt to escape, and, if he succeeds, will raise a clamour against all concerned. True it is that ultimately the truth will be discovered in the individual case; but that will not prevent the same course from being followed on another occasion. It is commonly said of a patient, that he is quite well as long as he does not drink; and so of a truth he is, though he does not become insane because he drinks, but drinks because he is insane: the indulging in liquor is a consequence of his insanity. The case is not to be confounded with that of the drunkard; and this is a sufficient answer to those who sneeringly ask, if we would confine every man who gets drunk. It is not the vicious habit, but the uncontrollable insane impulse, with which we have to do at present. There are, no doubt, individuals who become mad whenever they indulge in liquor to excess; but theirs is a different case; and though we may consider them proper subjects for restraint, it is not easy to point out how that may be effected in present circumstances. We repeat again, that cases of recurrent oinomania require long seclusion from the world; and, therefore, when it is established by the history of the patient, that he labours under the disease, a deaf ear ought to be turned by relatives and authorities to all his remonstrances and prayers for liberation, till by minute observation it shall have been ascertained that his paroxysms cease to return. It may be thought that this is not any easy matter to ascertain, and assuredly it requires an experienced and intelligent man to decide the question. To one accustomed to such cases, it is not so difficult. So long as after certain intervals the patient becomes restless, listless, unwilling to engage in any occupation, anxious to be discharged, irritable, &c., so long he is not free from his malady. The more confident he is of his recovery, the less he is to be trusted. We confidently appeal to our brethren accustomed to such cases, whether their experience does not confirm our statement.

The third variety of Oinomania is the *chronic*, which is by far the most common and the most difficult to cure. The patient is incessantly under the most overwhelming impulse to swallow stimulants. To gratify his insane propensity, he sacrifices comfort, decency, and reputation, withstands the claims of affection, consigning his family to misery and disgrace, and denies himself the common necessities of life. As occurs in the recurrent form of the disease, he derives no pleasure from his potations; he does not relish society, but, on the contrary, shuns it; he is quite conscious of his state, and bitterly laments it; and all the gratification which he enjoys from yielding to his insane impulse, is the temporary relief from the dreadful misery, bodily and mental, which he endures. Awaking in

the morning from stupor or a restless sleep, morose, fretful, disgusted with himself and all around him, incapable of any exertion, physical or intellectual, shuddering at the past, wretched in the present, and despairing as to the future, he feels that life is no longer to him a boon, and he is strongly tempted to commit suicide. Nor is this always resisted, as many melancholy facts in our own experience amply testify. In this miserable state of mind, weak and tremulous in his body, his insane impulse is strong and overpowering. Stimulants he must have at every hazard; and they are procured at whatever sacrifice. With every fresh dose, he recovers more and more vigour of mind and body, till he feels comparatively comfortable, and will then exert himself in a way which would astonish any one who had seen him a few hours before. Hope springs up in his breast. He will yet be a man; he will overcome his propensity; he will once more assume his proper position in society. The truth is, that the insane impulse is quieted for a time. In a few hours, however, it returns, and the patient drinks till complete intoxication is produced. Then again succeed the suffering, mental and bodily, the comparative tranquillity and restoration of power, the state of excitement followed by insensibility, and unless absolutely secluded, the patient continues the same course till he becomes maniacal or imbecile, or dies. This is that fearful state described by the late Charles Lamb, in which reason revisits the mind only during the period of incipient intoxication, his ordinary state being that of melancholy imbecility.

The causes of this are injuries of the head, diseases of the heart, hereditary predisposition, and intemperance. To these may be added, the causes of the other varieties of the disease, which, unless properly treated, will end in this.

In this variety we have the same uncontrollable impulse as in the others. So convinced are the patients themselves of this, that many instances are on record of the unfortunate individuals so affected having voluntarily sought the advantages of an asylum, to protect themselves against their malady. These have been generally cases of the recurrent variety, and of men of stronger minds than usual, though, with all their power, incapable of resisting the malady. Instances, however, are found of those suffering from the chronic form pursuing the same course. In this, as in the recurrent variety, nothing can be done without seclusion; and surely what some patients have themselves felt to be their only refuge against their calamity, it cannot be unjust or harsh to force on others whose minds are more impaired. The chronic form requires long treatment. The whole man must be renewed, before he can with safety be discharged; and this will require a period of at least two years. On the ground of its being necessary for the treatment, seclusion is justified; but on other grounds, it is necessary. The patient is dangerous in most cases to himself and others. He frequently entertains delusions respecting individuals, which are not to be trifled with. He becomes jealous of his wife; fancies that his children are in league against him; and believes that conspiracies are formed among his friends or strangers to injure him. In his low state he is suicidal; in the stage following, there may be comparative tranquillity; and before he is thoroughly intoxicated, he is highly excitable, and often destructive. On the three grounds, then, of treatment, protection to the patient, and safety to the community, such patients ought to be secluded.

To apply all this to the case of Mr. Vicars. We have no means of judging to which variety his malady belonged at first; and this is of less consequence, as even after he had been in an asylum, it assumed another form. That he was insane, there can be no doubt; and that he at first laboured under one of the varieties of oinomania, there can be as little. That he required protection against himself and his acts, we fully believe, though the evidence of this was not sufficient to satisfy the jury. Men in his state are prone to dissipate their property, and easily become the prey of the designing.

We have now stated our views, however imperfectly, respecting the abridgment of the personal liberty of the insane. We feel that the subject is not yet exhausted, and hope at some future time, to resume the subject when an opportunity is afforded. Meanwhile, the disposal of property claims our notice; and as some trials have recently occurred which demand examination, we shall make it the subject of a future article.

#### ART. XI.

*Letter from the Council of the Royal College of Surgeons of England to Sir George Grey, Bart., &c., Secretary of State for the Home Department, &c. Dated 23d of April, 1850.*

THE incessant struggles of the Medical Profession for organization have been already noticed by us.—(See vol. III, p. 31.) They have again forced the Council of the College of Surgeons of England into action; and, we may add, into some trifling concessions, which are stated in this Letter to Sir George Grey. We shall present our readers with a critical analysis of its contents; but we ought to add that it is not easy of comprehension, being, as a literary composition, by no means well written, and having its legislative portions curiously mixed up with controversial remarks.

The first proposition of the College is, that all Members of twenty years standing may be “nominated to the Fellowship without examination” under certain conditions: a second, “that Fellows of the College practising Midwifery shall not be ineligible to the Council:” a third, “that no Fellow shall be eligible for a seat in the council, unless he shall have been a Member of the College for twenty years, or a Fellow for fourteen years;” and a fourth, “that electors, whose residence at a distance, or whose engagements, prevent their attendance, may vote by transmitting papers to the College.”

The Council recommend their first proposition to the notice of Sir George Grey in the following terms, in quoting which we may observe that the paragraph is an example of the somewhat confused style in which the document is drawn up. After referring to the spirit of hostility and opposition exhibited by “some of the general practitioners” who were omitted from the list of Honorary Fellows, the document proceeds thus:

“The Council, however, notwithstanding the injustice of the charges with which they have been assailed, and the delay which will be occasioned in the development of their plan, are unwilling that any sense of wrong should continue to alienate any portion of their members, and not less that it should prove a bar or impediment to a comprehensive legislative measure, of which the urgent need is universally acknowledged, for the regulation of the whole profession of physic and surgery,



and for the settlement of questions bearing on the interests of the profession, and of the public, of far more importance than the titles and designations which are to be borne by practitioners in surgery. And in proof of what they advance, they need only advert to the provisions contemplated in any enactment of the Legislature for the superintendence of the Profession, by a Supreme Board or Council, for ensuring thereby a high and uniform qualification throughout the empire, for regulating the reciprocity of practice in the three kingdoms, for the authorised registration and public notification of all qualified practitioners, and for securing the public against the pretensions and malpractices of uneducated and dishonest persons. With the sincere desire, then, of healing all differences, the Council have had the honour of submitting to you their resolution of the 4th of February last, with the earnest prayer that they may be forthwith empowered to nominate to the Fellowship, without examination, and under the conditions only of the prescribed certificate, those who, having been members of the College at the date of the charter of her present Majesty, now are, or when they shall be, Members of twenty years standing."

The prescribed form of certificate is subjoined. This being duly signed by six Fellows, and ten guineas being paid, the admission of the candidate is voted or balloted for by the Council.

"We, the undersigned Fellows of the Royal College of England, do from our personal knowledge of the high moral character and professional attainments of A. B., of C., declare that in our opinion he is deserving of the honour of the Fellowship, and that he does not openly trade in medicines. We therefore recommend the said A. B. to the Council, to be admitted a Fellow of the college."

Members in the Army or Navy need only have the signature of the heads of the medical departments of the respective services; those in the service of the East India Company, of the secretary of its military department; and those in the colonies, that of "the Governor (!) of the colony certified by the Colonial Secretary." It does not appear, however, that the election will necessarily result on the presentation of the certificate; the Council, composed exclusively, be it remembered, of two dozen surgeons resident "within five miles, by highway or road, from the General Post-Office in St. Martin's-le-Grand," may reject the candidate by *secret* voting, and need give no reason for the rejection. We have carefully read over the paragraph in which the Council set forth the motives that have determined them to propose this change in the charter; and although we frankly acknowledge that we cannot get at its full meaning, two things are clear,—firstly, that certain nominations to the Fellowship under the new charter have excited a sense of wrong, and a spirit of hostility and opposition in certain members of the college, who were not then nominated;—and, secondly, that this proposition is brought forward as an olive-branch, is propounded "with the sincere desire of healing all differences." The result, we need hardly say, has been to cause new differences, without in any way "healing" the old. The body of reformers represented by the National Institute, repudiate the thing altogether. In their address to the College, they emphatically state it is not what they want, or asked for, nor anything like it:

"The Council of the National Institute, in furtherance of repeated resolutions adopted by large and influential societies and general meetings, have upon every occasion maintained that they considered it *essential to a satisfactory settlement of the Medical Reform question, that the general practitioners should have the unrestricted right to regulate the education and examination of the candidates for admission*

into their body, in every branch of medical science, subject only, in common with other corporate and educational bodies, to the general supervision of a Controlling Council," &c.

The same National Institute emphatically assert the same principle in their able memorial to Sir George Grey, with, however, the addition of others. We quote two clauses from this memorial :

"That the general practitioners of this country respectfully, but firmly, *assert their right to direct the education of their own class*, and to institute examinations of the competency of individuals intended for general practice, *and to manage and control their own affairs*; and that they view with the greatest possible alarm every proposal to deprive them of that right, and thereby of the power of maintaining and elevating their standard of education and qualifications in accordance with the present rapid advancement of every other department of science and art."

So much for the educational status of the profession in connexion with open corporations; the next clause touches on municipal interests and government :

"That a vast majority of the profession—the general practitioners—from the *absence of any bond of union, or legally authorised executive to represent them*, have, for many years past, been placed in a position of the greatest embarrassment and humiliation; and that owing to this radical defect, and the *want of a sufficient controlling power over the executives* of the existing medical corporations, they have hitherto been unable to obtain a recognition of their rights and privileges, or protection from illegal and unprofessional practice,—*privileges which they deem essential to the welfare and dignity of the profession.*"

It must be obvious that the "healing" concessions of the College are in no respect calculated to satisfy men holding medico-political views of this kind. The Council retain in their own hands the power of secretly rejecting any Member of the College from the Fellowship, and consequently from the body by which they can in any degree be controlled or made responsible; and it would be expecting too much from poor human nature to anticipate, that with Curtius-like self-sacrifice, they would admit any of those who had made themselves obnoxious to the powers that be.

Nor are these "healing" propositions more satisfactory to those grumbling "general practitioners," to appease and conciliate whose sense of wrong they are expressly made. Thus the Lancashire and Cheshire Medical Association, presided over by Sir Arnold Knight, M.D., at a special meeting held on March 28th last, resolves :

"That this meeting is of opinion that it would be a great injustice to demand a fee of ten guineas from those gentlemen whom the Council of the College of Surgeons propose to raise to the fellowship, in virtue of their twenty years' standing as members, inasmuch as no such fee was demanded from those upon whom the distinction was originally conferred."

The same numerous and influential Association (comprising every class of practitioners) declares with equal explicitness against the other proposed changes in the charter, and the meeting also resolved :

"That the modification of the charter of the College of Surgeons recently proposed by the Council is **ALTOGETHER INSUFFICIENT to meet the views of the members of that body, and the requirements of the profession generally.**"

A Committee, appointed at a public meeting of the profession in Manchester (where it is sufficiently numerous to have a College of its own), to watch the progress of the medical reform question—address a memorial

to Sir George Grey, in which they thus express their sentiments as to the proposed new admission of fellows :

"A very small minority were selected from amongst the members arbitrarily, and without any fixed principle; and in the new grade thus constituted, all the powers and influence of the College were made to inhere. The Committee do not attach any value to the fellowship as now proposed to be conceded to members of twenty years' standing, on payment of a fee of ten guineas, seeing that the members elected to that distinction under the provisions of the late charter were called upon to make no such payment. Any admission to the fellowship upon terms less favorable, must continue the distinction which the Committee hold to be both unjust and invidious."

This memorial puts the Fellowship question in another point of view. We have seen influential associations considering it, firstly, in its bearings upon the interest, government, and educational position of the *profession*; secondly, abstractedly as a question of justice and injustice; but here we have a glimpse of its working upon the interests and position of *individuals*. A certain number of *members* are occupying a locality and competing with each other for public esteem on equal terms, at least as to college titles. They have all similar collegiate rights and privileges; they all bear the same title; they have all, therefore, in the eye of the public, undergone the same examination as to their qualifications. Suddenly, the College confers on some of these competitors in the race a title which in the eye of the public indicates a *superiority*—perhaps of qualification—perhaps of grade—certainly, in *something professional*; and so the fortunate possessor gains *pro tanto* an advantage over his less fortunate brethren. But the title thus given does not indicate superiority; it is asserted to have been conferred "arbitrarily and without any fixed principle." Hence a feeling of angry resentment against the authors of what must be considered as a wrong. While, however, the title is thus conferred, measures are taken that it shall really represent a higher education, and consequently superior qualifications for practice, by granting it after special examination; so that the same title may indicate an imaginary or a real superiority. It might be made an interesting point in casuistry to determine how far the holders of the honorary title are justified in using it; but we cannot feel surprised that the "healing" proposition of the College should wound the *amour propre* of the "Fellows by examination," for they, at least, have been put to some expense and much study—that "weariness of the flesh,"—to obtain it; and may justly urge, that to live as a member for twenty years is an easier and less creditable road to the dignity than the one they have taken as the "examined Fellows." These Fellows have had a meeting, and drawn up a memorial; they shall state *their* opinion as to the "healing" proposition in their own words.

"Your memorialists most respectfully submit that the proposed alteration of the Charter of 1843 will be highly injurious to the profession and unjust to themselves; that it will destroy all confidence in the statements of the Council, and in the stability of any professional institution whatever; that it will degrade the fellowship, and render it valueless in the public estimation, because a large number of members would then be admitted to that honour without any examination whatever, in spite of the notorious fact, that several of those who have been examined have been rejected. Lastly, that it will be a breach of faith towards your memorialists, who have, in reliance upon the public official documents of the College, subjected themselves to this examination, if other members are admitted to the fellowship on more

facile terms, because its value as a mark of surgical proficiency will be thereby destroyed."

The authors of this Memorial seem to have gone up to the examination for the Fellowship, as a commercial speculation. They thought it to be valuable in public estimation, because it indicated a superiority in surgical proficiency. Its possession, they argued, would and ought to give them a good start ahead of their competitors; they therefore sought it; and now, why should the "rejected" be admitted to hold their valuable title in common with themselves? There is much selfishness, but there is also some justice, in the argument.

Let us turn, however, to the "Letter" of the College, and learn why they have established this additional method of qualification, and would annually raise to the most honorable rank a number of professional men not *duly*, because not *fully*, educated in their profession;—men, as they make it appear, too indolent, or too weak in intellect, or too poor in spirit, or too imperfect in their primary education, to seek for and obtain that diploma which is the *true* "mark of surgical proficiency." The arguments the Council adduce in support of their views shall be given in their own words; we need hardly say they are altogether theoretical:

"The Council have no hesitation in adding, as the result of their long experience, that in the qualifications of their members, as destined for general practice, they dare not require any higher standard of surgical education than is compatible, on the one hand, with the needs and safety of the public, and on the other, with the length and consequent expense of a course of study which may be proportionate to the scale of remuneration of the majority of general practitioners; and they believe that the infallible consequence of raising unduly the standard of education would be, practically, the evasion of any qualification, and the surrender of the poorer classes, under any surgical emergency, into the hands of the vendors of drugs and other uneducated persons."

The class of "Fellows" was determined upon by way of carrying out "a plan which they had long contemplated and cherished, of improving the education and qualification of surgeons." They were to constitute the class of "electors of the Council," and to become entitled to that distinction by proof of a liberal general education, of a longer course of study, and of a larger amount of professional knowledge, practical and scientific, than the College could venture to require of those who had hitherto sought the diploma as members; but the Council liberally permit the "Fellows" to engage in general practice:

"But the Council may be permitted to affirm, that the possession of the name and privilege of a Fellow is limited by no other conditions or restrictions than those of a moral character, of high education, and of superior professional attainments, and that he is in no respect prohibited from general practice, or from being in any or every sense of the word a general practitioner."

We might be induced to adopt the "experience" of the Council as to the effect of a high standard of education in diminishing the number of general practitioners—the attendants of the poor classes—if we did not find that the M.D.'s of the University of London, and their own Fellows, are in their due proportion amongst the medical practitioners in attendance on the poor. It is the *res angusta domi*, or the want of connexions or friends, and not an imperfect education, which drives the man of talent and energy to seek practice where he can get it; and we believe

the Council will soon hear of a "Fellow" keeping an open shop. But, in fact, their whole chain of argument is a series of inconsistencies. If the "Fellow" become ordinarily a "general practitioner," as they hint he will, they have done that indirectly which they emphatically declare they "DARE NOT" do, and which would "cause the general practitioner to disappear before the druggist and chemist!" If the "Fellow" is not to displace the "member" in his attendance on the multitude, why require from him a knowledge of the principles of *medicine* and *midwifery*? If the "member" be expressly educated to be the servant of the poor, why is he *not* carefully examined as to his skill in midwifery, and especially *operative* midwifery? These facts are inexplicably inconsistent with the intentions of the College, so repeatedly, nay, so solemnly avowed; namely, that the "member" has an inferior education required from him, for the sole purpose of adapting him to the wants of the multitude.

Turning from this part of the educational question, we think our readers will agree with us in a low estimate of the educational tact displayed by the council, in requiring the candidates for the Fellowship to be examined, at the mature age of twenty-five, in mathematics and the Greek, Latin, and French languages. Common sense and all experience are equally opposed to such a plan. If the Council had instituted a matriculation examination of candidates *for admission to the study* of their profession, whether they intended ultimately to be "Members" or "Fellows," and required classical and mathematical knowledge in *them*, they would have made a most beneficial arrangement, and done an immense service to the profession; inasmuch as youths without the mental powers and educational training requisite to make good practitioners, might have been thus arrested at the first step of an imprudent and disastrous course of life. What good end can be attained by requiring a man of twenty-five to "rub up" his mathematics, and *Greek, Latin, and French* languages, we don't know; but the council are all admirable Crichtons indeed, if *they* could successfully confront Messrs. Gabriel Stokes, Smith, and Brasseur after a "quarter's schooling;" yet we believe they are all excellent "Fellows" and proficient surgeons, and might well have omitted this bit of pedantry.

We have now only to notice the money-payment required; and in doing this, we will revert to our previous article on 'Medical Organization.' (Vol. III, p. 31.) If the reader will turn to that article, he will easily comprehend why the Council of the College of Surgeons have failed in their efforts to conciliate their opponents. They have not been practical, for they have not consulted "the wants and wishes" of their fellow-practitioners; they have lost sight of the great principle, that "the profession should be organized conformably with its natural tendencies." Union and communion are earnestly longed for; law and government and order are sought; protection for the material and technical interests of the profession demanded. There is no common centre of feeling, no head to devise, no hand to execute; the practitioner enrolls himself in "medical associations" and "medical societies," subscribes his means, labours diligently to accomplish his ends, and turns an imploring eye to the corporations for assistance, but all in vain. He is wearied with the chaos around him, and is justly angry that those bodies with which he is

connected nominally, and which officially represent the profession, its wants and its wishes, take no note of his request for a head and a home. But perhaps there is nothing in which the corporations have manifested so great a disregard of the interests of the profession, as in pecuniary matters. Taxation is at least no sentimental grievance, and cash payments are realities. We might have expected some regard to be shown for the practitioner's pocket, if his feelings were disregarded. The complaint of the members of the College is, that all being on a level, the Council, without requiring a money-payment, arbitrarily raised certain individuals of their body to a superior rank; they request the Council to do them justice in the matter, and the Council offer them the same title on the payment of ten guineas! This, of course, is not equal dealing; and as it exposes the Council to the charge of injustice, the demand should at least have been accompanied with some conciliatory statement of the motives which had led the council to inflict this heavy tax. Yet there is not one explanatory word; all that the members are told is, that the Council has "a sincere desire to heal all differences," and remove a "sense of wrong."

In the midst of all this change and difference of opinion, the Secretary of State is bewildered, and although only waiting to grant what the profession desires, is helpless, because he cannot discover what the desire is. It is obvious that the great practical point in the present stage of the reform movement is, to devise the means whereby the wishes and feelings of the profession may be represented. Voluntary associations have failed; the corporations, as constituted, have failed; it therefore remains for the Government to take such steps as have been, and are, found available to this end, in political changes generally. We see no better means than the local registration and organization of the profession throughout the provinces, for which a short Act would alone be necessary. Two assessors should be associated, a barrister and a physician or surgeon, whose duty it should be to investigate all the legal or quasi-legal qualifications of English practitioners. They should divide the country into convenient districts; they should examine diplomas, &c., and make out authentic lists of the holders for each district. Power should be given them to arrange for the election (by the medical profession resident therein) of a *registrar* for each district, and this registrar should have authority to call the profession of his district together for deliberation, whenever required by the Secretary of State, or on the requisition of a certain number of members. Ample notice should be given of the subject or subjects to be decided upon, and the decision of the meeting of the associated members of the profession (proxies being allowed) should be considered to be the decision of the profession of the district. Questions touching particular colleges should be decided by the members or quasi-members of those colleges; so that the Licentiates of the Apothecaries' Company might decide as to all points touching their relations to it; the members of the College of Surgeons on all points relating to their connexion with the College; Scottish graduates, &c., on matters specially relating to them.

We know of no other means than these, whereby the interests at stake in the proposed changes in medical organization can be separated from the chaotic mass in which they are now involved, so that the general interests of the profession may be distinguished from those of individuals, and each

reconciled with the other. Hitherto, every step taken in medical organization has only added to the confusion and difficulties already existing, by multiplying the interests to be reconciled; as an illustration, we may mention the three new classes of practitioners recently constituted, namely the graduates of the University of London, and the "examined" fellows and "honorary" fellows of the College of Surgeons; if a College of General Practitioners be constituted, at some future time, it will make a fourth.

Since the foregoing was written, the Editor of the 'Lancet' has put forth a scheme for taking the votes of Medical Practitioners throughout the country, for the information of Sir George Grey. Giving him full credit for the best intentions in the matter, we venture to prophesy that not one third of those qualified to vote will take this method of expressing their opinions.

#### ART. XII.

*On Diseases of Menstruation and Ovarian Inflammation, in connexion with Sterility, Pelvic Tumours, and Affections of the Womb.* By EDWARD JOHN TILT, M.D., Physician to the Farringdon General Dispensary, and to the Paddington Free Dispensary for the Diseases of Women and Children.—London, 1850. 12mo., pp. 250.

It must be acknowledged that the pathology of the female generative organs has recently received a full share of attention. Lately we noticed Dr. H. Bennet's work on 'Inflammation of the Uterus;' and we have now before us a treatise almost entirely concerned with inflammation of the Ovaries. It is not a little embarrassing to find that these authors,—in enunciating their views of the pathology of very dissimilar parts,—indeed, as we may say, of the two poles of the internal sexual organs, viz. the cervix uteri and the ovaries,—should each lay claim to the disorders of menstruation. Inflammation, as the key-stone of the pathology of the female generative apparatus, is the principal point on which they are agreed. With Dr. Bennet, in the majority of cases, whenever the function of menstruation is deeply disturbed, whether in the single or married, inflammation or ulceration of the neck of the uterus is the cause. Dr. Tilt, however, pleads for the ovaries, as furnishing, not their exclusive, but their most common cause. In this scientific encounter, we cannot but remark that Dr. Bennet has the advantage, of which he has most fully availed himself, of a part which is easily accessible to two senses, and whose minute changes can be felt, seen, and portrayed. Dr. Tilt deals with organs which are only rarely and with difficulty reached with the finger, and are concealed from the sight. The former set of doctrines is alluring to the superficial observer, and has a harrowing interest for women; the latter has no equivalent attraction for the one, and would but languidly move the emotions of the other. A large ostentatious book is the massive produce of Dr. Bennet's pursuits; the present small volume is, we are thankful to say, the offering of Dr. Tilt.

But while the senses of touch and sight may often discover something on or in the cervix uteri to justify treatment, and finish the inquiry of the majority of practitioners, a conviction has grown upon those who have

long since broken ground on this subject, and by a daily experience tested the French practice, and the larger pretensions of Dr. Bennet, that the importance of inflammatory diseases of the cervix, and the so-called ulcerations of this part, has been exaggerated; and that many of its supposed phenomena, including even the ulcerations themselves, are really often due to affections of the ovary and body of the uterus.

A presumptive argument in favour of the importance and extent of the influence of the ovary among the sexual organs, may be fairly drawn from its anatomical position, its high organization, and functional endowments. It bears the same relation to the oviducts, uterus, and external organs of generation, that the kidney does to the ureter, bladder, and urethra. It holds these parts as tributaries, subservient to the acts of reproduction and menstruation, which have their origin in it. It would be strange, indeed, if, when the female fails in having issue, or is the subject of menstrual derangement, the ovaria should not in many instances be implicated. It is just this inquiry, which Dr. Tilt undertakes:—How far are the disorders of menstruation dependent on ovarian disease; and what is the particular lesion which causes them?

His general answer to these questions we take to be, that ovarian disease is a very common, though not an exclusive, cause of menstrual disorders; and that subacute inflammation of the ovaries is the most frequent lesion.

In his Introduction, Dr. Tilt propounds several questions, the answers to which are supposed to prepare and clear the ground upon which he works in the body of his treatise.—Why is medicine so uncertain? What are the principal diseases of menstruation? What is menstruation? What are the organs of menstruation? What is inflammation?—These are the five consecutive interrogatories; and although they look vast enough to supply the materials for a book, instead of an introduction, they are very concisely answered, and still afford Dr. Tilt the relief of a little fine writing about the use of words. We need hardly allude to the first and last questions, further than to mention that the author considers, and very truly, that one of the principal causes of the uncertainty of medicine lies in a want of precision in medical language.

The different diseases of menstruation are passed in review, in order to show that as substantive complaints they are liable to mislead, by representing divers disorders under the same name.

“What does *Amenorrhœa* imply?” says Dr. Tilt. The following is his answer:

- “Absence of organs of ovulation, their destruction, their chlorotic arrest of development;
- Sub-acute or acute ovaritis;
- Or it may represent the inflammation, or the obliteration, of the Fallopian tubes;
- Undersized womb;
- Inflammation of the womb;
- Morbid stricture, or obliteration of the neck of the womb;
- Ulceration of the neck of the womb (*Dr. H. Bennet*);
- Its induration (*J. P. Frank*);
- Retroversion of the womb (*Dr. Rigby*);
- Or the organs of reproduction may be perfect; but, under the influences of various acute and chronic diseases, the menstrual flow may be impeded or suppressed.



"This word Amenorrhœa, which answers to so many conditions, can, then, be no longer admitted as a substantive term. It means so much that it means nothing." (*Introduction*, p. xix.)

The same kind of collective answer is given to similar questions about dysmenorrhœa, menorrhagia, leucorrhœa, &c.

We quite concur with Dr. Tilt in thinking that these terms are sufficiently confusing; and, no doubt, as the pathology of the sexual system improves, their acceptance will be more and more limited. But we must say that our author's method of crowding synonymes, which are not really synonymes, and smothering each disorder in its turn, is hardly consistent with what is generally understood respecting them. Like other expert advocates of particular views, Dr. Tilt knows how to involve all existing opinions in a cloud, from beneath which he may recover his own, looking all the freer and better for the surreptitious separation. But he knows perfectly well, that when used as substantives these terms are preceded by a qualifying adjective, which relieves them of the wide and loose signification that would otherwise attach to them. Thus we speak of *plethoric* amenorrhœa, *organic* amenorrhœa, *symptomatic* amenorrhœa; also of *mechanical* dysmenorrhœa, *membranous* dysmenorrhœa, *neuralgic* dysmenorrhœa, &c. &c. We quite admit the deficiency of the nomenclature; but do the modern views of the physiology and pathology of the sexual organs warrant its summary rejection? Would the practitioner or patient be benefited by regarding the majority of these complaints as the result of inflammatory ulceration of the cervix, or of subacute ovaritis? With the exception of leucorrhœa, which must for the future have a precise meaning, and be regarded almost entirely as symptomatic of well-recognised organic disease, we do not think that the terms in common use can be dispensed with, or their application be in any material degree contracted. These recent attempts to affix a seat and particular lesion to the disorders whose common designations are to be supplanted, present so much contrariety among themselves, that they suggest the idea of being as much the strivings of youthful talent to bring itself prematurely out, as the careful deductions of watchful experience. A man who is old, calm, and wise enough not to be readily caught by novelty, will wait awhile before he gives up the opinions which have been the slow growth of many years; and will not be disposed to surrender them at the first call. He feels that there is something intrusive and unsound in the active rivalry of these claims on his attention, and that one successful hit which has for a time taken with the public, is sufficient to start into life a tribe of combatants more suited to fight for a glittering prize, than possessed of just pretensions to win one. We must not be supposed to apply this remark to Dr. Tilt, whose opinions, as we shall see, in spite of their being at the outset over-exacting, are entitled to a favorable consideration.

In answer to the third and fourth questions, which refer to the nature of menstruation and the organs of menstruation, Dr. Tilt adopts—with a little coyness, however—the modern theories. He regards the menses as a "sero-sanguinolent secretion propelled by an ovarian influence from all or different parts of the generative intestine, and principally from the womb." We must notice the two points included in this definition rather more in detail,—1, the ovarian influence; 2, the nature and source of the discharge.

1. *The ovarian influence.*—The general proposition that the ovaries

influence menstruation is universally admitted. Even their destruction by disease will frequently, though by no means invariably, stop menstruation. The only question which is still vexed is, whether oviposition, as the initial act of generation, is indicated by menstruation. On this point, Dr. Tilt, in the little he says upon it, quotes some important facts which go to disprove it. "We defy," he says, "the staunchest supporters of the ovular theory to explain why the ovula floating in the fluid of fully-developed Graafian follicles, in girls of from two to four years of age, observed by Carus, did not in them produce a menstrual flow; and why in Mrs. M——, who had begun to menstruate twelve hours before her execution, no appearance of recent rupture of a vesicle, or of the discharge of an ovum, could be found in the ovary." The careful examination of the ovaries in this case, made by a man of Mr. Paget's acknowledged skill, is far too valuable not to be given in full:

"Mrs. M—— had begun to menstruate about twelve hours before her execution. The ovaries were of moderate size, and presented numerous marks of cicatrices, with some small bands and threads of false membranes on their surfaces. In the right ovary, three Graafian vesicles projected slightly on the surface and looked healthy, containing clear serous fluid. A fourth was of very large size, about 3" in diameter, and prominent. In the left ovary, one Graafian vesicle was fully developed and prominent. We looked for ova in the contents of all these, but in vain. The surface of the ovaries was generally rather more than usually vascular, but there was no peculiarly vascular spot, nor any appearance of the recent rupture of a vesicle, or the discharge of an ovum. In the right ovary, near the surface, was a small cyst or cavity, containing what looked like a decolorized clot, and bounded by a thin layer of bright yellow-ochre substance—an excellent example of a fibro-carpus-luteum of one or more months' date, certainly not more recent. The veins at the lower part of the ovary were large and turgid. The ovarian ends of both tubes were completely closed. Tracing the tubes from the uterus, they proceeded for about two inches naturally, and I think both pervious. They then began to dilate and to grow thinner, and thus, gradually dilating, they ended in pyriform enlargements, completely closed in, presenting no trace of orifice or of fimbriæ, and not attached to the ovaries, except by some intervening tissue. Each of the enlarged saccular ends of the tubes measured about 1" by  $\frac{1}{4}$ " and its walls were thin, and lined with mucous membrane, which had a ciliary epithelium. They were filled with thick, grumous-looking, and ropy claret-coloured blood, with well-formed blood-corpuscles, all like those of recent blood, and including a very large proportion of white ones, some of which were very large, and contained numerous granules. This blood could be pressed along the tubes to the uterus; but the tubes appeared to have contained none, except at their dilated ends. The blood did not coagulate, and no serum separated from it. The uterus was large, especially at its cervix, which appeared swollen. The os uteri was circular. The walls of the uterus were thick and soft, and their out-surface, about the fundus, had a partially livid hue. The cavity of the uterus was nearly full of black fluid blood, containing well-formed corpuscles, with an ordinary proportion of white ones. In this blood was a small round mass of soft white flocculent substance, about 1" in diameter, like decidua. It appeared to be formed entirely of cells, like lymph, in various degrees elongated and attenuated, as in the development of filaments of cellular tissues. They are just like those of the deeper layers of granulations, only smaller. The mucous membrane of the uterus appeared pale, but healthy. False membranes were attached to many parts of its fundus.

"The closure of these Fallopian tubes accounts for the woman being barren, though married, and having, it was believed, had frequent intercourse with others besides her husband. (I afterwards learned that she was a woman of extreme sexual passion.)

"It would seem probable that in menstruation blood may sometimes flow from the vessels of the tubes as well as from those of the uterus. Certainly the blood in these tubes did not pass into them from the uterus; for 1stly, there was none in them, except at their dilated ends; 2dly, what they contained differed from that in the uterus, in being thick, grumous, and claret-coloured, while that in the uterus was like common venous blood, and contained a larger proportion of white corpuscles." (pp. 190-3.)

The readers of some former obstetric articles in this Review, will see that this examination accords with the doctrines we have always laid down on this subject. We have yet to learn that an act of oviposition takes place in the human female at every menstrual epoch; and we have seen the "fibro-corpus-luteum" described by Mr. Paget (which is not for a moment to be confounded with the proper corpus-luteum of pregnancy), recently formed in the ovary of a young woman who died of phthisis, and who had not menstruated for some months. But in spite of the facts he has quoted, and his strong comment on them, Dr. Tilt—with a somewhat eccentric simplicity—has adopted the ovular theory. "A theory," he says, "is an intellectual staircase; and as some kind of staircase is necessary to obtain a knowledge of the interior of a building, to make use of its appurtenances, so must we have some kind of theory in order to catalogue and turn to account the innumerable facts of modern science; and as, moreover, we do not totally discard the staircase because a few of its steps do creak, we adopt the ovular theory of menstruation as the best, although it does certainly creak in some points." What! a creaky theory of generation! It is a true physiological alarm. It is many years since, that Drelincourt collected 262 recorded theories of generation, all of which creaked, and his own made the 263d; so that Dr. Tilt, if he is content with a creaky theory, has enough to choose from. But having adopted the theory, Dr. Tilt speaks of ovulation, or the periodical maturation and dehiscence of ova, with as much glibness as M. Pouchet would, and as though he had not previously given a reason for the uncertainty of medicine.

2. *The menstrual discharge and its source.*—The opinion which Dr. Tilt supports of the menstrual discharge being a secretion, and not blood mixed in its course with mucus from the cervix and vagina, which alters its properties and chemical characters, is certainly against recent facts. When Dr. Tilt shall have had the opportunity of seeing and squeezing the uterus of a female who has died menstruating, we feel sure he will agree with us that it is from open veins that the menstrual blood flows; and further, that he will abandon not only this idea of its being secreted, but also the now almost obsolete notion of its possessing noxious qualities. But the extent of surface which Dr. Tilt considers may furnish the flux is not a little surprising. He thinks that the whole of the generative canal, or intestine as he calls it, down to the vagina, may furnish the discharge; and he further says, that in most of the cases where the bodies of women who have died menstruating have been opened, blood has been found in the oviducts. This is quite contrary to our own experience; for in some cases which we have observed, the tubes have been empty of blood, and the seat of the discharge was strictly limited to the cavity of the body of the uterus. Of course the vagina, like the mucous membrane of the stomach, or bronchial tubes, *may* exhale a periodical vicarious hemorrhage; but it certainly does not do so normally, of which

every practical obstetrician must have had proofs. The blood in the tubes in Mrs. M.'s case is not at all conclusive on this point. The closure of their distal ends, and the distension of the canal of the ducts, is in itself sufficient to cast great doubt upon their contents being taken as representing a physiological act. Besides this, we must take into account the mode of death, which may have had some effect on the fluid.—But we pass from these preliminary considerations to the body of the treatise.

In a *Prolegomenon* Dr. Tilt notices the confused state of our knowledge of ovaritis, and the probable reasons which have retarded its accurate study. The principal of these are the smallness of the ovaria, and the difficulty of manually examining them; as also the repugnance of the patient to submit to such an examination, and the reluctance of the practitioner to propose it, when the pains and colics are supposed to be but menstrual. In giving a short sketch of "ovarian bibliography" he has mixed up pelvic cellular inflammation and abscess with ovaritis, without any apparent reason. But the main practical point which is discussed in this prolegomenon, is the method of exploring the ovary. This is treated under the heads of Abdominal, Vaginal, and Rectal Exploration, and Exploration by double touch.

*Abdominal exploration* alone affords but a limited amount of information; but the presence of a swelling in one or other ovarian regions, large enough to surmount the brim of the pelvis, may be felt; and the heat, sensibility, and prominent physical characters of the tumour may be ascertained. Dr. Tilt refers to the advantage and importance of examining for any ovarian swelling shortly after parturition, when the laxity of the abdominal walls allows a free dip into the abdomen and pelvis, and a ready means of exploring the tumour.

*Vaginal exploration.*—The falling of a large ovary into the recto-vaginal pouch, and therefore on the posterior roof of the vagina, permits the organs to be felt by a vaginal examination. If the swelling is large enough to have been raised into the abdomen, external pressure from above towards the pelvis, and one or two fingers within the vagina from below, allow of the swelling being held between the hands; and by their acting in concert, much exact information as to its physical character, and its relation to the surrounding parts, may be obtained. M. Recamier introduces the finger into the vagina, beneath the raised leg. Dr. Simpson considers that the uterus ought to be anteverted, and turned somewhat to the opposite side by the uterine sound, in order to stretch the broad ligament of the side under examination,—a practice which it is difficult to follow, and which is likely (as we ourselves know) to be followed by much local irritation. Fluctuation may be detected in an ovarian abscess by external pressure from above, towards the fingers within the vagina below; or if the cyst be low down, a portion of its lower segment may be embraced between two fingers within the vagina, to which the sense of fluctuation may be communicated.

*Exploration per rectum.*—Dr. Tilt agrees with those authors who think that the ovary may be reached in its normal position by the introduction of the finger into the rectum, the female being placed in the usual obstetric position. That this may be accomplished, we know from experience, provided the patient have a shallow pelvis, with thin yielding structures, and she lie passive and quiet under examination. But it must not be supposed

that, without these advantages, the ovaria can be always or even often reached, although Dr. Tilt makes no reservation of this kind. When touched in a healthy state, they are not painful; but when inflamed, their sensibility is very great, and there is a fixed severe pain in the corresponding inguinal region. By rectal examination the physical characters of an ovarian swelling can be made out with much general accuracy.

*Double touch.*—"We have given the name of 'double-touch,'" says Dr. Tilt, "to a mode of exploration wherein the two previous modes are combined, so that the index-finger being placed in the rectum, and the thumb in the vagina, it is possible to embrace between the thumb and the finger any intervening morbid growth." As a variety of this double touch which we have often practised with advantage, the author might have added the substitution of the other index-finger for the thumb, so that the swelling may be felt by the two forefingers, which allows of a deeper stretch into the vagina, and is available on this account in some cases in which the other would fail. This method of exploration has been used by P. Frank and Dr. Blundell; and we have sufficient experience of it to add our recommendation of it to the author's. Dr. Tilt illustrates the value of the double touch by some interesting cases occurring in Paris, under the care of Professor Recamier and Dr. Rayet.

Dr. Tilt considers *Ovaritis* under two forms: 1, *sub-acute ovaritis*; 2, *acute ovaritis*. The author distinguishes the sub-acute from the acute ovaritis, by limiting the inflammatory action in the first variety to distinct parts of the ovaries, as the ovarian follicles, and such small portions of the ovarian stroma as to give rise to little swelling, and no febrile action. In describing the pathological anatomy of sub-acute ovaritis, Dr. Tilt refers to the periodical congestion of the ovary during menstruation, as affording an easy means of transition into an inflammatory state. He describes the signs of a local pelvic peritonitis, which so readily covers the uterus, tubes, and ovaries with false membranes; also the inflammatory changes in the ovaria themselves, and the Graafian follicles in particular. Under this title, too, he notices the liability of the Fallopian tubes to inflammation; and the morbid appearances which have been seen to result from it,—especially the closure of the fimbriated extremity; and the distension of the canal of the tube by serous, albuminous, puriform, or bloody fluid. It appears to us that Dr. Tilt, in relating the various post-mortem appearances in the ovaria, has been rather exacting in behalf of this sub-acute disease, by ascribing an inflammatory origin to them all. Inflammation here is to be seen in all its eccentricities. In some cases it enlarges the ovary; in others it atrophies, wrinkles, and reduces it. The vesicles may become red from injection, and their cavities full, or they may be white or gray, with their cavities almost empty. Fluids of all colours, and products of the most opposite consistency, alike acknowledge the same all-powerful origin. Certain we are that seldom does a woman die without some of these phenomena being found in one or both her ovaries; and we confess ourselves somewhat sceptical about Dr. Tilt's universal key to their interpretation. In the cases which we have observed of women dying during menstruation, and in the ovary which has contained a *corpus luteum* after recent impregnation, we have noticed that several follicles in the centre of the stroma, as well as towards the surface of the ovary, have been largely distended, or broken into, and filled with blood;

and the changes which follow in the absorption of the blood, or the retroceding metamorphosis of the vesicle, would supply Dr. Tilt with the evidences of subacute ovaritis. But surely these are not morbid phenomena; they are the regular accompaniments of a physiological act; they show that, in the human female, as in the lower animals, there is oftentimes a waste of ova, and that Nature's prodigality clears away the effete remains, and forms anew. True, if we see similar appearances in other parts, we might call them inflammatory; but this is the case, as Dr. Tilt justly remarks, with the rent and closure of the opening of a burst follicle, after the dehiscence of an ovum; and it would be confounding physiological and morbid phenomena, to call a *corpus luteum*, in any stage of it, an inflammatory product, and to speak of the organ containing it as in a state of sub-acute ovaritis. The same remark applies also, in our opinion, to the opaque and wrinkled thickening of the ovarian tunics, which may occur as a slow but normal change, without the intervention of chronic inflammation. We think it of great importance to be especially careful in the pathology of the ovaria, by allowing free latitude, in reference to minute changes of the follicles, to their periodical physiological actions.

*Causes of sub-acute ovaritis.*—Menstruation, with its attendant phenomena of ovarian congestion, is a principal predisposing cause; and an attack of sub-acute ovaritis frequently dates from a menstrual period. The lymphatic constitution, the ovarian stimulus of marriage (especially when intercourse is too frequent), intemperate or lustful indulgences, and the privation of sexual congress in single women of strong passions, (whom, by the bye, Dr. Tilt, with a quaint turn of words, calls *involuntary nuns*,) are severally shown to tend to its production. The left ovary is more liable to inflammation than the right, in the proportion of seventeen to five.

Amongst the exciting causes are various mechanical injuries from falls, blows, &c., the use of instruments in difficult labour, styptic injections to restrain flooding, and stimulant injections into the unimpregnated uterine cavity. Dr. Tilt enters at some length into the inquiry, as to how far retention and suppression of the menses causes ovaritis. Having enumerated the causes of menstrual retention and suppression, he remarks that they have a "two-fold influence in the production of ovaritis; first, by the retention of what was to have been excreted, and the consequent congestion of the organs which secrete the menstrual discharge; secondly, by the arrest of the ovarian discharge, and the subsequent oppression of the system by some reflected influence of a nervous kind." The transmission of inflammation from the uterus to the ovaries, by the Fallopian tubes, is considered by Dr. Tilt a very frequent, though not a generally admitted cause. In proof of this he refers to several cases by different authors, where ovaritis is assumed to have been transmitted from an ulceration of the cervix uteri, from malignant disease of the cervix, and from catarrhal inflammation of the cervix. He says also that "we often find symptoms of ovarian engorgement disappear, from merely treating the uterine ulceration," a circumstance which has not fallen within our own experience, although the converse we have very decidedly seen, when ovarian pain and tenderness on moving the uterus from the side affected, have followed the application of the nitrate of silver to the cervix. Indeed, it cannot be said to be uncommon in practice, to find symptoms of ovarian irritation following the partial arrest of a discharge from the os and cervix

after the use of caustics, and relieved on the renewal of the discharge. More profound inflammatory lesions of the uterus, ovaria, and peritoneum, are quoted as having been known by M. Gendrin to follow the deep cauterization of the cervix, and the use of styptic injections.

Dr. Tilt is disposed to agree with Dr. Hervez de Chegoin, that ovaritis may be produced by the irritative pressure of a retroverted uterus. He quotes the opposite opinions of Dr. Rigby and Dr. Oldham on this subject; and thinks that the truth lies between the extremes. Still he condemns the use of the stem-pessary, which is advocated by Dr. Rigby. Dr. Tilt knows of no facts to prove that drastic medicines produce ovarian disease; but he inclines to Siebold's idea, that abortive remedies may primarily influence the ovaries. As specific causes of ovaritis, our author refers to the puerperal state, the rheumatic diathesis, and blenorrhagic infection. With regard to the latter, he remarks on the obvious analogy between orchitis in the male, and ovaritis in the female, under similar circumstances of infection; but he also notices the opposite opinion of Dr. Simpson, which, to our surprise, our own observation has confirmed,—that after carefully seeking for it in gonorrhœal females, it has not been found. Dr. Simpson saw it in only one case out of some hundred, and even that case was doubtful.

*Symptoms of ovaritis.*—1. Dr. Tilt first describes the general symptoms of ovaritis, such as are common to it under all its forms; and then those "peculiar phenomena with which they may be allied, and by which the local disease itself is often masked."

"The patient experiences a dull pain in the ovarian region, often imperceptible when she is in a state of repose, but brought on by walking, riding, by any sudden movement, or even by pressure on the side. The pain is also increased by the act of straightening the thigh upon the pelvis, as in the erect posture, by which the integuments are put upon the stretch, and pressure is thus exerted over the part. Some patients are unable to maintain the erect posture without resting the foot of the side affected on a stool, so as to keep the thigh more or less bent upon the pelvis, whereby the integuments, &c. are relaxed. Radiating from the ovarian region, the pains are felt across the loins; they descend towards the thighs and fundament, and are of a dull, dragging, heavy, and sometimes of an overwhelming nature. They are distinguished by the patient from other pains resembling colic, and which depend on uterine contractions, although both species of pain may be experienced at the same time; they are likewise to be distinguished from those *superficial* pains which are caused by reflex nervous action, and which so frequently accompany every species of disorder of the organs of generation. They are, however, seldom so acute as to induce the patient to seek for advice. She may submit to them for years, but should she find them so wearisome to mind and body as to be led to seek advice upon her case, she is frequently treated for uterine disease." (pp. 76-7.)

In addition to these symptoms, there is pain in sexual intercourse; and in two cases Dr. Tilt has noticed pain and swelling in the left side, coinciding with inflammation of the left ovary. When an ovary is large enough to descend upon the roof of the vagina, pains in defecation are severe; and the ovary, Dr. Tilt says, bears down the uterus, so as to produce its complete retroversion. The finger in this case may feel the ovary per vaginam, and the upper part of the vagina is sensibly hot. But when the ovary cannot be felt, stretching the fundus of the womb away from it, by moving the cervix in the opposite direction, occasions local suffering.

**Nymphomania**, in the opinion of the author, is not excited by *ovaritis*; but, on the contrary, the female under the influence of inflammation loses sexual passion, and even repels sexual intercourse. The secondary pelvic inflammation, which follows some days after labour, as described by Dr. Doberty, is quoted by Dr. Tilt as a puerperal variety of sub-acute *ovaritis*.

Upon the several symptoms here noticed, we would remark, that it is by no means an invariable fact, that even when an ovary sub-acutely inflamed can be felt by the vagina, pain in defecation is complained of. The pressure of the *fæces*—as we have frequently found—does not mechanically influence the ovary; although sometimes we have known the pain to be very acute. The extent to which the rectum is impeded by ovarian swellings, or by a retroverted uterus, is often greatly exaggerated; and is made to square with some ideal plans of the relative position of these pelvic viscera in health, rather than with their mutual accommodation, as seen practically in disease. We assent generally to Dr. Tilt's notion of nymphomania being unconnected with *ovaritis*; but a well-marked case occurred in the practice of a friend of our own, in which an aged female exhibited intense and sudden sexual passion, and during the prevalence of which she died; one ovary was four times as large as its fellow, and was evidently acutely inflamed. The influence which Dr. Tilt has ascribed to the swollen ovary, in bearing down the uterus in a retroverted state, appears to us to be rather imaginary. That the uterus should be large, heavy, and in consequence directed backwards, as a sympathetic engorgement from acute or even sub-acute *ovaritis*, we readily allow; and this has been recognised by some authors. But that the ovary should cause this displacement, irrespective of the womb being overweighted, is contrary to our own experience. The puerperal form of sub-acute *ovaritis* referred to, is really, we believe, inflammation of the cellular tissue of the pelvis, in which the ovary may possibly, in some cases, be involved. Every experienced obstetrician is well acquainted with these cases; but it is an unpractical attempt to magnify his subject, which has induced Dr. Tilt to fix the starting-point of the inflammation in the ovary, and to include in a secondary way the adjacent cellular tissue.

2. The symptoms which, accompanying and depending on *ovaritis*, supplant the real affection, are *amenorrhœa*, *dysmenorrhœa*, *menorrhagia*, and *hysteria*; and each of these is separately noticed, although the author, as we have before observed, very properly disclaims their constant and necessary connexion with this disease. In association with the *amenorrhœal* type, Dr. Tilt argues in favour of the production of chlorosis from an arrest in the normal evolution of the ovaries; and in cases where chlorosis has followed a sudden suppression of the menses in a healthy woman, he infers that the sub-acute *ovaritis* occasioned by it produces just what an arrest of development produced in the first case.

The action of the sub-acute *ovaritis* in the production of *dysmenorrhœa* is two-fold.

"1. Sub-acute *ovaritis* may of itself produce *dysmenorrhœa*, as a simple result of the process of morbid ovulation, and not by the agency of any appreciable inflammation of the womb, or of its neck, and without any appearance of false membrane in the catamenia. This is what we have seen and believe to be frequent.

"2. *Ovaritis*, as Dr. Oldham has well shown, often causes *dysmenorrhœa* by



determining hypertrophy of the uterus, inflammation of its neck, and a diphtheritic exudation from its mucous surface. We know that the ovaries, in virtue of their governing influence over the uterus, induce periodically a state of vascular turgescence in the walls of this organ; and it is not surprising to find that ovaritis frequently induces the exaggeration of this physiological state, or the inflammation of the inner surface of the womb and of its neck; thereby transforming the thin, transparent mucous membrane of the womb into a thick, soft cribriform membrane, and producing the retention or painful excretion of the catamenia, which are mingled with pseudo-decidual membranes." (pp. 87-8.)

In illustration of the menorrhagic type, he quotes a case of Dr. Rigby's; and there can be no doubt that copious menstrual losses, with double or intermenstrual periods, are a frequent result of ovarian irritation and congestion. In practice, speaking from our own experience, this is the most common of these disorders; dysmenorrhœa is the next in frequency; and then follows, as a rare affection, the amenorrhœal type, on which our author has laboured to make out something theoretically.

Dr. Tilt's theory of hysteria may be given in his own words:

"Hysteria, a disease almost peculiar to women in the reproductive period of her life, often attends menstruation or its anomalies, is often accompanied by a painful tension of the pelvis, and mucous discharges from the vagina, which reminds one of the abundant flow of tears under the influence of sub-orbital neuralgia; and as, on numerous occasions, no other lesion is found to explain these symptoms in the dead body, except congestion or inflammation of the ovaries, we are led to believe that hysteria often originates in ovarian irritation. Given the nervous, irritable disposition alluded to, and the laborious elaboration and elimination of the first ovule, or the monthly repetition of the same function, the delay or the denial of the proper ovarian stimulus, and sometimes even its enjoyment—we shall find that hysteria is always connected with ovarian irritation, and often depends on sub-acute ovaritis; whether we consider the causes of hysteria, its symptoms, and the only lesions which are found when (as in some rare cases) the patient is carried off during its occurrence." (p. 93.)

In illustration of this theory, Dr. Tilt quotes several cases from eminent medical authorities, which justify the view of the occasional dependence of hysterical disorder on sub-acute ovaritis.

*Terminations of subacute ovaritis.*—The affections which are bound up with subacute ovaritis, and which Dr. Tilt calls its terminations, are sterility, ovarian, and intra-pelvic peritonitis, uterine congestion and inflammation, and inflammation of the cervix; and he explains the persistence of severe pains and relapses after the cure of the more obvious uterine lesion, by referring them to the ovary, which continues morbidly affected.

*Sterility.*—Slight uterine lesions, says Dr. Tilt, the mere effects of ovarian irritation, are considered, and sometimes treated, at the hazard of the patient's life, as the primary cause of sterility; and to enforce the truth of this observation, he transcribes the fatal case which was recorded by Dr. Oldham, and quoted in detail in a recent number of this Journal.

How does sub-acute ovaritis produce sterility? Dr. Tilt's answer is, "1. By accelerating the shedding of imperfectly-developed ova. 2. By the retention of blighted ova. 3. By impeding their transmission from the ovaries to the uterus." The *first* of these causes is maintained by a purely theoretical argument, which may turn out to be true, but needs facts to support it. It allows the author to apply the very expressive term of

remittent menstruation, to cases where there is scarcely any interval between the periods; and to use the phrase ovarian abortion, to designate the escape of an immature ovum. The proofs of the operation of the *second* of these causes depend on the authority of De Graaf, and an ovum seen by Dr. Stanlen in the left ovary of a female who committed suicide. "Under the ovarian peritoneum was a minute cyst, containing an ovum about the size of a cherry-stone—it adhered to the cyst in two thirds of its circumference—and was distinctly formed of the chorion and amnion, but offered no trace of a foetus!" And Dr. Tilt really believes, with our present knowledge of the formation of the amnion, that this was an ovum! But this cause of sterility is supported with unquestionable proof, when the author refers to the production of false membranes, which sometimes strangle the ovary in their web. The *third* cause refers to the blocking-up of the Fallopian tubes by mucus, and has no real or apparent connexion with sub-acute ovaritis.

The effect of sub-acute ovaritis in inducing uterine engorgement and congestion of the cervix, is illustrated by some cases; we have no doubt about the fact.

*Treatment of sub-acute ovaritis.*—Dr. Tilt premises that the treatment of sub-acute ovaritis should only be undertaken during the intervals between the menstrual times. His resources for this purpose are to a great extent local, and such as the profession is sufficiently familiar with, as they differ in no important respect from what is usual in inflammatory engorgements of the uterus. Leeches over the site of the ovarian pain, eight to twelve in number, followed by poultices, are recommended. Dr. Tilt does not approve of leeches to the os uteri, or to the rectum, as recommended by Dr. Rigby. We quite accord with him as to the uselessness of the latter plan; but we are by no means disposed to assent to an exclusion of the former, which we have constantly found most serviceable. They entail a comparatively small loss of blood in women who cannot spare it; the bites heal well, without irritation; and as for the excruciating pain which Dr. Tilt speaks of, we are bound to say we have never met with it. Saline purgatives are required to remove scybala, which might irritate the ovary. Sedative injections per rectum are recommended; but Dr. Tilt thinks that narcotic vaginal injections rather irritate than relieve, a conclusion with which our own experience is perfectly at variance. Blisters and mercurial inunction combined with sedatives, as the extracts of belladonna, hyoscyamus, and opium, are habitually employed by him. Warm baths, the horizontal posture, the protection of the feet against damp, and of the pelvic organs against cold by wearing drawers, abstinence from sexual intercourse, and a general treatment which will invigorate the constitution without increasing the local irritability and determination of blood to the pelvic organs, are enjoined. There is no mention of the best remedy of all, in our minds, namely, the prescription of very small doses of mercury in combination with tonics. We do not hesitate to say that, in an extensive experience, we have derived more permanent good from this combination, variously adjusted, than from any other means.

In the *amenorrhæal* type, our author, with a questionable prudence, enjoins the employment of leeches as indispensable, even when chlorosis is present. We have met with cases which have been treated by leeches, and

have failed to recognise their value. The author limits them to cases where, the menses having been suppressed, chlorosis follows. But when the chlorotic change has occurred in the blood, leeches even in small numbers depress the powers of life, and occasion a feeble health and a protracted convalescence. The same observation applies to the menorrhagic type, in which, Dr. Tilt says, "in spite of the patient's weakness, we must apply leeches." Our comment on this imperative injunction is, that milder means are far better; and that if mercury be given in the way we have mentioned, the local irritation will be lessened, and the powers supported, without the necessity of reducing a circulation already drained. In the treatment of the hysteric type, Dr. Tilt advocates the employment of cold water enemata. He starts the question, whether marriage ought to be sanctioned when the ovaries are sub-acutely inflamed—which he answers decidedly in the negative. We can only observe, that taking our author's views *in extenso*, and holding him to them on this question, he would, if made autocratic in this matter, keep up the stock of "involuntary nuns," who would then form a large, but we should expect a very rebellious, section of the female community. It is true that he qualifies, and rather neutralizes, his veto, by giving an affirmative answer to the question,—Is marriage to be countenanced when the ovaries are prone to be sub-acutely inflamed? maintaining that the proneness may be removed by the normal performance of the proper functions of these organs. But we must own that we cannot clearly make out the rule he would lay down on this subject.

*Treatment of sterility.*—The treatment of sterility has nothing special in it, but is included in the general treatment of sub-acute ovaritis. But Dr. Tilt takes occasion to animadvert on the practice originated by Dr. Tyler Smith, and which has been somewhat ostentatiously paraded as the new operation for the cure of sterility. It consists of—1, the introduction of a speculum; 2, the passing a hollow canula, slightly curved, through the uterus to the orifice of the Fallopian tubes; and 3, the introduction of a fine whalebone bougie through the hollow canula, which is then to be passed through the Fallopian tubes, under the idea of removing any obstruction from them. No one but the most shallow enthusiast or designing impostor could for one moment maintain the possibility of removing by this flexible-bristle—passed at this distance through a yielding and tortuous canal—any *solid* obstruction, such as the deposit of scrofulous matter, the congenital occlusion of the fimbriated extremity of the tube, or its strong close girthing by false membranes. Hence it is that Dr. T. Smith lays especial stress upon the removal of thick mucus, or a hardened plug of mucus, the "mere debris of the Fallopian secretion," which in gorgeous language he declares "may cut off an illustrious race or change a dynasty." Of course, this saving pathological condition, although so suspiciously allied to such a palpable hint to the sterile in high places, is just enough to rescue Dr. T. Smith from any slur upon his professional intelligence or purity of motives, in counselling women to go through a doubtful, dangerous, difficult, almost impracticable operation, with the hope of clearing it away. We quite accord with the following sentiments of Dr. Tilt:

"Will men of eminence attempt to probe, dilate, and inject, the Fallopian

tubes? We hope not, for peritonitis is not a disease to be trifled with. When we consider that we can only *guess* at this possible cause of sterility, and have no positive evidence of its existence; when we remember that in the dissecting-room, it is often difficult to pass a probe from the uterus into the Fallopian tube,—the difficulty of the operation seems tantamount to an impossibility. This impossibility cannot be regretted; for the advantage attending the operation could only be attained at the risk of imminent danger to the patient's life.....

"We still doubt the possibility of its performance on the living body, and we would wish to see it put in practice, and thus confirmed by the operation being performed on the dead subject, and the probe being then found in the oviduct on the opening of the body. But, admitting the possibility of the operation, if the tube contains but its ordinary secretion the process is useless—if the mucus be thick, then the bougie will no more remove it than it removes the glutinous plug which so often obstructs the neck of the womb; besides it could not modify the inflammatory condition of the lining membrane of the tubes, which causes them to secrete the glutinous substance, and thereby produces their temporary occlusion; whereas, if it were attempted to cauterize the oviducts, or inject them with different liquids, we consider the danger of the practice would far exceed the inconvenience of the infirmity it is intended to alleviate." (pp. 143-6.)

Amongst the practices which have lately been shown to be so mischievous to women, and the mention of which in a recent meeting of the Medico-Chirurgical Society, excited the indignant murmurs of a large body of the profession, we do not hesitate to say that this, although not specially referred to, is the worst. The object it proposes is founded at the best on doubtful pathological states; for the observations by different authors, which Dr. T. Smith has quoted to support this operation, are wanting in that precision of statement, as to the organic changes in the tubes which have caused the albuminous exudations, the physical characters of the morbid secretions, and their capability of being removed by a bougie, which might, if supplied, have been conclusive against it. Speaking from our own knowledge on this subject, we are bound to say that we know nothing of thick or hardened mucus, as a solitary cause of obstruction to the canal of these tubes. The operation is full of mischief. The canula has all the worst properties of the uterine sound; and as to the whalebone, if it should chance to find its way into the tube, it can only tickle, tease, and scratch its parietes. And this is to be done upon a diagnosis of the local affection which is a mere guess, or gathered up only from such large and undefined negative considerations, as will allow the greatest number of votaries to embrace this forlorn hope, with nothing certain as to the cause of the sterility. What success has attended this practice, we do not know; but, unless the chapter of chances is favorable, which ought always to be calculated on in such a disorder, we anticipate a result very closely matching the nature of the affection it seeks to cure. But we had well-nigh forgotten that Sir B. Brodie has given Dr. T. Smith a little doubtful encouragement. However great as a surgeon, Sir Benjamin's opinion in obstetric matters is but third rate; and it looks like a consciousness of weakness, to have sought a kind of testimonial help from him. But, as Dr. Tilt observes, "Sir Benjamin has omitted to inform us whether he has performed the operation himself;" and we can hardly imagine a greater caricature of this eminent man, than to fancy him probing a barren woman's fallopian tubes, to move away some mucus which may or

may not be there. We have not yet heard of any one, save Dr. T. Smith himself, who has undertaken this operation. Even those who are ready to undertake anything, however difficult or troublesome, provided it only promises fair, seem to hold off from this; and Dr. T. Smith may appropriate to himself the undisputed prerogative power of cleansing this narrow secluded crossing to the ovaries. He appears sanguine of success. We only hope that, in the using of these instruments, the shadows of sterility may not be deepened by injury to life; and that, as in some late cases which offered a far better prospect than this, the bright anticipations of progeny may not suddenly be obliterated by the dismal dark announcement of a desolated home.

*Acute ovaritis.*—We need not follow our author into his description of the pathological anatomy, or the causes, symptoms, and diagnosis of acute ovaritis. The chapter on the diagnosis is not so full as it should be. It should embrace the diagnosis between ovarian inflammation and abscess, and pelvic inflammation and abscess, which the author appears to confound; and also the diagnosis of an inflamed ovarian cyst, and of malignant disease of the rectum, or the softer forms of cancer in the pelvic structures, from the varying tumours following acute inflammation of the ovary.

*Terminations of ovaritis.*—Acute ovaritis may end in resolution; or if suppuration occurs, the pus may be eliminated through the vagina, either directly, or through the tube and uterus, or by the bladder, intestine, or external surface, or it may burst into the peritoneum. In this chapter Dr. Tilt considers the effect of a local pelvic peritonitis in obliterating the tubes, and their subsequent rupture. As we have before said, we are quite at variance with him as to the distension of the tube by menstrual blood; and we cannot coincide with his admiration of the dilatation of the os uteri in cases of dysmenorrhœa, on the assumption that “the menstrual blood not finding a free exit by the os uteri, distends the Fallopian tubes and is poured into the peritoneal cavity.”

The most important subject connected with the treatment of acute ovaritis, is the question of artificially opening any collection of pus, or leaving it for spontaneous elimination. Dr. Tilt is a warm advocate for the former, and he relates cases and authorities in support of this practice. Professor Recamier, who has an ardent admirer in Dr. Tilt, has constantly operated in these cases. We quote the author's opinion on this subject:

“The thickness of the parietes of the abscess may be such as to delay its spontaneous opening for a long time. It may thus acquire a large size, and predispose the patient to peritonitis, by extension of the inflammation, as well as by the continued presence of a large quantity of pus in the system; there is in such a case a greater chance of its perforating the peritoneum, and causing mortal peritonitis. Even when the perforation fortunately takes place through the skin or a mucous membrane, it will seldom do so until too much mischief has occurred, by extensive inflammation in the adjoining organs and cellular tissue, for the constitution to be benefited by the result; while, at the same time, prolonged sickness, hectic fever, and subsequently protracted suppuration and permanent fistula, reduce the patient to the most frightful state of marasmus. It often happens that the spontaneous opening of the abscess is not effected in the most favorable situation for voiding the pus, and thus a vitiated fluid is allowed to remain in the cul-de-sac, causing

inflammation of the surface of the cyst, which may be followed by symptoms of its absorption. Should the abscess communicate with the bladder or the intestines, the contents of these viscera may penetrate into the ovarian abscess, causing symptoms which are afterwards explained by the post-mortem examination. Thus, in two cases, where matter was found in ovarian tumours, death supervened upon diarrhœa, which had lasted a year, although the causes of its existence were not satisfactorily explained. The issue of the pus may also occur in an intermittent manner; thus Chomel mentions, in his lectures, that two of his patients experienced every two or three months a swelling in the iliac region, and then passed a considerable quantity of pus by the vagina. One had been in this state for two years, the other for eight. If, instead of leaving the opening of pelvic tumours to Nature, the surgeon, as soon as fluctuation becomes manifest, opens them with all due precaution at the place where they point, and whence, consequently, the pus can easily flow, the patient is immediately relieved from the pain arising from the inflammatory distension of the cavity, and from many other dangers which we have already enumerated. Loss of strength being thus prevented, the patient has a better chance of recovering from the complaint; for it stands to reason that the small incision thus made has a greater tendency to heal than the rugged lips of a spontaneous and ulcerated opening. Chronic inflammation of the neck of the womb, of the vagina, the rectum, and the bladder, the results of the continual passage of pus on the mucous membranes of these parts, is also generally avoided by this artificial opening; no doubt from the tumour collapsing, and its sides speedily adhering, and thus healing without fistula. By opening these tumours in that portion of their extent accessible to the surgeon, we have also the great advantage of being able to inject various liquids into their cavity, whether our object in so doing be to remove the fetid secretion, or, by keeping them full, to preclude the entrance of air." (pp. 214-16.)

Dr. Tilt has added some useful remarks on the method of operating, and the precautions which are necessary in doing so. A vaginal opening is always to be preferred.

We are only doing justice to Dr. Tilt, in saying that his views on the pathology of the ovaries are likely to modify and improve the present treatment of uterine disease. He has taken great pains in the collection of the materials for this work, and has done full justice to those who have preceded him. It is as his cordial well-wisher, that we would suggest to him not to be in too great a hurry to publish again; but to wait until he has acquired for himself a larger experience in the particular investigations he has undertaken. He could not have selected a subject more difficult as to its practical details; and his present work is a good earnest of what he may accomplish by and bye, if he steadily pursues it at the bedside and in the post-mortem room. Although we consider that it would have been much the better for that well-founded confidence and sound discrimination, which a larger personal experience can alone convey, we still regard it as a seasonable and valuable publication, well-deserving the attentive perusal of those who are interested in obstetric medicine.

## ART. XIII.

*A Manual of Elementary Chemistry, Theoretical and Practical.* By (the late) GEORGE FOWNES, F.R.S., Professor of Practical Chemistry in University College, London. With numerous Wood Engravings. Third Edition.—London, 1850. Foolscep 8vo, pp. 605.

THE rapid sale of this Manual evinces its adaptation to the wants of the student of chemistry; whilst the well-known merits of its lamented author have constituted a guarantee for its value, as a faithful exposition of the general principles and most important facts of the science to which it professes to be an introduction. We learn from the advertisement to the present edition, that its preparation for the press was the daily occupation of Professor Fownes, until a few hours previous to his death, which occurred in January, 1849. "His wish and his endeavour, as seen in his manuscript, were to render it as perfect and as minutely accurate as possible." The only part which he had left unaltered, namely, the *Animal Chemistry*, has been revised by Dr. Bence Jones; who has also superintended the passage of the volume through the press. The additions amount, in the whole, to nearly forty pages; and of these the greater part, as might be anticipated, are devoted to the constantly-accumulating novelties of organic chemistry. We are glad to meet with a notice of the "law of substitution" (p. 355), and of several other doctrines recently advanced, which seem likely to exercise an important influence on the progress of the science; but we still think that the chapter on the 'General Principles of Chemical Philosophy' might be extended with considerable advantage to the learner; and now that there are so many separate treatises for the instruction of the student in analytical operations, we doubt whether it would not be preferable to omit all reference to these,—meagre as such an account of them must necessarily be,—and to allot increased space to those portions of the Manual, in which we should like for the student to find information of a kind that can only be gleaned elsewhere from large and expensive works.

In our notice of the second edition of this work (vol. I, p. 254), we made a few observations upon the impolicy of attempting to cram the medical student with the *details* of chemical science, and upon the superior value of instruction in its *principles*, such as may at the same time prepare him for the readier mastery and fuller comprehension of such of its specialities as he may be more particularly concerned with, and may have a superior effect as a mental discipline. To these remarks we would refer our readers; our subsequent consideration of the subject having only led us to an increased conviction of their truth and importance. We have only to add, that Dr. Bence Jones appears to have performed his editorial task most thoroughly, the want of the author's final supervision being nowhere discoverable.

## Bibliographical Notices.

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ART. I.—*An Arctic Voyage to Baffin's Bay and Lancaster Sound, in search of Friends with Sir John Franklin.* By ROBERT ANSTRUTHER GOODSIR, late President of the Royal Medical Society of Edinburgh.—*London*, 1850. 12mo, pp. 152.

MANY of our readers may be aware, that among the officers of Sir John Franklin's expedition,—the fate of which is now a source of such eager apprehension and lively sympathy,—Mr. Harry Goodsir, the brother of the distinguished Professor of Anatomy in the University of Edinburgh, held the post of Assistant Surgeon and Naturalist. Prompted by the natural desire of gaining intelligence of his lost relative, another brother Mr. Robert A. Goodsir made an arrangement, in March 1849, with Mr. Penney, master of the "Advice" of Dundee, to sail with him as Surgeon on the whaling voyage on which he was just starting; it being understood that Mr. Penney would do all he could in the work of exploration, consistently with the object of his voyage. The energy and talent shown by Mr. Penney on this and on a former occasion, have led to his being intrusted by the Government with the command of an expedition which sailed some weeks since in search of the missing ships, and in which, we believe, Mr. Robert A. Goodsir has also a place.

The little work before us does not announce itself with any pretensions to notice, either as to its subject, or to the manner in which it is treated; being a narrative of the ordinary events of a whaling voyage, written by one who is evidently unpractised in the craft of authorship. We are sure, however, that any one who may be led to take it up by a feeling of interest in the author's object, will be induced to continue his perusal by the interest which he will soon take in the narrative itself; so easy and natural is the mode in which the incidents of the voyage are described, and so much variety does it derive from the observing powers of the author. Of this we shall give our readers an opportunity of judging for themselves, by making a few short extracts.

The first of these relates to *night* on the arctic seas, which, as all our readers know, is not characterised, during the summer months, by the darkness which we mentally associate with that division of the twenty-four hours. Mr. Goodsir had taken his place at the oar of one of the whale-boats, which had given chase to a "run" of whales; and the whole party had been so absorbed in the eagerness of pursuit, that they did not duly note the lapse of time,—having a sort of idea that they had been a night and a day absent from the ship, but not feeling any certainty on that point.

"I suspected it was again night; but I could scarcely think it possible, the time seemed to have passed so rapidly. But there was a *stillness* about the air that



must have struck every one as peculiar to the dead hour of the night; and although I have noticed it in far different situations, it never struck me so forcibly as it did here. The light passing breezes and cats' paws, which had dimpled the water for some hours back, had died away. It was now so calm that a feather dropt from the hand fell plumb into the sea. But it was the dead stillness of the air which was so peculiar. No hum of insect, none of the other pleasant sounds which betoken it is day, and that Nature is awake, can be expected here even at midday in the height of summer, twenty miles from land, and that land far within the Arctic Circle, where, if one may say so, a third of the year is one long continuous day. Yet there is a most perceptible difference,—there is a stir in the air around,—a sort of *silent music* heard during day, which is dumb during night. Is it not strange that the deep stillness of the dead hour of night should be as peculiar to the solitude of the icy seas as to the centre of the vast city?" (p. 88.)

On a subsequent occasion, when a dead whale, far advanced in putrefaction, was being towed to the ship, a curious occurrence is chronicled by Mr. Goodsir, as follows:

"The approach of the sweet-smelling stranger was announced by the most unearthly music, though, perhaps, it would not have been thought so by a thoroughbred Highlander; it was the bagpipes to a note—to a tone. I almost thought I could recognise a long-remembered strathspey; but where could be the bagpipes? It was soon all explained, however; the thrusts of sundry lances into the swollen carcass had made small apertures into the abdomen, from whence issued the gas confined therein, each forward tug of the boats graduating the tension of the abdominal muscles, and at the same time graduating the emission of the gas, transformed the dead whale into a musical instrument" (p. 127.)

The following passage will convey an idea of the exuberance of some of the most beautiful forms of animal life, even in the midst of ice-bound waters:

"During the beautiful weather which we enjoyed at Pond's Bay, nothing could be more interesting than a walk on the floe; in every crack of the ice were to be seen *Acalephe* of the most beautiful forms and brilliant colours,—crimson, purple, and azure; whilst their long tentacles floated gracefully beneath them, and their ever-moving cilia were brightly iridescent. The less gay but as graceful *Clio* would be seen floating amongst them, and the sombre-coloured *Limacina* moving quickly by fits and starts; but not nearly so quickly as the merry bounding hither and thither of the bright yellow little *Gammarus*. Fancy all these in a narrow split or crack of the floe not more than a foot and a half wide, bounded on either side by the deep blue of the submerged part of the ice, which appears as if it were suspended to the bright white of that portion which is above water." (p. 130.)

Mr. Goodsir did not find his expedition of as much value to him in a zoological point of view, as he seems to have anticipated:

"The naturalist, if he takes his chance in a whaling voyage, will find that he will be able to do little if the voyage is successful *as a whaling voyage*; but if, on the contrary, it is 'a bad year,' in whaler parlance, then there is little to prevent him from reaping a rich harvest. I had every assistance possible from Mr. Penny, but from my own inexperience I am afraid I lost not a few opportunities of observation. You may recollect the story of John Hunter's sending out a surgeon specially to Greenland to make a collection for him, and that at not a little expense—of his chagrin when the man returned with a collection consisting of a piece of whale's skin, to which were attached some of the whale louse (*Oniscus ceti*) and nought else. But I much suspect that the man need not be so much laughed at. Ten chances to one, he was with people who would laugh and sneer at his every effort, and throw difficulties in his way that he would not have it in his power to surmount. I was not thus situated myself; but I saw and heard

quite sufficient to show me that the position of surgeon of a whale-ship is no very enviable one, and too often, I am sorry to say, rendered worse from their own mismanagement." (p. 115.)

We shall only add a couple of passages, which notice, in an incidental way, an important change in the habits of whaling-sailors, whose ample experience must be regarded as affording unequivocal testimony to the superior value of the beverages they now prefer :

"Our men had been this year provided with an apparatus which greatly conduced to their comfort, in the shape of conjurors fitted up with large lamps by which means they could make for themselves hot tea or coffee as they required it, when they were away from the ship for any time. Each of the boats was supplied with one of these conjurors. . . . Then each and all betook themselves to the infusions and decoctions of their tea and coffee, the whaling sailor's greatest luxury and comfort. He has no objection to his grog, but I think he has, long ere this, found out, that hot strong tea or coffee, particularly the former, is by far the best beverage he can take in these climates." (pp. 135-8.)

We cannot conclude this notice without expressing the earnest hope, that either Mr. Goodair's party, or some of the generous volunteers who have set forth on the same mission, may meet with the desired success ; and that none of those who are perilling their lives for the succour of their distressed countrymen, may become the objects of similar anxiety.

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ART. II.—1. *Eleneo Sommario delle Operazioni di alta Chirurgia eseguite nel decorso anno 1847, nel venerabili Arcispedale di S. Spirito.*—Roma, 1848. 4to, pp. 56.

*Account of the great Operations of Surgery, performed in the year 1847, in the Great Hospital of Rome.*

2. *Prospetto di alcune Operazioni d'alta Chirurgia eseguite dal Dottore F. ARIETTI, di Vercelli.*—Vercelli, 1848. 8vo, pp. 31.

*Account of some of the great Surgical Operations performed by Dr. ARIETTI, Professor of Anatomy in the University School of Vercelli.*

3. *Delle Operazioni di Chirurgia Minore e della Vaccinazione, modo di eseguirle, loro Indicazioni ed Effetti.* Di G. B. MONTEGGIA.—8vo, pp. 60.

*On the Minor Operations of Surgery, and on Vaccination ; the Manner of performing them, their Indications and Effects.* By G. B. MONTEGGIA.

It is very rarely that we obtain Italian surgical works : in the first place because Italian surgeons prefer translating from French and English, adding a few notes of their own, to the labour of recording and arranging the results of their personal observation ; and in the second, because their original works, when published, are found with difficulty, owing to their peculiar manner of publication. When an Italian medical man writes a work, he almost invariably prints it himself, at his own risk and for his sole profit. He will neither trust a bookseller, nor allow him a share of gain. The consequence is, that even in the towns where a book is printed, the bookseller knows nothing of it, or pretends that he does not, and it is only by making the acquaintance of an author, that one hears that he has works for disposal. The last of the works just quoted is apparently a new one, and is part of a larger work on the 'Institutions of

Surgery,' yet the title-page does not even contain the name of the city where it is published, still less a date, or any direction where it can be obtained. Again, it is very rarely that works when met with are found to contain much original matter, any account of the progress of Italian surgery, or any record of the cases treated in the immense hospitals which are crowded with patients in all the larger towns of the peninsula. The pamphlet of Dr. Arietti is a specimen of the most numerous class of the works of Italian surgeons, and it is a very bad specimen of a very bad class. A medical man, in the course of a few years, meets with five or six cases which he considers unusual or successful, or at any rate likely to do him good if made known. Upon this, he makes a pamphlet, dedicates it to the chief personage of his district, glorifying himself and the "Most High, who has blessed the beneficent hand that saved the patient." (Arietti, p. 31.) The cases are, one of lithotomy in a child; one of lithotomy; one of extirpation of an encysted tumour; and one of arteriotomy in a case of apoplexy: none of them possessing more than the most ordinary interest. Among the cases in the Report of the Roman Hospital are three of lithotomy in children, two being successful, one fatal; five successful cases of cataract operated on by depression; one of cancer of the lip, which reappeared; nine cases of contraction, in one of which violent hemorrhage was stopped by the application of Matico leaves; and in another, death followed gangrene of the cord to which ligature had been applied in mass. Some cases of encysted tumours, fistulæ, hydrocele, amputations of the penis and of the fingers and toes, with one of amputation of the thigh, and another of extirpation of the eyeball, complete the list of operations in this hospital during the year. We find nothing of interest in the details.

The little work of Dr. Monteggia is a plain and useful work for students, detailing what should be known about venesection, arteriotomy, leeches, cupping, the seton, issues, moxa, blisters, enemata, and vaccination; but not containing anything which will not be found in our own elementary works and dictionaries.

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ART. III.—*A Popular History of Mammalia; comprising a familiar Account of their Classification and Habits.* By ADAM WHITE, F.L.S., Assistant in the Zoological Department of the British Museum.—London, 1850. Square 16mo, pp. 364. With Sixteen Coloured Plates.

WE regard it as a most valuable feature in our present literature, that men of high scientific rank do not now disdain to write elementary treatises; so that the race of mere compilations and transcripts which so long held possession of the field, is being gradually driven from it by original works of vastly superior character. In this enterprise the Messrs. Reeve are bearing an honorable part; and we wish them all success. We have already noticed one of the series of illustrated popular works on Natural History, which is in course of publication by them; and the volume now before us fully sustains the character which we had the pleasure of giving to Mr. Landsborough's 'History of British Seaweeds.' It consists of a popular view of the classification of the Mammalia, the characters of the principal groups and of their most important subdivisions, and notices of the habits and mode of life of some of the most interesting species. The

illustrative figures are drawn with great spirit, principally from living specimens in the menageries of the Earl of Derby and the Zoological Society, and the remainder from stuffed specimens in the British Museum. The classification adopted is that of Mr. Gray, and is consequently in accordance with that of the zoological collection which is under the charge of that eminent naturalist. In some respects this is an advantage, as the book is the better enabled to serve as a guide to the Mammalian department of the British Museum. But we cannot help expressing our regret that a view of the relations of animals, so inconsistent with all sound anatomical and physiological principles, as that which places the whole series of Marsupialia (including the graminivorous Kangaroo) between the Moles and the Seals in the order *Ferae*, and the Ornithorhyncus and Echidna between the Ant-eaters and the Sloths in the order *Edentata*, should still be perpetuated even in an elementary work,—much more, in our national collection. In an Appendix, we are furnished with a conspectus of the class Mammalia, recently published by the Prince of Canino, in whose classification the labours of anatomists and physiologists are fully recognised.

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ART. IV.—*An Elementary Course of Geology, Mineralogy, and Physical Geography*. By DAVID T. ANSTED, M.A. F.R.S., Professor of Geology in King's College, London, &c. &c.—London, 1850. Post 8vo, pp. 584. With 234 Wood Engravings.

THE author's object in composing this treatise has been to offer to the students of geology a systematic collection of the principal facts upon which the science is based, the inferences fairly deducible from them, and the more immediate and direct applications of them to practice. Commencing with the outlines of Chemistry, he has given a general account of those combinations of elementary substances most frequently met with, of the forces acting upon matter, and of the laws which express the mode of action of those forces. These general principles are then exemplified in a survey of the existing condition of the earth's surface, and of the changes now in progress, tending to modify the physical features of various parts of the world; it being the author's endeavour to give a true and rational account of the various phenomena of the surface, the forms of continents and islands, the mountain and river systems, and generally the horizontal and vertical profile of our globe. All these facts and descriptions, which together constitute one great department of Physical Geography, are followed by an account of the atmospheric and aqueous actions, whose constant performance tends to alter the earth's surface by reducing its inequalities; and this, again, is succeeded by an account of those subterranean movements, whose result is to add to the inequalities of surface at present existing, or to produce others in new directions.

From these descriptions, the author passes to another department of his subject, namely, Mineralogy; under which head he explains and illustrates the nature of those solid materials or mineral substances, of which the whole superficial crust of the globe is made up. The singular relations of crystalline form, and the various other physical characters of these substances, are first dwelt on, and then the actual properties of the minerals themselves. This portion of the work is, perhaps, the one that

will least interest the general reader ; but it is the one which is the most important in its practical bearings.

The author then commences the subject of Geology, by a description of the simple rocks, as distinguished from, or made up of, the simple minerals ; he explains on the condition of these, dwells at some length on their structure and mechanical position, and endeavours to illustrate the true meaning and vast importance of those changes of condition, or phenomena of metamorphism, a knowledge of which is the key to all the higher speculations of geological science, and to many of its practical applications. An outline of descriptive geology is then given, embracing a brief account of the various stratified rocks, commencing with those of most modern date. In bringing these successively under notice, the author endeavours to keep constantly before the reader the fact, that group after group of different formations,—some deposited under water and some on land, some indicating the existence of a sea and others of an estuary or river,—has occupied districts now elevated high above the waves, and exposed to our researches ; and also that during the long succession of changes thus indicated, there has been a corresponding succession of changes in the races of animals and plants inhabiting the earth's surface, as indicated by the remains imbedded in the strata thus superposed on one another. The disturbances of these beds by mechanical violence acting from beneath, producing various appearances of movement and dislocation, are shown to be connected with the same kind of action, which, in its more obvious manifestations, produces volcanic phenomena, the outpouring of crystalline rocks once existing beneath the surface in a state of igneous fusion, and the more extreme cases of metamorphism.

As the natural conclusion to this outline, we find in the last two chapters an explanation of the applications of geological knowledge to engineering and mining.

It will be obvious, from this account of the scope of the work, that it is one of a most extensive range ; and that if its execution be accordant with its design, it must afford to the student a comprehensive and exact knowledge of the principles of geological science, and of those departments more especially which have the most directly practical bearing. We have much pleasure in expressing our conviction, that the author's purpose has been most ably fulfilled. Professor Ansted's mind is one of an eminently systematic character ; and he has succeeded in condensing within a very moderate compass such a large amount of information, that the reader is led to wonder how such great subjects can be so fully treated in so small a book. Of course this kind of condensation does not improve the attractions of the treatise to those who read only for amusement, or for the sake of picking up a few general ideas on the nature of geological inquiry ; for such it is not intended. And it is right that we should mention, to avoid misleading our readers, that the department of palæontology is purposely kept in subordination to that of the structure and composition of rocks.

We must not omit to state that the numerous illustrations, selected from the *Elementary Treatises on Geology and Chemistry* published under the direction of the French Government, are of a very superior kind ; and that the terms on which they have been obtained have been sufficiently favorable to prevent the treatise from being rendered too costly for the student.

ART. V.—*Revelations of Egyptian Mysteries. History of the Creation; the Causes and the Progress of the Degeneration of Nature, the Conflagration and Manner of the Resurrection of the World, as allegorically represented by the Egyptian Philosophy: showing the Justice of the inculcations of the ancient Egyptian Priests, and wise men, teaching that Salt was fatally hurtful to Human Nature. With a Discourse on the Maintenance and Acquisition of Health, on Principles in accordance with the Wisdom of the Ancients.* By ROBERT HOWARD.—London, 1850. 8vo, pp. 284.

It is not very often that we meet with men who have received a medical education, who turn from the present towards the past rather than the future, for the sake of resolving their doubts and enlightening their ignorance. There is generally some kind of confidence in the value of modern knowledge of physiology and therapeutics, which, if not strong enough to induce satisfaction with the existing state of the healing art, encourages the expectation that the progress of science will gradually render its practice more certain. And those who cast a "longing, lingering look behind," are content to dwell rather on the methods of observation employed, and on the sagacious spirit manifested, by the great minds which have from time to time shone forth as luminaries through the darkness of the pervading ignorance, than on any special rules of practice which they have laid down. To meet with a man who gravely assures us that in the hieroglyphics of the Egyptian philosophers, and in the earliest writings of the Grecian poets, are locked up the greatest treasures of medical wisdom, and that the diseases and degeneracy of mankind have chiefly resulted from a departure from their precepts, is, to say the least, a very curious phenomenon well deserving the attention of psychologists. Such extraordinary productions as the one before us should be treated, it appears to us, not as subjects for ridicule, but as illustrations of the strange results which sometimes proceed from the pursuit of some one class of studies to the exclusion of all others, especially when the student does not from time to time bring his conclusions to the test of common sense. The mind thus becomes possessed of one set of ideas, which gradually acquire such complete domination over it, that their incongruity with the ordinary course of nature is not perceived, and all external realities are strained and twisted into conformity with them.

We shall content ourselves with a brief statement of Mr. Howard's Theory of Disease: the Conflagration and manner of Resurrection of the World being topics beyond our province. Diseases, our author maintains, have arisen from violations of the law of Nature, which ordains that:

"No part of the earth which has not passed through the vegetable state ought to be ingested; and no fossil substance, taken from the earth in its original, crude, and raw state, or after any process of refinement which man is capable of operating upon it, can be used as food without engendering disease."—(p. 9.)

The "fossil substance" which man most constantly employs is *salt*; and Chapter VII is devoted to "an Investigation of the consequences resulting from the artificial use of salt." We can only make room for the following extracts by way of sample:

"After contemplating the indescribably horrible effects which salt produces, it seems impossible not to believe that it is, in its nature, under its artificial application,

most ferociously inimical to man's constitution; and I can conceive of nothing more wonderful than the fact of his having paid so little attention to this circumstance, or than that of its having been so marvellously concealed from his observation." (p. 48.)

"There is a very remarkable resemblance between the effects produced on the human constitution by salt, which is a chloride of sodium, and those caused by calomel, which is a chloride of mercury; accordingly that pernicious agent, like salt, is wont to cause many of the symptoms of scurvy, and also to induce consumption, and wonderfully to exasperate the disease, and expedite its progress when it is already in existence." (p. 49.)

Having assumed that the use of salt in large quantities is the essential cause of scurvy, Mr. Howard continues:

"When salt is more gradually introduced into the body, as in the way in which it is commonly taken with the food, it is evidently the direct cause of consumption or decline; it is clearly shown to be so by the circumstance that the conservative powers during that disease are constantly occupied in the expulsion of salt acrid phlegm from the lungs, by which they are at first irritated and excoriated, and then fall into an ulcerated state. .... Consumption is a milder form than scurvy of what may be justly called the plague of salt."—(p. 50.)

From these views it of course follows that abstinence from salt will wonderfully facilitate and expedite the cure of diseases,—a result which we are gravely assured by Mr. Howard, he has constantly observed by experience. The benefits of this system are most evident in the treatment of consumption:

"When, by abstinence from salt and salted meats, the constitution has become sufficiently freed from its influence, the irritation and excitement peculiar to consumption cease in a most remarkable manner; the cough and expectoration are greatly abated, and the condition of the patient is altogether wonderfully improved; as may be observed by any consumptive or catarrhal person making the simple experiment. By such means, with the aid of judicious medical treatment, that disease, if it have not too far undermined the constitution, is easily and permanently cured." (p. 255.)

If any of our readers wish to see how plausible the strangest hypothesis may be made to appear, when all the facts and suppositions that can be made to tally with it are heaped together, and everything that can be stated on the opposite side is carefully kept out of view, we recommend them to glance over Mr. Howard's pages. We have no doubt that he will make many converts; and that the anti-saline treatment (if he plays his cards properly) will take its place among the profitable empiricisms of the day. We would not rank Mr. Howard, however, with the impostors who make their market of human ignorance; for it is obvious that he most devoutly believes his own doctrines, and thinks that he has conferred a blessing on mankind by the publication of them, and by the corroborative testimony he has adduced from "the wisdom of the ancients." How is such a man to be treated? By fine and imprisonment? Surely not; but by enlightening the public—provided *he* cannot be convinced—as to the need for salt in the animal body, and the proved consequences of its withdrawal. Mr. Howard seems to place much confidence in the indications afforded by the natural habits of animals; did he never hear of the "salt-licks" of the United States, which are the resort of herds of buffaloes that are drawn to them at intervals by the craving they experience for this article? Or did he never hear of the evils which have arisen out of "salt

monopolies" in European states, and, in more recent times, in British India? No sensible person who examines into such evidence, can doubt for a moment that salt is a necessary of life; and that, if *totally* deprived of it, the most healthy body must speedily become diseased.

As a sample of the character of Mr. Howard's mind, which will afford some amusement to our readers, we shall present them, in conclusion, with his solution of the long-disputed problem of the purpose of the Egyptian pyramids:

"The ancient Egyptians raised the pyramids in order that they, like the stony mountains, might, on the occasion of the next terrestrial conflagration, become ignited and cause the fire to devour the stone of those regions of the country, where the earth did not naturally present sufficiently elevated summits for that purpose, and for the fulfilment of which other circumstances were not favorable; so that their country might by that means become covered with fertile spots, which would not otherwise have happened. It is possible, and even likely, that the Egyptians may have observed that some of the fruitful oases in the Libyan deserts had been so produced, by artificially built mountains. I think it extremely probable that such evidence may still continue, in the neighbourhoods of existing oases, where there are probably pyramids not entirely consumed; as in various parts of the world are found stony mountains which have been partially melted by fire. The Egyptians did not want that instruction; but it is likely they knew that such had been. I have repeatedly described that the combustion of stone cannot go on to any very great extent without an abundant supply of water. Accordingly the Egyptians have, with wonderful ingenuity, made provision whereby the fire should be supplied with plenty of water. Those pyramids which have been entered are found to contain vast cisterns and reservoirs of water," &c. &c. (p. 205.)

Which "fully accounts for the milk in the cocoa-nut," as the showman hath it. Verily "the force of ——— can no further go." Let our readers supply the hiatus with any word of two syllables—such as wisdom or folly, learning or nonsense—which seems to them most apposite to the preceding extract.

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ART. VI.—*Practical Ventilation as applied to Public, Domestic, and Agricultural Structures. Being an elucidation of Plans and Suggestions, of easy application, for Ventilating every species of Architectural Structure; with Remarks on Heating, Construction of Fire-places, Cure of Smoky Chimneys; and an Appendix on the Ventilation of Ships, Steam-boats, and Railway-carriages.* By ROBERT SCOTT BURN, Engineer.—*Edinburgh*, 1850. Post 8vo, pp. 208. With numerous Illustrations.

THE object of this little work is sufficiently explained in its title; and we have only to speak of its execution. The author exhibits a full acquaintance with the physiological principles on which the necessity for ventilation depends; and has evidently given great attention to the various plans for accomplishing it, of which so many have been promulgated within the last few years. We are not aware of any important suggestion which has escaped him; and in throwing together the results of his own experience, and in explaining the methods of others, he has given to his little work an eminently practical character, which enables us strongly to recommend it to the attention of our readers.



ART. VII.—*Atalectasis Pulmonum; or Closure of the Air-Cells of the Lungs of Children*. By GEORGE A. REES, M.D.—London, 1850. Post 8vo, pp. 42.

THIS pamphlet consists, for the most part, of a reprint of observations which the author has at different times made public through the medical periodicals; and he mentions "the little attention the subject has received from the profession" as his reason for thus reproducing them. We believe, with him, that cases of Atalectasis are very frequently met with, and very commonly misunderstood; and any attempt, therefore, to call the attention of practitioners to the subject deserves commendation. Having in this number (p. 155 et seq.), as well as on a former occasion (vol. III, p. 424), brought it under the consideration of our readers, we need say no more, than that we believe Dr. G. A. Rees to have been among the first, if not the very first, to point out the occurrence of Atalectasis as a disease subsequently to the period immediately succeeding birth; his first paper on the subject having appeared in the 'Medical Gazette' more than eleven years ago. We cannot but think that a more complete treatise, based on the subsequent researches of MM. Rilliet and Barthez, MM. Bailly and Legendre, Dr. West, Dr. Fuchs, and others, with the experience of Dr. Rees himself, would be welcomed as an acceptable addition to our professional literature.

ART. VIII.—*A Universal Formulary; containing the Methods of Preparing and Administering Official and other Medicines. The whole adapted to Physicians and Pharmacutists*. By R. EGLESFIELD GRIFFITH, M.D.—Philadelphia, 1850. 8vo, pp. 567.

THE design of this work, as we are informed by its author, is to present a compendious collection of formulæ and pharmaceutic processes, derived from various sources; the principal aim being to select materials most generally applicable, and of practical utility. It contains also a number of very useful tables, together with an explanation or vocabulary of the principal abbreviations and Latin terms used in prescriptions, general rules regarding the administration of medicines, and a short chapter which seems to us out of place in such a work, and which is too concise to be of much use, on the management of the sick-room.

The formulary is arranged alphabetically, according to the pharmaceutic names adopted in the United States Pharmacopœia, the several preparations being found under the scientific appellation of their principal ingredient, as in Dr. Christison's 'Dispensatory;' but each formula being designated by its English name, the English appellations for the articles composing it being used, and the quantities of these ingredients being expressed in words, and not in the usual pharmaceutic signs. The reason assigned for this change is the occurrence of mistakes, which are affirmed to arise out of the orthodox method; we are by no means sure, however, that such an advantage balances its obvious disadvantages; and a better education on the part of the dispenser would enable him to read the "abbreviated cabalistic terms" now in use, with as much ease as the full-length vernacular. No such alteration can afford any security against

*carelessness* on the part of the compounder; and this we take to be the chief source of errors in dispensing, such as the plainest and fullest directions on the part of the prescriber cannot avert.

The vast collection of formulæ which is offered by the compiler of this volume contains a large number which will be new to English practitioners, some of them from the novelty of their ingredients, and others from the unaccustomed mode in which they are combined; and we doubt not that several of these might be advantageously brought into use. The authority for every formula is given; and the list includes a very numerous assemblage of Continental, as well as of British and American writers of repute. It is, therefore, a work to which every practitioner may advantageously resort for hints to increase his stock of remedies and of forms of prescription.

To the 'Formulary' are appended various other useful matters, such as lists of incompatibles, a table of doses, a table of synonymes in the Pharmacopœias of London, Edinburgh, Dublin, and the United States; general directions for the officinal preparations; a general view of the actions, treatment, and analysis of the most important poisons; and then a series of indices, the first of which is quite a new feature (if we mistake not) in a work like the present, being an index of remedies for all the principal diseases to which flesh is heir, arranged alphabetically under their several names. Thus under *Amenorrhœa*, which stands near the beginning of the list, we have the following, each having the proper reference to its page, which it is not needful for us to insert:—

"Hiera picra, and other preparations of aloes. Injection ammonia. Muriate ammonia. De Haen's pills. Iodide calcium. Cantharides and savine. Infusion dittany. Metallic iron. Bolus metallic iron. Ammoniated iron. Ointment bromide iron. Pills carbonate iron. Mixture chloride iron. Lozenges iodide iron. Ointment iodide of iron. Black oxide iron. Phosphate iron. Sulphate iron. Hooper's pills. Compound galbanum pills. Compound infusion gentian. Compound tincture guaiacum. Ammoniated tincture guaiacum. Infusion pennyroyal. Bacher's pills. Tincture black hellebore. Compound pills iodide mercury. Saturated tincture iodine. Clyster iodine. Muriate manganese. Horehound. Myrrh and sulphate iron. Tincture myrrh and hellebore. Madder. Decoction madder. Savine and ginger. Savine pills. Oil savine. Compound infusion senna. Mixture of borax." (p. 495)

Fancy a young practitioner, desirous of giving his patient every chance of cure, commencing at the beginning of such a list, giving a week's trial of each remedy, and going on from one to another, until his patient might either get well, or become tired of his polypharmacy, or peradventure get worse instead of better in spite of his medicaments. Such, we apprehend, is the kind of indiscriminating trust in remedies, which such a guide may tend to encourage; and we would earnestly beg all who may employ Dr. Griffith's 'Formulary,' to consult this index as rarely as possible, and then never to use it as a guide to practice, but merely as supplying hints, some of which may be used with advantage. The other indices facilitate reference to every article in the 'Formulary;' and they appear to have been drawn up with the same care as that which the author has evidently bestowed on every part of the work.

ART. IX.—*Pathological Researches on Death from Suffocation and from Syncope; and on Vital and Post-mortem Burning. Suggested by the Case of the alleged Bridgnorth Matricide.* By SAMUEL WRIGHT, M.D. LL.D. D.C.L., Professor of Clinical Medicine, in Queen's College, Birmingham, &c. &c.—London, 1850. Small 4to, pp. 34. With Three Plates.

THE special purpose which Dr. Wright had in view in this publication, seems to have been, in the first place, to examine whether, as asserted by Dupuytren, death can take place instantaneously from the effect of extensive burns, and yet congestion of the principal viscera be subsequently found; and, secondly, to inquire whether any discriminating marks can be proved to exist, between post-mortem burns and those inflicted on the living body.

In reference to the first of these questions, the author brings together a mass of evidence, derived from his own observations as well as from the experience of others, to establish the entire difference in the post-mortem appearances that present themselves in those who have died from *asphyxia* and those who have died from *syncope*; the former being especially characterised by the turgescence of the right cavities of the heart; and the latter by the comparative if not entire emptiness of its cavities. He concludes, therefore, that if death from burning were sudden and syncopal, there would be no venous turgescence of the heart; and that the presence of such turgescence, especially if strongly marked, is decidedly indicative of death from asphyxia.

On the second point, Dr. Wright affirms, that a burn can never take place in a *living healthy subject*, without a red line of demarcation between the injured and the uninjured parts; although the application of heat, acids, &c. may fail to produce anything else than a whitening and loosening of the cuticle, when there is a very depressed state of the vitality of the surface. The existence of vesications containing serum has been held to indicate that the burning took place during life; but Dr. Wright has experimentally proved, by the application of the flame of a spirit-lamp to the skin, that a serous blister *may* be raised after the cessation of the general circulation, provided that there be a certain amount of vitality still persistent in the tissues. Most of his trials have been made upon limbs just amputated; in which he has found that serous vesications may be produced within from half a minute to four minutes and a half after their separation from the body, the vesications brought out beyond that time being gaseous only. But he has succeeded in obtaining serous vesications on the dead subject, in cases which he did not regard as particularly favorable, as long as half an hour after death; and in one instance, serous vesications resulted from the application of the flame three hours and a half after death. In this last instance, the blisters did not rise until after the lapse of an hour subsequently to the application of the heat; they were filled by a fluid of a pale straw-colour, and readily coagulable by heat; but neither around nor beneath them was there the least shade of redness.

The facts thus ascertained by Dr. Wright support the opinion which he gave on the trial of Mercy Newton, that the appearances in and upon the body of the deceased were indicative of death by asphyxia rather than by burning, and of the post-mortem production of the marks of burning.

The history of this criminal procedure is altogether too remarkable to be passed by without notice. No fewer than *nine* Coroner's Juries sat upon the case successively, and were successively discharged without being able to agree upon their verdict; and the prisoner was consequently committed for trial upon a magistrate's warrant alone. She was arraigned at the Shrewsbury Assizes in March 1849, before Mr. Justice Coltman; but the jury was discharged, not being able to agree upon a verdict. She was again arraigned in July of the same year, before Mr. Baron Rolfe; when the same result occurred. She was a third time arraigned at the Spring Assizes of the present year, before Mr. Justice Patteson, and pronounced "not guilty." Dr. Wright's treatise was ready for publication previously to the last trial; but he kept it back out of fairness to the accused. As a doubt had been thrown, however, upon the validity of the opinions which he advanced on the former trial, it would have served the ends of justice, if he had been furnished with an opportunity of fortifying these opinions by the detail of his subsequent experience. He was not called as a witness, however, on the third trial.

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ART. X. — 1. *A Practical Handbook of Medical Chemistry.* By JOHN E. BOWMAN, Fellow of the Chemical Society, and Demonstrator of Chemistry in King's College, London. Fcap. 8vo, pp. 280.

2. *A Practical Text-Book of Inorganic Chemistry, with Qualitative and Quantitative Analysis.* By DUGALD CAMPBELL, Demonstrator of Chemistry in University College.—London, 1850. Fcap. 8vo, pp. 392.

MR. BOWMAN's object in publishing the little volume now before us has been (to use his own words) "to supply a book that will be found useful, not only to the medical student, but also to the practitioner, to whom the value and importance of the applications of modern chemistry and microscopic analysis to his art, are becoming daily more and more apparent." He lays no claim to originality; and as, moreover, all physiological considerations are rigidly excluded, our sole duty appears to be to decide on the merits of the book as a guide to a particular department of general chemistry.

In the first hundred pages, which are devoted to the consideration of the urine, and which we have examined more carefully than the remainder of the work, it strikes us that the mode of arrangement is capable of very considerable improvement. We will take as an illustration of our meaning, the subject of *Urea*. Section II of the First Chapter is devoted to this substance: we are there told how its presence in the urine may be shown, and how it may be obtained in a state of purity from the urine; its crystalline form and chemical characters are next described; the proportion of urea in 1000 parts of healthy urine is stated, and the oxalate and nitrate of urea are duly noticed and correctly figured, the whole occupying a little more than three pages. Some twelve pages further on, we find six paragraphs (52-56) describing the quantitative determination of urea in healthy urine; and after an additional twelve pages, we arrive at paragraph 87, on "urine containing urea in abnormal quantity." But even now we have not exhausted the miscellaneous notices of this substance; for paragraphs 181-184 are devoted to the "examination of urine suspected to

contain urea in abnormal quantity," and paragraphs 301-3 to very much the same subject. This method (we may almost say, *want*) of arrangement not only gives rise to much confusion in the mind of the reader (unless indeed he is previously acquainted with animal chemistry, and in that case he would hardly select a mere manual as his guide), but renders the author extremely liable to the risk of making apparently contradictory remarks. Thus, for instance, in paragraph 10 we read, that "the proportion of urea present in *healthy* urine appears to vary from twelve to upwards of thirty parts in 1000," a statement with which the concluding part of paragraph 87 is hardly consistent, viz. that "*in disease* it often amounts to thirty parts, or even more." The italics are our own.

With the exception of the mode of arrangement, there is little with which we have to find fault. We may, however, remark, that the most recent chemical investigations render it very doubtful whether the statement in paragraph 19, that uric acid "probably exists for the most part in combination with ammonia," is correct, the present idea being that it exists as urate of soda; that on reading paragraphs 33, 662, and 678, and on referring to fig. 70, we cannot see what are the different results that follow the application of acetic acid to pus and mucus corpuscles; and that the table in paragraph 279, "showing the amount of solid matters and of water in urine of different specific gravities," is due to Drs. Christison and Day, rather than to Dr. G. Bird, to whom our author assigns it.

The Second Part treats of *Calculi and Concretions*, and is judiciously executed. The Third Part gives a very brief view of the chemistry of *Healthy and Morbid Blood*; and the Fourth treats of *Milk, Mucus, Pus, and Bone*; concluding with a chapter on the *Examination of Mixed Animal Fluids*. There is nothing in any of these departments to call for special comment; and indeed the same observation equally applies to the Fifth Part, on *The Detection of Poisons in Organic Mixtures, &c.*

While we trust that Mr. Bowman's unassuming little volume will not supplant the larger and more complete treatises of Simon, Marchand, and Lehmann,—works with whose contents every physician of the present day should be familiar,—we can conscientiously recommend it as, on the whole, fairly carrying out the objects for which it was written, and as a safe guide both to the student and to the practitioner in the simpler processes of animal chemistry.

Mr. Campbell's little volume, which is designed for the use of students attending practical classes, holds very much the same relation to inorganic chemistry, that Mr. Bowman's does to organic; being intended to afford elementary instruction in the facts of the science, to those who have not time or inclination for taking a more comprehensive survey of it. Each element and its principal compounds are treated of in the following manner:—1. The methods employed for preparation. 2. The general appearance and properties of the substance. 3. The characteristic qualitative tests. 4. The quantitative estimation and separation from other substances. This methodical arrangement of the topics to which the students attending a practical course of instruction in Chemistry will necessarily have their attention chiefly directed, will doubtless render the work very useful as a laboratory-companion; but we hope that none will rely upon it as teaching them all they require to know of the principles of the science.

## PART THIRD.

## Periscope.

## ANATOMY AND PHYSIOLOGY.

*Some Observations on the Contractile Properties of the Blood-Vessels and Lymphatics.*  
By Professor KÖLLIKER.

PROFESSOR KÖLLIKER gives in this paper a number of experiments on the vessels of an amputated limb, as a sequel to those performed by him on several placentæ, for the details of which he refers to a dissertation by Dr. Wild.

The leg of a boy, aged 15, was amputated above the knee, for necrosis of the os femoris. The limb was subjected to a series of experiments by means of an electromagnetic apparatus, from the moment of its removal at 25 minutes past 10 a.m., until 12 o'clock.

1. The vena saphena minor was touched in the fossa poplitea, on the lower part of the leg and on the foot, the vena saphena magna on the lower part of the leg and dorsum pedis. A few seconds after the application of the wire, contraction took place; at the end of a minute the parts touched contracted so much, that the blood they contained in large quantities was pressed out, until the vessel had the appearance of a white cord. On smaller veins of the skin, the effect was not so rapid or so powerful.

2. Three applications of the wire had no effect on the vena poplitea, but it was already very flaccid and empty before the experiment. The vena tibialis postica was emptied by contraction in a minute.

3. The almost empty arteria poplitea contracted a little after the wire had been applied for two minutes. The effect was greater on the arteria tibialis postica.

4. Five well-filled lymphatics on the skin of the dorsum pedis,  $\frac{1}{4}$  —  $\frac{3}{8}$ " in diameter, contracted powerfully in the space of  $\frac{1}{2}$  to 1 minute; the lumen of the vessel did not entirely disappear, but it was diminished at least one half.

5. No effect was produced on the skin of the shin.

6. The effects on the nerves and muscles were the same as is usually described; the irritability remained longest in the muscles.

Duration of irritability in the veins	.	.	.	1 hour 15 minutes.
"	"	arteries	.	1 " 10 "
"	"	lymphatics	.	1 " 12 "
"	"	muscles	.	1 " 35 "
"	"	nervus ischiadicus	0	" 35 "
"	"	nervus tibialis posticus	0	" 45 "

In the first four the irritability still persisted, when the experiment was interrupted at 12 o'clock, and existed in the vessels in a powerful degree. The writer does not consider these experiments as in every respect conclusive, but urges that they prove the existence of contractility in the coats of the blood-vessels and lymphatics, and he promises to carry his inquiries further.—*Kölliker and Siebold's Zeitschrift*, 1849.

*Neurological Observations.* By Professor KÖLLIKER.

In these observations, the writer calls attention to the ganglion-cells, and the fibres arising from them. In doing so, he casts a retrospective glance at his discoveries in 1845, as to the connexion of true nervous fibres with the processes of the ganglion-globules; at which time he declared the pale processes of these globules to be all true nervous fibres, and considered that those with free ends serve to connect different parts of the nervous system with its centre, acting as *nervi nervorum*. Some time after this, Robin and Wagner discovered the origins of several nervous fibres from one globule. Wagner having since seen ganglion-cells with double fibres, doubts the possibility of only one fibre arising from them, and in consequence does not admit Kölliker's supposition, that the ganglia send fibres to the periphery like central organs. Though the writer does not consider the fact of the nervous branch issuing from the ganglion being increased in size, as a proof of the origin of nervous fibre (since as he thinks it may proceed from other causes, such as a division of the nerve in the ganglion), he is entirely in favour of the single origin of fibres from the ganglion, and considers it to be the rule in the mammalia. On the other hand, he cites several writers to prove the infrequent occurrence of a double origin of fibres from the ganglion-cells giving origin to single fibres; such are those of Volkmann and others, but more especially of Engel. This anatomist saw a small pear-shaped ganglion attached to a bundle of nerves in the perichondrium of a cartilage of the trachea; it contained fourteen ganglion-cells, and sent off from its pointed end a bundle of nerves to join the larger bundle to which it was attached. This smaller bundle contained seven fibres, one of which proceeded from a ganglion-cell, while the others seemed in close connexion with the covering of the ganglion. No nerves entered the ganglion.

Professor Kölliker points out that in fishes a number of cells exist, in which two fibres insert themselves; but that in the higher classes of the vertebrata, double origin is the exception, and single origin the rule. In reply to the objection that the other fibre might have been broken off in the cells he examined, he says that in such a case the place of its insertion at least would have been seen. In the brain and spinal cord there are:—1. Nervous cells with one and two processes. 2. Of these first, some certainly end in dark-edged nervous fibre. 3. There are nerve-cells with two or more processes, which decidedly do not end in peripheric nerves. Nerve-cells with single processes are frequent in the brain and spinal cord; those with double processes exist there as well as in other parts of the bodies of vertebrated animals. The ganglion-cells of the spinal cord of the frog send off double or single pale processes, which sometimes are on the same, sometimes on the opposite side. In some cases they become darker, and assume the characters of real nervous fibre; but this was only observed in those with single fibres. The writer has found nerve-cells with no fibres as the component parts of ganglia; he calls them independent ganglion-globules. They are not only found in the central organ and spinal cord, but also in the ganglia of the sympathetic and cerebro-spinal nerves, and are of such frequent occurrence that it is rather a question if a ganglion exists without them. They are best seen in the heart of the frog, the *G. gasseri*, and the *G. spinalia*. They may be considered as elementary cells, being composed of a membrane, granular contents, a vesicular nucleus, and a nucleus-corpuscle.

The pale processes of the ganglion-globules change their appearance and become dark-edged and nervous tubes. In touching on this point, Professor Kölliker enters into a long explanation and defence of his views on this subject in opposition to those of Bidder. He maintains that the sheath of the primitive nervous fibre is the continuation of the covering of the ganglion-cell; and that the contents of the ganglion-cell and its pale process are in immediate connexion with the contents of the primitive fibre. Bidder affirms that the ganglion-globules lie in the enlargements of the nervous tube. The whole question turns on this point;—are the contents of the enlarged nerve-tube to be considered as ganglion-cells, or is the whole enlargement, its contents, and the cell-wall adhering to and continuous with the nervous sheath, to be considered as such? Kölliker pronounces in favour

of this last opinion; and enters into a long discussion to support it. With regard to the relation of the fine to the coarser nervous fibres, he says, that there is no appreciable difference between them in an anatomical point of view. The fine fibres of the sympathetic do not differ from the thicker cerebro-spinal nerves. Volkmann and Purkinje have considered the fine fibres of the brain and the nerves of the higher senses as forming a third kind of fibre, on account of their tendency to form varicosities and of their excessive fragility. Kölliker does not consider them as differing essentially from other nervous fibres. The coarser fibres in their peripheric expansions become so fine, that their ends resemble the fibres of the sympathetic. In all instances where nervous fibres divide, fine branches are given out, which have the same characters as those of the sympathetic. All fibres under 0.003" have simple outlines, and entirely the appearance of those of the sympathetic. All the fibres in the embryo without exception, during their development, are very fine, and partake of the characters of the sympathetic fibres; but subsequently assume those of the cerebro-spinal nerves. The writer is most decidedly persuaded, that the fibres of the sympathetic system arise from the sympathetic ganglia, from the ganglia of the cerebro-spinal-nerves, and in all probability from the brain and spinal cord. With regard to the ganglia, he says, 'we know that they send forth more fibres than they receive, and I have proved that increase of the branch of exit is caused by the fine fibres arising singly from the ganglion-cells; this is the rule, but sometimes two fibres arise from them.' The writer thinks that it is quite justifiable to admit the origin of fine fibres from the medulla, in consideration that the roots of the cerebro-spinal nerves contain a large number of them, which enter the sympathetic by the rami communicantes, and because he has observed the origin of fine fibres from the nervous cells of the medulla of the frog. As no satisfactory conclusion can be arrived at concerning the origin of the fine fibres of the sympathetic, he thinks that the best way to come to some sort of unity on the subject is to distinguish the fine fibres arising from the sympathetic ganglia as fine ganglion-fibres, those arising from the medulla spinalis and oblongata as fine medullar fibres. He does not agree with Bidder and Volkmann with regard to the distribution of fine fibres; nor does he lay much stress on the method of counting them (*Zählungen*) adopted by Volkmann. On this point, he says that it may throw some light on the mode in which the fine fibres arising from the spinal-cord and ganglia are distributed; but it does not explain the mode of distribution of fibres arising from other parts, because we cannot distinguish the fine fibres arising from the sympathetic ganglia from those coming from the spinal ganglia and spinal cord. He cannot admit that the fine fibres alone are the agents of organic functions, as the coarse fibres of the nerves of the spinal cord agree with them in many points: he therefore believes that the coarse fibres are not alone agents of the will and of sensation. The presence of coarse fibres in a nerve proves to him that it is an agent of the brain as well as of other centres; whilst the existence of fine fibres alone proves that the influence of the spinal cord and ganglia is active, but that the agency of the brain, as far as it is shown in voluntary movement, does not exist. The writer thinks that we are not justified in ascribing other functions to the sympathetic fibres, than those of the ordinary sensory and motor cerebro-spinal nerves. They are fibres of sensation and movement; and bear the same relation to their central organ as the cerebro-spinal fibres do to theirs. When the centre is irritated, movement takes place in the contractile parts they supply; when affected at their periphery they irritate their centres. He most decidedly rejects the idea that the fine fibres have any influence on nutrition and organic metamorphosis; and sees no grounds whatever to admit of a physiological difference between the fine fibres and those of the cerebro-spinal nerves. But this does not apply to the central organs from which they arise. It is possible that the part of the medulla from which coarse fibres arise has other functions than that in which the fine fibres take their origin, and again differs from the ganglia, the chief sources of fine fibre. Therefore:

1. The fine fibres, in an anatomical point of view, distinguish themselves from the coarse fibres, (which all arise from the spinal-cord and distribute themselves in



parts capable of voluntary motion and sensibility,) by having their origin partly in ganglia and partly in the spinal-cord, also by their distribution.

2. In a physiological point of view, no fact exists which forces us to attribute other powers or energies to the fine fibres than to other fibres of movement or sensibility: on the other hand, inasmuch as their central organs possess peculiar powers, the coarse fibres serve to produce effects different from those of the fine fibres.—*Kölliker and Siebold's Zeitschrift*, 1849.

*On a new Form of Smooth or Non-Striated Muscular Fibre.*

By Professor KÖLLIKER.

KÖLLIKER describes the smooth muscles as composed of short, isolated fibres, each containing a nucleus. He calls them muscular or contractile fibre-cells, and gives three varieties:

1. Short, round, spindle-shaped, or rectangular plates, like those of epithelium, 0·01" long, and 0·006" broad.

2. Long plates of irregular rectangular, spindle or club-like shape, with fringed edges, 0·02" long, and 0·003" broad.

3. Narrow, spindle-shaped, round, or flat fibres, with fine ends, which are either straight or wavy, 0·02" long, or even 0·25" long, and 0·002" to 0·01" broad.

The first and second of these forms are only to be found in the walls of vessels, the first may be mistaken for the cells of epithelium.

These muscular fibre-cells are composed of soft light yellow substance, which swells in water and acetic acid, in which last it becomes of a paler colour. There is no appreciable difference between the outer and inner parts, though in acetic acid it would seem as if each fibre-cell had a delicate covering. Their substance is homogenous, with longitudinal stripes; and they often contain small pale granules, sometimes yellow globules of fat. Each fibre-cell has without exception a pale nucleus, sometimes only perceptible in acetic acid. Its form is peculiar, being like a small staff rounded at each end. The substance of the nucleus is homogenous; its length is 0·006"—0·004", its breadth 0·0008"—0·00013". The muscular fibre-cells lying side by side, or end to end, form the smooth muscles as they appear to the naked eye. They may be divided into:

1. Purely smooth muscles containing no other tissue: such are those of the nipple, corium, of the interior of the eye, of the intestines, of the perspiratory glands of the axilla, of the cerumen glands of the ear, of the bladder, of the prostate, of the vagina, of the small arteries, of the veins and lymphatics.

2. Mixed smooth muscles, which contain, besides the muscular fibre-cells, cellular tissue, nuclear fibre, and elastic fibre: such are the trabeculae of the spleen and corpora cavernosa of both sexes. They are also found in the tunica dartos, gall-ducts, the fibres of the trigonum vesicæ, the circular fibres of the larger arteries and veins, the long and transverse fibres of the prostata, urethra, Fallopian tubes, and of the womb; they change by imperceptible transitions into the first form; this is the case in the trachea, bronchi, urethra, the inner muscular layer of the testicles, seminal ducts, &c.

Kölliker says, that he has found smooth muscles in the skin to a far greater extent than is generally supposed. In the subcutaneous cellular membrane of the scrotum, penis (prepuce), and the anterior portion of the perineum, they are well developed. The greater number seems to exist in the tunica dartos; in the perineum and prepuce there are fewer. In the tunica dartos they form a muscular coat resembling, on a small scale, the tissue of the bladder. In the nipple and areola (especially in the female) the smooth muscles are strongly developed, somewhat resembling those of the tunica dartos, but having no fibrous covering. In the areola, up to the base of the nipple, they are arranged in circular order; in the nipple they are circular and vertical, the ducts passing between them. Some lie in the corium

and form the corpus reticulare; others belong to the subcutaneous tissue. Smooth muscles are also found in every part of the body covered with hair, in the hair-bulb, and in the upper portion of the corium. In the parts not covered with hair, such as the palm of the hand, the smooth muscles are wanting. One or two bundles of muscular fibre encircle each hair-bulb or sebaceous gland. Kölliker remarks, that the tensor choroideæ does not insert itself into the processus ciliaris, but that it lies flat on its anterior surface, and that it arises from the canalis schlemmii. The sphincter pupillæ, he says, may be easily seen in the eye of the white rabbit, and in the blue eye in man, on removing the uvea. In man it is  $\frac{1}{4}$ " broad, and forms the pupillar edge of the iris. He has also observed a muscular ring near the annulus iridis minor. The dilator pupillæ does not form a continuous membrane, but seems to consist of isolated bundles of fibres passing between the muscles to insert themselves in the edge of the sphincter. He has never seen the anastomosis of these fibres mentioned by Todd and Bowman. The writer thinks that the elements of all these muscles are smooth muscular fibre, though he admits that he has seldom succeeded in isolating the muscular fibre-cells in the human body. He does not think that the *M. cochlearis* discovered in the ear by Todd and Bowman deserves the name of a muscle; he is rather disposed to consider it as a ligamentous structure, and calls it the ligamentum spirale; he looks upon it as a means of attachment for the zonula membranacea. Remarking that the smooth muscles of the intestines resemble one another in their histological characters, he points out one peculiarity, viz., that they present a knotty appearance with ends running out into fine spirals. He thinks that it is not improbable that the knots are due to a contraction of the fibre. The fibre-cells of the intestine seem to be striped, as if they were composed of an envelope and some homogenous striped contents. No nuclear fibre is found among them, but they are covered and bound together by cellular membrane.

The small perspiratory glands seldom possess smooth muscular fibres, although these are always present in the large perspiratory glands of the axilla, and in the cerumen glands of the ear.

Kölliker does not admit the presence of muscular fibre in the lacteal glands.

In the lungs he finds that the structure of the small and large bronchi is the same. Outside of the epithelium they present a layer composed of longitudinal fibres of areolar tissue, and a number of strong, fine, elastic fibres. Then follow one or more circular layers of smooth muscular fibre, with some nuclear fibre running transversely; lastly, a layer of cellular tissue, with nuclear fibre. He never could find muscular fibre running longitudinally through the bronchi. With respect to the vesicles of the lungs, he could come to no satisfactory conclusion. Long nuclei are seen in the walls of the vesicles, but they are not so long and narrow as those of the smooth muscles, and appear to him to belong to the capillaries. The smooth muscles of the trachea and bronchi resemble in their elements those of the intestines. In the ox, the gall-bladder, the ductus cysticus, d. choledocus, and the ducts lying out of the substance of the liver, present a large amount of muscular fibre of the smooth species. It is strongly developed in the canals, in which it is so disposed longitudinally; in the gall-bladder this is not so much the case, a transverse, and even an oblique layer of fibres being placed between the two longitudinal layers. In the human body, the muscular structure is very faintly developed in the gall-ducts. Kölliker could only discover a very delicate layer at all approaching muscular fibre. In the pancreatic ducts of the human body, no trace of muscular fibre exists. In the lacrymal apparatus there are no muscular fibres; in the ductus stenonianus none; the ductus whartonianus has a very faint layer of smooth muscular fibre.

No part of the internal structure of the kidney shows traces of muscular fibre; it is only in the calices and pelves that it becomes apparent. The muscular fibres of the pelves and calices are composed of an outer longitudinal coat, and an inner transversal layer; they are continuations of the same in the urethra, and all partake of the general characters of smooth muscular fibre. Supposing the disposition of the muscular fibres of the bladder to be well known, the writer observes that the trigonum vesicæ consists of a pretty strong layer of pale yellow fibres immediately

most ferociously inimical to man's constitution; and I can conceive of nothing more wonderful than the fact of his having paid so little attention to this circumstance, or than that of its having been so marvellously concealed from his observation." (p. 48.)

"There is a very remarkable resemblance between the effects produced on the human constitution by salt, which is a chloride of sodium, and those caused by calomel, which is a chloride of mercury; accordingly that pernicious agent, like salt, is wont to cause many of the symptoms of scurvy, and also to induce consumption, and wonderfully to exasperate the disease, and expedite its progress when it is already in existence." (p. 49.)

Having assumed that the use of salt in large quantities is the essential cause of scurvy, Mr. Howard continues:

"When salt is more gradually introduced into the body, as in the way in which it is commonly taken with the food, it is evidently the direct cause of consumption or decline; it is clearly shown to be so by the circumstance that the conservative powers during that disease are constantly occupied in the expulsion of salt acrid phlegm from the lungs, by which they are at first irritated and excoriated, and then fall into an ulcerated state. .... Consumption is a milder form than scurvy of what may be justly called the plague of salt."—(p. 50.)

From these views it of course follows that abstinence from salt will wonderfully facilitate and expedite the cure of diseases,—a result which we are gravely assured by Mr. Howard, he has constantly observed by experience. The benefits of this system are most evident in the treatment of consumption:

"When, by abstinence from salt and salted meats, the constitution has become sufficiently freed from its influence, the irritation and excitement peculiar to consumption cease in a most remarkable manner; the cough and expectoration are greatly abated, and the condition of the patient is altogether wonderfully improved; as may be observed by any consumptive or catarrhal person making the simple experiment. By such means, with the aid of judicious medical treatment, that disease, if it have not too far undermined the constitution, is easily and permanently cured." (p. 255.)

If any of our readers wish to see how plausible the strangest hypothesis may be made to appear, when all the facts and suppositions that can be made to tally with it are heaped together, and everything that can be stated on the opposite side is carefully kept out of view, we recommend them to glance over Mr. Howard's pages. We have no doubt that he will make many converts; and that the anti-saline treatment (if he plays his cards properly) will take its place among the profitable empiricisms of the day. We would not rank Mr. Howard, however, with the impostors who make their market of human ignorance; for it is obvious that he most devoutly believes his own doctrines, and thinks that he has conferred a blessing on mankind by the publication of them, and by the corroborative testimony he has adduced from "the wisdom of the ancients." How is such a man to be treated? By fine and imprisonment? Surely not; but by enlightening the public—provided *he* cannot be convinced—as to the need for salt in the animal body, and the proved consequences of its withdrawal. Mr. Howard seems to place much confidence in the indications afforded by the natural habits of animals; did he never hear of the "salt-licks" of the United States, which are the resort of herds of buffaloes that are drawn to them at intervals by the craving they experience for this article? Or did he never hear of the evils which have arisen out of "salt

monopolies" in European states, and, in more recent times, in British India? No sensible person who examines into such evidence, can doubt for a moment that salt is a necessary of life; and that, if *totally* deprived of it, the most healthy body must speedily become diseased.

As a sample of the character of Mr. Howard's mind, which will afford some amusement to our readers, we shall present them, in conclusion, with his solution of the long-disputed problem of the purpose of the Egyptian pyramids:

"The ancient Egyptians raised the pyramids in order that they, like the stony mountains, might, on the occasion of the next terrestrial conflagration, become ignited and cause the fire to devour the stone of those regions of the country, where the earth did not naturally present sufficiently elevated summits for that purpose, and for the fulfilment of which other circumstances were not favorable; so that their country might by that means become covered with fertile spots, which would not otherwise have happened. It is possible, and even likely, that the Egyptians may have observed that some of the fruitful oases in the Libyan deserts had been so produced, by artificially built mountains. I think it extremely probable that such evidence may still continue, in the neighbourhoods of existing oases, where there are probably pyramids not entirely consumed; as in various parts of the world are found stony mountains which have been partially melted by fire. The Egyptians did not want that instruction; but it is likely they knew that such had been. I have repeatedly described that the combustion of stone cannot go on to any very great extent without an abundant supply of water. Accordingly the Egyptians have, with wonderful ingenuity, made provision whereby the fire should be supplied with plenty of water. Those pyramids which have been entered are found to contain vast cisterns and reservoirs of water," &c. &c. (p. 205.)

Which "fully accounts for the milk in the cocoa-nut," as the showman hath it. Verily "the force of ——— can no further go." Let our readers supply the hiatus with any word of two syllables—such as wisdom or folly, learning or nonsense—which seems to them most apposite to the preceding extract.

ART. VI.—*Practical Ventilation as applied to Public, Domestic, and Agricultural Structures. Being an elucidation of Plans and Suggestions, of easy application, for Ventilating every species of Architectural Structure; with Remarks on Heating, Construction of Fire-places, Cure of Smoky Chimneys; and an Appendix on the Ventilation of Ships, Steam-boats, and Railway-carriages.* By ROBERT SCOTT BURN, Engineer.—*Edinburgh*, 1850. Post 8vo, pp. 208. With numerous Illustrations.

THE object of this little work is sufficiently explained in its title; and we have only to speak of its execution. The author exhibits a full acquaintance with the physiological principles on which the necessity for ventilation depends; and has evidently given great attention to the various plans for accomplishing it, of which so many have been promulgated within the last few years. We are not aware of any important suggestion which has escaped him; and in throwing together the results of his own experience, and in explaining the methods of others, he has given to his little work an eminently practical character, which enables us strongly to recommend it to the attention of our readers.

ployed. Thus, of five rabbits, the first exposed to 245° F. died in 7 minutes, and the last, exposed to 140°, in 33 minutes. Three dogs, exposed respectively to 212°, 194°, and 176°, died in 18, 24, and 30 minutes. The same rule applies to birds, but they bear an elevated temperature for a shorter period than mammals. Cold-blooded animals endure it longer. Thus, frogs exposed to 176° or 212°, lived for half, three-quarters, or a whole hour.

The first remarkable circumstance to be observed is the enormous *loss of weight*. Thus, rabbits exposed to 140° or 212° lost about 15 grains per minute, and the same was observed in respect to the mammals and birds. The loss was not so much in proportion to the degree of elevation of temperature, as to the duration of a high one; and it is dependent more upon pulmonary transpiration (produced by the accelerated respiration) than cutaneous. Thus, a rabbit so placed in the heated stove that its head was excluded, lost only 10 grammes in weight; while another, in the same time, so placed that his head was within the stove, and his whole body without it, lost 25 grammes.

The *increase of temperature* of animals exposed to great heat is an important phenomenon. A living rabbit, having a temperature of 102°, and one just killed with a temperature of 87°, were placed in the stove at 176°. At the end of 20 minutes the former died convulsed, having acquired a temperature of 112° in all parts of its body; while the dead one exhibited a temperature of 122° to 125½°, externally, and but of 107½° in the rectum. Thus, while the living animal dies long before an equilibrium can be established, that is gradually effected in the dead one, as in other inert matter. The experiments were so frequently repeated as to render it certain that mammals always died when a temperature of 113° or 115° had been attained. Birds bore an increase to about 118½°, and cold-blooded animals, which soon establish an equilibrium at a medium temperature (68° to 77°), when exposed to that of 176°, died when their own reached 104°.

The following are the changes constantly found in animals that have succumbed to a high temperature:—The blood is found black in both arteries and veins. It forms, with difficulty, a dark and diffuent coagulum, while the serum is rendered turbid by the globules it holds in solution. Owing to this state of the blood, ecchymoses are found on the skin and mucous membranes. Bloody stools are present, while the lungs, liver, and especially the kidneys, are infiltrated with blood.

If the experiments are repeated in *moist* heated air, the effect is far more rapid; the animal attaining the excess of temperature, beyond which its life cannot be prolonged, far more speedily, owing to the vaporous air being a better conductor than dry air. Comparative experiments with hot water, for the same reason, yielded a yet more rapid result.

2. *Effects of Low Temperatures.* While an elevation of the temperature of animals is not borne beyond 8° or 10°, it may be lowered to a far greater extent. Thus, dogs and rabbits exposed to a freezing mixture, their own temperature being 104°, and in some instances artificially raised to 107½° or 109°, lost 5½° in 10 minutes, 11° in 15 minutes, and 12½° in 20 minutes; the loss in one example amounting to 36° in 40 minutes. The general conclusions to be drawn from numerous experiments are: 1. An animal placed in a temperature of from 32° to 46°, during a period not exceeding 5 minutes, undergoes a diminution that may lower it to 2-3ds of its normal temperature. 2. This diminution continues to go on, even after the animal has been withdrawn from the cooling mixture. 3. Left to itself, such diminution takes place, until, having arrived at nearly the half of the normal temperature, the animal dies. 4. If this point have not been attained, the application of warmth may yet restore the normal temperature.

To procure the fatal diminution of the temperature of an animal, it is not necessary to plunge it into a freezing mixture. It suffices to wet its surface continually with alcohol, ether, or cold water (even the wetting it with, or immersing it in warm water, will attain the same end), the effects varying according to the degree of diminution of temperature attained, and the conditions in which the animal is afterwards placed. That the lowering of the temperature is not exclusively produced by the evaporation of moisture adhering to the skin, is seen in the fact, that

the same phenomena are observed after immersion in mercury. However we may explain the circumstance, we must not lose sight of the practical applications. Thus we see, that even warm baths are effectual means of diminishing internal heat; and if we wish to produce the full effect, the person must not be dried or rubbed too soon after leaving them, lest a premature reaction interfere with their refrigerant effect. In the recovery of drowned persons, too, the restoration of the lost temperature, by the administration of warm fluids by the mouth or rectum, is of the first import.

Animals exposed to a freezing mixture are quiet at first, then utter cries, and try to escape; a spasmodic quivering of the muscles ensues; the respiration and circulation are accelerated, and death rapidly follows the development of coma. The anatomical changes are by no means so marked as those seen in death from elevation of temperature. The lungs are healthy or a little loaded. The blood is liquid, and sometimes pretty red on the left side of the heart. The muscles are dry, scarcely any blood flowing from them.

One of the most remarkable effects of low temperature is the production of *anæsthesia*; and it may be laid down as a general rule, that animals whose temperature varies from  $100\frac{1}{4}^{\circ}$  to  $112^{\circ}$ , become anæsthetic when this is reduced to  $75^{\circ}$  or  $77^{\circ}$ . The state of anæsthesia may be produced in animals which are chilled by temporary immersion, or by the application of ether, &c., as well as in those that are kept in contact with cold bodies; and its existence in the most complete degree does not imply additional difficulty in restoring the animal by means of warmth.

The experiments giving rise to the preceding remarks, demonstrate the *intimate relation between the functions of the skin and the animal temperature*; and another series which M. Magendie has had on hand since 1842, and is about now to continue, afford additional illustration of this. These are performed by covering the bodies of dogs, guinea-pigs, and rabbits, with gum, gelatine, caoutchouc varnish, or similar substances which dry rapidly, and do not impede the respiratory movements. Animals so covered die in from two to eight hours, the circulation having become almost completely arrested in the large vessels. The temperature, measured in the rectum, may diminish  $14^{\circ}$  or  $18^{\circ}$  without the animal exhibiting suffering; but at this point, or soon after, it becomes dull and plaintive, is unable to stand, and breathes both seldom and slowly; and at the temperature of  $68^{\circ}$  or  $70^{\circ}$ , it usually dies.—*Lectures at the College of France. Reported by M. Durand Fardel. L'Union Med., 1850, Nos. 45, 46, 47.*

#### *Structure and Optical Properties of the Eye.*

VALÉE states that he has found, by a comparison of Krauss's measurements of the dimensions of the eye, that the exterior convex surfaces have exactly that geometrical form (curved surface of the fourth order, Herschell's 'Treatise on Light') which produces foci free from deviations. He calls them *optoidal* surfaces; and also finds that the posterior convex surfaces are, at least, so far optoidal, as the pencils of light penetrating into the eye infringe upon them. In a subsequent memoir, Valée applies to the eye the proposition, that of a series of refracting surfaces of any form, which are penetrated by rays issuing from one point, one may always be so selected that all the rays are again concentrated into one focus. He is of opinion that the cornea always receives, through the muscles of the eye, the form requisite for the production of sharply-defined images on the retina. These changes of form might be smaller, the less the form of the other refracting surfaces differs from that of the optoidal surface. (*Compt. Rend., xxiv, p. 676; Ibid., xxv, p. 501; and Liebig's Report, vol. i, p. 166.*)

*Dispersion of the Human Eye.*—Matheissen is of opinion, that the means for effecting the achromatism of optical impressions are arranged by nature external to the real optical apparatus of the eye. This opinion is contradicted by Valée, who states that the strong converging pencils of light emergent from the crystalline lens are brought to a point (*appareil acuteur*) by the vitreous humour, which he

considers is composed of layers of different density, and that the rays of different colours are thus brought into one line. (*Compt. Rend.*, xxii, pp. 875 and 1096.) Pappenheim has measured the ratios of refraction of the vitreous humour in the anterior and posterior layer. He has found the former to be 1.3339 and 1.3343, the latter 1.3371, differences which are, at all events, too slight to justify Valée's *appareil acuteur*. (*Compt. Rend.*, xiv, p. 901. *Liebig's Report*, vol. i, p. 166.)

*Defects of Vision*.—Hamilton gives an account of a peculiar defect of vision. A man, 25 years of age, who was short-sighted, and partially blind at night, was able to recognise horizontal lines tolerably well, but could scarcely distinguish vertical ones, and was much less able to delineate them. An experiment, made with a card pierced with two holes, through which the head of a pin was observed, showed that the latter was seen by the left eye at a distance of 6 or 9 inches, according to whether the holes were taken horizontally or vertically; with the right eye the ratio of  $5\frac{1}{2}$  to  $6\frac{1}{2}$  was observed. The defect was remedied by a cylindrical lens. (*Froriep's Notizen* [3], vii, p. 219; and *Report*, p. 167.)

*Musca Volitantes*.—Brewster has founded, on experiments with his own eye, a view respecting the *musca volitantes*, which differs from the explanations given by De la Hire, Porterfield, and Mackenzie. He points out as the cause, portions of the cells in which the vitreous humour is inclosed, of which the torn filaments float about in the chamber, and throw shadow and reflected light on the retina. By means of two bright lights placed before the eye, two shadows of the same fibre were obtained, by the aid of which Brewster has measured the apparent diameter of the *musca volitantes*, as well as the distance from the retina of the bodies producing it. According to Brewster, the *musca* occur in every healthy eye, becoming dangerous only by too great an accumulation; they are, therefore, by no means a symptom of approaching blindness from cataract or amaurosis.—*Phil. Mag.* [3], xxxii, p. 1. *Liebig and Kopp's Report*, vol. i, p. 167.

#### *Researches on the Formation of the Muscles in Vertebrated Animals.*

By DR. LEBERT.

TOWARDS the 36th hour of incubation, the contractions of the heart in the chick are very manifest and regular. Nevertheless, it is at that time composed of nothing else than "organo-plastic globules," imbedded in an interglobular granular substance. The structure of the heart in the early embryo of all other vertebrata is the same, and this may be recognised as the permanent condition of the heart in certain invertebrated animals, especially the compound ascidians. From the time that the first blood-globules appear, it is easy to discriminate between these and the organo-plastic globules of the heart. Between the 3d and 4th days of incubation, the structure of the heart presents a less defined aspect, and the cellular envelopes of the organo-plastic globules in part disappear. At this epoch, also, a superficial layer of fusiform cells may be distinguished, in the place of the future pericardium. Between the 4th and 5th days of incubation are seen, for the first time, in the midst of the mass of globular particles, certain elongated sub-cylindrical bodies, sometimes grouped together in a reticular manner; such bodies are the first rudiments of the muscular fibres, not merely in the heart, but also in the ordinary muscles of vertebrata in general; and they may hence be designated "myogenic cells." These cells do not appear to originate in any direct transformation of the organo-plastic cells, nor can it be shown that they are descended from them. The myogenic cells are rather larger than the organo-plastic, and are indistinctly marked with longitudinal striæ; they are destitute of nuclei, but they frequently contain a considerable number of globular particles. Between the 7th and 8th days, the organo-plastic globules undergo a considerable diminution, and the muscular substance undergoes a more complete development. A longitudinal striation shows itself in the contents of the cylinders; their internal granules group themselves along these striæ, which correspond to the primitive fibres;

the transverse markings do not show themselves until some time afterwards. Between the 7th and 9th days, the myogenic cells become more regularly cylindrical and their extremities more rounded; the tendons then begin to be visible, and enclose the lower part of the cylinders, without having any relation of continuity with them. The interior of the cylinders becomes more regularly striated in the direction of its length; and it is between the 10th and 12th days that the transverse striæ appear on the surface, and multiply rapidly, becoming at the same time more and more regular; little by little, the organo-plastic globules which separate the primitive cylinders disappear; the cylinders approach one another; and before the end of embryonic life, they are found to be grouped into fasciculi. The history of the development of the voluntary muscles is precisely the same as in the heart; but it is some days later in the chick, the heart being in full functional activity before the myogenic cells appear elsewhere.

The general history of the development of the muscles is the same in mammalia as in birds, making allowance for the difference of rate; thus in the embryo of the bat, the formation of the heart begins to take place when the fœtus is no more than 2 millim. in length; whilst they cannot be distinguished as elements of the voluntary muscles until the embryo is from 12 to 15 millim. in length. The contrary is the case, however, in the tadpole; for the muscles of the back and members may be seen in process of formation in tadpoles of 3 or 4 millim. in length; whilst the muscular structure of the heart is not further advanced in embryos of twice the length. The contrast, in this respect, between the embryo bird and mammal on the one hand, and the tadpole on the other, obviously has reference to the circumstances under which their respective development is taking place; the tadpole being destined to obtain, by its own locomotive powers, that food, with which the others are supplied in a different mode. In the perch, the heart appears proportionably earlier than in the tadpole, and sooner acquires its characteristic structure. Its first pulsations precede the earliest appearance of the red corpuscles of the blood; and the author is quite satisfied that these corpuscles are not, as some have supposed, detached organo-plastic globules. The first appearance of the myogenic cells is synchronous with the first subdivision of the cavities of the heart, which takes place when the embryo of the perch is from 3 to 4 millim. in length; and the first formation of the voluntary muscles takes place about the same period.—*Annales des Sciences Naturelles*, Juin 1849.

## ORGANIC CHEMISTRY.

### *Chemical Researches on the Respiration of Animals of different species.*

By MM. REGNAULT and REISET.

THESE experiments were made in order to determine what are those changes in the atmosphere surrounding animals, which depend on pulmonary respiration and the exhalations of the skin and intestinal canal combined, and what are those which proceed from the exhalation of gases by the skin and intestinal canal alone.

Great care seems to have been taken to ensure satisfactory results; and every detail of the subject is entered into with almost mathematical precision. It would take up too much space to give a description of the new apparatus made use of in these experiments, which will be better understood by referring to the diagram in the original. It consists chiefly of a large glass bell, in which the animal is enclosed, with an apparatus for absorbing the carbonic acid gas, and another for furnishing oxygen in the stead of that consumed by respiration. The remainder of the apparatus is of a very complicated nature, being adapted to the purposes of measurement and analysis.

The analysis of the gaseous atmosphere contained in the apparatus after the experiments on respiration, showed that it was always composed of carbonic acid gas, oxygen, nitrogen, and a small portion of combustible gas, giving water and carbonic acid gas when ignited. Sometimes this small portion of gas was found



to be pure hydrogen; but on combustion with oxygen, it nearly always formed water and carbonic acid.

The first or preliminary experiments were made in order to determine *whether Nitrogen was exhaled or absorbed* during respiration. For this purpose a fowl, a duck, three pigeons, two rabbits, a cat, and a dog, were successively put in the apparatus during several hours, and the result showed a constant exhalation of nitrogen. The writers next endeavoured to test the truth of Dr. Edwards's assertion, that birds exhale nitrogen in spring and summer, and absorb it in winter. A fowl, a duck, two rabbits, and a dog, were submitted to experiment during the winter, and were surrounded by a glacial atmosphere; but though the respiration was not so active, nothing was observed which at all seemed to corroborate Edwards's statement, except that the exhalation of nitrogen was less than in a warmer temperature. The temperature of the atmosphere surrounding the animals in these experiments was that of melting ice. A fowl, a rabbit, and a dog, were put in an atmosphere containing more oxygen than atmospheric air, in order to discover whether the composition of the air has any influence on the exhalation or absorption of nitrogen. The animals did not seem to experience any annoyance from this change of atmosphere; and the result of the experiment proves, that the quantity of oxygen in the atmosphere surrounding animals has no influence on the quantity of oxygen consumed by them in respiration; that nitrogen is always exhaled, though the quantity is perhaps less when less oxygen exists in the air; and that they continue to live in the same atmosphere for a long time, provided the carbonic acid is absorbed by the potash in the apparatus, and its place supplied by the same volume of oxygen. The quantity of nitrogen exhaled is very small.

A series of experiments now follow, on a number of rabbits in a normal atmosphere; some were well fed, some were fasting; in one case, a rabbit was covered with oil at the desire of M. Magendie, who had observed that when animals are covered with oil or varnish, they become cold and die. In this case, the animal died an hour after having been taken out of the apparatus. The results of these experiments show that nitrogen is exhaled in a proportion of 1-100th of the oxygen consumed; that the consumption of oxygen varies very much in different individuals; that it presents no proportion to the weight of the animal; that the consumption of oxygen varies very much, not only in different individuals, but also in the same individual at different periods; that the quantity of oxygen consumed by animals, when fasting, is much less than when they are receiving their usual nourishment; that the quantity of nitrogen exhaled varies little, whether the animals be feeding or fasting; and that more nitrogen was exhaled when the animals were feeding on bread or corn, than when feeding on vegetables, the quantity of oxygen consumed being about the same.

Eight experiments were made on dogs fed on meat; the duration of the experiments varied from 10 hours 15 minutes to 27 hours; the temperature from 59° to 77°. Two other dogs were fed, one with bread, fat, and water, and the other on bread, fat, water, and a little meat. Another dog, having fasted for 38 hours, was put into the apparatus, and submitted to experiment for 22 hours; he was then taken out, fed with mutton-fat, and again put in for 13 hours 15 minutes. One dog was covered with gelatine, but it seemed to have no effect on him in any way. The exhalation of nitrogen varied very much in the different dogs fed with meat, but it was always small, being 1·7 per cent. of the weight of oxygen consumed. The amount of oxygen consumed by dogs fed on farinaceous food mixed with a little meat, was greater than when they were entirely fed with meat. In the experiment in which the dog was fed with bread, fat, water, and meat, the contents of the stomach were vomited, and two *litres* of pure hydrogen were found in the apparatus. From this circumstance the writers are led to conclude that it is probable that pure hydrogen is formed in the stomach during digestion, but that it is again consumed in the further processes of digestion. On comparing the latter with the former experiments, it was found that for the same amount of oxygen consumed, the quantity of carbonic acid formed was greater when

the animals were fed on farinaceous food than when they were fed on meat alone. In the experiment on a dog in a state of inanition, much nitrogen was found to be absorbed: when fed, this dog consumed 6.59 gr. of oxygen per hour, when fasting only 5.57 gr.; when fed with fat alone, he consumed somewhat less oxygen than when feeding on bread.

Professor Sacc of Neufchatel sent the writers four marmots, which had been subjected to experiments by him from the month of November, 1847, until the 21st of February, 1848. During this time they were frequently weighed, and a most curious fact was ascertained, viz., that the dormouse increases in weight when in a state of complete torpor, and continues to do so until he partially awakes and discharges urine.

The chief point of interest in these experiments is, that in the torpid state these animals consume a remarkably small quantity of oxygen. In one of these experiments, two of these animals were put into the apparatus, and a sufficient quantity of oxygen furnished to suffice for two days, at the rate of consumption during torpor: in the morning one was found dead, and from its change of position it had evidently awoke in the night and consumed all the oxygen. Its companion, when placed near the fire, awoke out of its torpor, though it had been shut up five or six hours in the atmosphere which killed the other. The writers explain the increase of weight in these animals during torpor, by proving, that the amount of oxygen consumed is greater than the quantity of carbonic acid formed, and that very little perspiration takes place.

A number of fowls were now put into the apparatus; some were fed on oats and water, some on meat; others on bread and water; some were fasting. The duration of the experiments varied from 24 to 87 hours. Under either kind of alimentation, nitrogen was exhaled; in a state of inanition it seemed to be absorbed. The consumption of oxygen was the same whether the birds were fed with meat or grain.

*Experiments to determine the part which the Body of Animals takes in Respiration.*—Having studied in the foregoing experiments the united effects of cutaneous and pulmonary respiration on the atmosphere, the writers endeavoured to determine the influence of the body alone on respiration. For this two series of experiments were made; one to determine the quantity of carbonic acid generated by the body of an animal shut up in an india-rubber sac, a free entry being allowed to atmospheric air; while in the other the body of the animal was allowed to remain enclosed with the same measure of air for a given space of time. From these experiments the writers conclude that the quantity of carbonic acid gas generated by the body of an animal is 1-50th of that furnished by pulmonary respiration; and that the quantity of exhalations from the bodies of warm-blooded animals is so small, that the results of all the foregoing experiments may be considered to proceed from the pulmonary respiration alone; but it is probable that in cold-blooded animals cutaneous respiration plays a more important part. Taking all these various experiments into consideration, the writers come to the following general conclusions:

1. That warm-blooded animals, quadrupeds, birds, &c., subjected to their ordinary regimen, exhale nitrogen, but never more than 2-100ths, sometimes less than 1-100th of the oxygen consumed.
2. That in a state of inanition, animals often absorb nitrogen. This absorption was constantly observed in birds, but not always in the mammalia.
3. That when fowls, after having fasted for a few days, are fed with substances differing from their usual food, they absorb nitrogen until they are accustomed to their new kind of food, when they again exhale it.
4. That animals suffering from change of food, or from other causes, absorb nitrogen.
5. The proportion of oxygen contained in the carbonic acid gas, and that consumed, seems more to depend on the nature of the food, than on the class of animals examined; the quantity is greater when the animal is fed with grain, less when fed with meat alone; when fed on vegetables, the proportions seem to oscillate between the two.
6. The proportion of oxygen consumed is nearly uniform in animals of the same

species, subjected to the same regimen; but it varies very much in the same animal when its alimentation is not regulated.

7. As all animals in a state of inanition present nothing but their own substance for the purposes of respiration, all warm-blooded animals in this condition have the same characters of respiration as carnivorous animals.

8. The proportion of oxygen contained in the carbonic acid gas, to that consumed in respiration, varies very much in the same animal, according to the alimentation to which it has been subjected, which alone would prove the fallacy of Valentin and Brunner's theory.

9. In the experiments on fowls, fed as usual on grain, more oxygen was found in the carbonic acid gas than had been furnished for the purposes of respiration, which the writers think suffices to prove that the hypothesis of Lavoisier, Dulong, and Despretz, on animal heat is inadmissible.

10. The quantity of oxygen consumed in a given time in the same animal varies at different stages of digestion. When in motion it is greater than in a state of repose. It is greater in young than in old animals, greater in healthy, lean subjects, than in fat individuals.

11. The consumption of oxygen in a given time by the same weight of animals, varies much according to their absolute size. It is ten times greater in the smaller than in the larger birds.

12. Warm-blooded animals exhale almost indeterminably small quantities of ammonia and sulphuretted gas.

13. The respiration of the well-fed dormouse when awake, does not differ from that of other mammalia similarly fed. But when it is torpid, nitrogen is often absorbed; and the difference between the oxygen inhaled and that contained in the carbonic acid gas is much less. The weight of oxygen taken up by non-gaseous combinations is greater than that exhaled in the carbonic acid; and as the animal while asleep loses little by perspiration, it increases in weight; but this increase is not indefinite, as it evacuates urine.

14. The consumption of oxygen by the marmot, when torpid, is only 1-50th of its consumption when awake; and probably this proportion is still less in a lower temperature, than in that in which these experiments were made.

15. At the moment of awaking, the respiration of the dormouse is very active; and it consumes more oxygen at that moment than when quite awake.

16. Reptiles of equal weight with warm-blooded animals consume less oxygen; nitrogen seems sometimes to be exhaled—sometimes absorbed by them.

17. Frogs without lungs live for a long time and continue to respire; the quantity of gases absorbed or exhaled varies little from that of frogs with lungs, which seems to prove that they principally respire through the skin.

18. The respiration of earth-worms differs little from that of frogs.

19. The respiration of insects, such as chaffers and silk-worms, is more active than that of reptiles; they seem to consume the same quantity of oxygen as the same weight of mammalia. The respiration of small animals is much more active than that of the larger animals of the same class.

20. Animals of various classes respire in the same manner, in an atmosphere rich in oxygen, as they do in the atmospheric air. Their respiration differs little from the usual standard, when in an atmosphere where hydrogen supplies the place of the nitrogen of the air; only the quantity of oxygen consumed is increased, owing to the necessity of compensating for the animal heat lost by the body in contact with hydrogen.

In conclusion, after answering some objections which might be made to the mode of carrying out these experiments, the writers express their regret that they have not been able to extend their inquiries to man, which was their original intention, but which they have not been able to accomplish on account of the great expense of so large an apparatus. They think that the study of the respiration of man in different pathological states might furnish valuable information with regard to diagnosis, &c. and that different gases might be administered constantly in small doses with great benefit to the patient. They hope that their experiments

will give a stimulus to renewed and continued research on this subject.—*Annales de Chimie*, 1849.

[The experiments of MM. Regnault and Reiset are chiefly valuable on account of the great accuracy of the means employed for the estimation of the changes effected by the respiration of the animals which were the subjects of them. We believe that it is generally admitted by chemists, that no means had been previously devised which at all approach these in precision. The results which they have hitherto obtained, although by no means valueless, are scarcely commensurate with the labour which has been bestowed upon them; consisting chiefly in confirmations and corrections of the results previously obtained by others. The chief new point of interest is the positive increase in the weight of the hibernating animal by absorption of gases. What is now most needed, is an exact determination of the influence of *external temperature* on the amount of oxygen consumed and of carbonic acid exhaled, in warm and cold-blooded animals respectively,—a point which these experimenters have almost completely left out of view. The results of previous inquiries lead to the belief, that an augmentation of external heat within certain limits produces a greater activity of respiration in cold-blooded animals, by increasing their general vital activity; whilst in warm-blooded animals, which maintain a fixed standard of bodily temperature, an augmentation of external heat diminishes the demand for the combusive process by which that temperature is sustained, and the greatest activity of respiration exists (except in hibernating animals) when the external temperature is the lowest. These points need a careful investigation; and we trust that MM. Regnault and Reiset will take some acute physiologist into their counsels, who may suggest and guide their inquiries.]

#### *Fermentation and Putrefaction.*

1st. *Views regarding the process.*—A memoir has been published by Liebig, which treats of fermentation and allied phenomena. This chemist restricts the term *putrefaction* to those processes of decomposition which are originally set up in some part of an organic compound by external influences, and are thence propagated throughout the entire mass without the further co-operation of the original cause. *Fermentation*, according to his definition, is the decomposition exhibited in the presence of putrefying substances or ferments, by compounds, nitrogenous or non-nitrogenous, which alone are not capable of putrefaction. Liebig opposes the view which considers fermentation and putrefaction as the result of vital processes, that is, of the development of vegetable organisms or of microscopic animals; and he adduces the fact, that no traces of vegetable formations are perceptible in milk which has been left for some time in vessels carefully tied over with blotting paper, not even after fermentation has regularly set in, and a large quantity of lactic acid been formed. He further remarks concerning these processes, and of the views which have been entertained respecting them, that chemists have directed their attention too exclusively to the phenomena manifested during the alcoholic fermentation; and that they have generalized upon a very limited number of facts, instead of seeking for an explanation of the process among fermentation-phenomena of a more general character.

Somewhat allied to the preceding are the opinions of Schmidt, who says that the development of fungi is not by any means necessary for fermentation; for he has observed that a given quantity of yeast, when brought into contact with the same amount of water, and varying quantities of urea, gave rise, during a given space of time, to the same amount of ammonia. A solution of gelatine, when mixed with urea, phosphate of soda, and sulphate of magnesia, filtered, heated to ebullition for half an hour, and then exposed to a current of atmospheric air, which had been passed through sulphuric acid, gave, after the lapse of eight days, the same amount of phosphate of magnesia and ammonia, as a similar liquid did, which, *cateris*

*paribus*, had been exposed to the action of common atmospheric air. Nor are fungi the *primum movens* of saccharic fermentation; for although the clear filtrate, obtained by throwing a watery solution of crushed almonds on a moist filter, soon induces fermentation in a solution of urea and of grape-sugar, yet, in the latter case, *no trace of ferment-cells can be discovered under the microscope*. If, however, the solution, which still contains sugar, is allowed to stand for eight days or a fortnight, after fermentation has ceased, an exuberant development of cellular aggregations is observed, but no fermentation ensues. "Now," says Schmidt, "if you take these cellular aggregations (fungi), wash them well with water, and introduce them into a fresh solution of grape-sugar, they will continue to grow luxuriantly, although they induce but a very weak fermentation, if any: hence," concludes this author, "the growth of fungi during fermentation is but a secondary phenomenon."

Schmidt is of opinion that fermentation is a process analogous to the formation of ether. He believes that one of the constituents of yeast, together with the elements of grape-sugar, gives rise to the formation of one or several compounds, which are decomposed in *statu nascenti* (like sulphovinic acid), splitting into alcohol and carbonic acid.

R. Wagner has carefully examined some of the foregoing statements; but he has not been able to confirm them. On the contrary, he has found that when the filtrate of crushed almonds induces the fermentation of grape-sugar, it invariably does so with the simultaneous evolution of carbonic acid and the formation of ferment-cells. Nor do Wagner's observations agree with some others mentioned by Schmidt; Wagner, for instance, states that the washed fungi introduced into a fresh solution of pure grape-sugar will not continue to grow without inducing fermentation. He even inquires whence they derive the protein-matters which are necessary to their growth and reproduction.

In direct opposition to the views entertained by Liebig and Schmidt are those of Blondeau, who asserts that every kind of fermentation is caused by the development of fungi: the alcoholic fermentation being due to the growth of *Torula cerevisiae*, the lactic to *Penicillium glaucum*. In proof of this, Blondeau states, that when beer-yeast is left in contact with water in a dark and damp place, it contains germs both of *Torula cerevisiae* and of *Penicillium glaucum*; the former of which can be separated by a filter, and will induce alcoholic fermentation in sugar-water, whilst the latter, which are extremely minute, pass through the filter, and the filtrate, mixed with sugar-water, will give rise to lactic fermentation. Acetic fermentation, according to Blondeau, is due to the development of *Torula aceti*; for he states, that sugar is converted into acetic acid, without the evolution of gas, if it is dissolved in water, and subjected to the prolonged action of casein at a temperature of 68° F. The conversion of nitrogenous substances into fat (fatty fermentation), as, for instance, of casein, in the manufacture of Roquefort cheese, and of fibrin under similar circumstances, is said by Blondeau to be dependent on the growth of *Penicillium glaucum* or *Torula viridis*. He affirms, moreover, that the former fungus also acts in effecting the butyric and urea fermentations.

Somewhat in confirmation of the preceding are the observations made by Wagner; viz., that the cells of upper yeast are chiefly distinguished from those of lower yeast by being incontinuous. He states, moreover, that when upper yeast is exposed in a watch-glass to the atmosphere, it soon exhibits the vegetations observed by Kützing. These gradually pass into *sporotrichum*, and, lastly, into *mucor*. Lower yeast, however, is, under the same circumstances, at once converted into *mucor*.

2. *Circumstances which affect the phenomena of fermentation, and the products which result therefrom.*—In both putrefaction and fermentation, Liebig distinguishes those processes in which the oxygen of the air continually co-operates, from such as are accomplished without farther access of atmospheric air. In lactic fermentation (as of sugar with chalk and cheese) mannite is invariably produced as a secondary product, though the formation of it cannot be satisfactorily explained. A solution of leucine, when in contact with the atmosphere, is rapidly decomposed upon adding a trace of putrefying fibrine; the solution contains a considerable quantity of vale-

riate of ammonia. Leucine, however, does not undergo decomposition without the addition of putrefying fibrin. Fibrin, when covered with a thin layer of water, and exposed to the atmosphere until putrefaction is accomplished, is found to be converted into a fluid which exhibits the deportment of a solution of albumen.

Liebig has communicated the results of some experiments made by Trantschold, concerning the power which porous substances (straw, paper, charcoal, flours of sulphur, &c.) possess of inducing alcoholic fermentation in a solution of grape-sugar to which some tartrate of ammonia has been added. He finds that the fermentation is due to the presence of a foreign substance in the sugar. This substance may be removed from the saccharine solution by treating it with ignited bone-black, after which the grape-sugar ceases to ferment under the influence of a porous body. Urea, when in contact with yeast, is rapidly converted into carbonic acid and ammonia. Schmidt states that when the urea is mixed with sugar, the transformation of the former will not commence until the whole of the latter is decomposed.

Wagner has made an extensive series of inquiries into the influence which various agents have over the fermentation of grape-sugar in malt extract. He finds that when yeast is dried at a temperature of  $212^{\circ}$  F., and then powdered, so as not to exhibit the form of cells, under the microscope, it induced fermentation only after the lapse of thirty-six hours. Schmidt states that the inactivity of crushed yeast is due, not to the destruction of the fungi, but to the chemical changes which are induced in it during the process of comminution; and he adds, that the crushed cells, introduced into sugar-water, give rise to the production of lactic acid, without the evolution of gas. Both of these statements, however, are contrary to the experience of Wagner.

Small quantities of mineral acids generally prevent fermentation, though phosphoric acid is an exception, for it appears to favour the process. The effect produced by organic acids is variable; butyric acid seems to act by transforming alcoholic fermentation into butyric, common yeast being converted into butyric acid ferment. Small quantities of acetic, tartaric, and especially lactic acid, exert the most favorable influence on the propagation of the ferment-cells. Fermentation is interrupted even by dilute solutions of alkalies, of soaps, and of chloride of lime; but it is not affected by dilute solution of quinine and strychnia, nor by *small quantities* of free sulphurous acid. Yeast is destroyed by small quantities of protoxide of iron, or of zinc, and by proto-chloride of mercury; but it resists the action of sulphate of protoxide of copper, arsenious acid, and tartar-emetic.

Finally, experiments on putrefaction and fermentation have been performed by Döpping and Struve. They do not, however, confirm the results obtained by Helmholtz, who states that organic substances (grape-sugar, gelatine solution, flesh, &c.) may be secured against putrefaction provided they are heated with water to ebullition, and then supplied with atmospheric air which has been ignited. Döpping and Struve find that all nitrogenous organic substances undergo decomposition even under the influence of ignited air, and that the phenomena are only retarded and changed by the act of ebullition. Helmholtz had observed that the fermentation of grape-juice is not communicated to another portion of grape-juice which is contained in a vessel closed by bladder and introduced into the fermenting liquid. In repeating this experiment, Döpping and Struve once remarked a slight evolution of gas and the subsequent formation of ferment-cells in the confined sugar solution, but these phenomena were not witnessed in their other experiments. Again, the observations made by these chemists do not confirm the statement of Gay Lussac, namely, that grape-juice expressed with the exclusion of air will not ferment. They found that although the atmospheric air was expelled from the vessel containing the grapes in various ways, as by hydrogen, carbonic acid, &c., yet fermentation invariably took place. They are inclined, therefore, to explain this discrepancy by saying that as different spaces of time elapsed between collecting and pressing the grape, it might have influenced the subsequent deportment of the juice.—*Annual Report of the Progress of Chemistry, &c.* By Liebig and Kopp, vol. i, p. 356, &c.

*On the Coffee Diet of the Belgian Miners.* By M. GASPARIN.

ACCORDING to M. Gasparin, the miners of Charleroi, on the Belgian frontier, have discovered the secret of supporting themselves in full health and robust strength, upon a far less proportion of azotised material than is deemed requisite in the rest of Europe—the addition of a considerable quantity of coffee to each meal being the means by which this is effected. It seems the diet is as follows:—Infusion of coffee and chicory (30 grammes of each), 2 litres; milk, 2-10ths of a litre; bread, a kilogramme; butter in variable quantity; green vegetables, 750 grammes; meat, 73 grammes; and beer, 286 grammes; all these united furnish a little less than 15 grammes of nitrogen, whereas inquiry in a great number of the French Departments has shown the quantity of this substance contained in the daily rations of grown men to vary from 20 to 26 grammes, giving an average of 23. Even the most austere of the religious orders, as the Trappists of Arguebelle, whose pallid countenances and feeble powers prove their alimentation to be at the minimum, consume 15 grammes of azote, and 402 of carbon or of hydrogen reduced to its equivalent of carbon. The inhabitants of the central prisons also, whose work is a mere nothing, consume 16 grammes of azote, and 475 of carbon, or reduced hydrogen. The Belgian miners are men of robust power; and when the French miners, whose diet is so much superior, come to Charleroi, they are unable to compete with them. The community is prosperous, having no poor, except such as have become so through accidents.

It is to the use of coffee alone that M. Gasparin attributes this remarkable effect. It cannot be due to the nutritive material of this substance, which only forms 1-35th of the nutritive components of the aliments employed. Does it, then, by increasing the activity of the digestive powers, produce a more complete assimilation of the aliments, or does it retard the nutrition of organs which no longer require a large consumption of material for their maintenance and reparation? According to the researches of Boëker, when the persons experimented upon made no use of coffee, they passed in the 24 hours urine containing 22·275 of urea, 0·578 of uric acid, and 1·291 of phosphoric acid; while, when they took coffee, the quantity of their urine rose from 1394·500 to 1733·750, and contained but 12·585 of urea, 0·402 of uric acid, and 0·854 of phosphoric. The temperance of people who employ much coffee in their diet is well known. The prolonged abstinence of caravans, and the slightly nutritive character of the diet of the Arabs, may also be cited; as also the excellent effects which the distribution of extra rations of coffee have been found to exert, in enabling the French soldiers to endure fatiguing marches in Algeria.—*Gazette Médicale*, No. 15.

## PATHOLOGY AND PRACTICE OF MEDICINE.

*On the Dyspepsia of Liquids.* By M. CHOMEL.

THERE is a symptom of dyspepsia having its seat in the stomach of great value, and one to which sufficient attention is not paid, viz., *gargouillement*. This stomachic *gargouillement* depends especially upon a dyspepsia of liquids, i.e., a difficulty of digesting them. They remain in such cases a long period without becoming absorbed, those taken in the morning still being in the stomach in the evening. For the production of this *gargouillement* it suffices to shake the body forcibly, or to apply both hands at opposite points of the epigastrium. It is a valuable sign, as it indicates that the quantity of liquids taken should be diminished.

The *saliva*, too, in dyspepsia is very *frothy*, and on this account gives rise to a sign that should direct attention to the stomach. Two white lines are formed along the edges of the tongue, in consequence of the action of this organ during speaking having frothed the saliva, which then accumulates at its edges.—*Union Médicale*, No. 29.

*A Remarkable Case of Tapping.* By Dr. GRIFFIN.

THE chief particulars of this case were furnished by the husband of the patient,—an intelligent man, who took notes of his wife's case, as she passed under the hands of various practitioners. After suffering from an obscure abdominal tumour, from 1820 to 1830, she was found to be dropsical, and was tapped within the next ten years 186 times, 751½ gallons of clear, straw-coloured fluid being drawn off. Since 1840 she has continued quite well, being able to walk several miles without fatigue.—*Amer. Journ. of Med. Sciences*, N.S. No. 38, p. 401.

*On Hæmoptysis in Phthisis.* By M. GENDLIN.

AFTER a good injection of the pulmonary arteries and veins of a lung infiltrated with tuberculous matter, we find that the extreme radicles of these vessels are impermeable around the tubercular concretions. This is the reason why these persons are so liable to hæmoptysis; for the obstruction of a certain portion of the capillaries, forces the others to receive the entire quantity of blood brought by the pulmonary artery. This produces their congestion and rupture, and consequent bloody expectoration. . . . .

Hæmoptysis may show itself in the form either of abundant discharges, or of mere streaks. The first usually occur in the early stage of the disease, and are due to the above-mentioned cause. Sometimes several such attacks may occur before the proper symptoms of tubercle become evident. In advanced phthisis, the cells and capillaries are too much obstructed to admit of profuse hæmoptysis; but bloody streaks may occur from rupture of some small, yet permeable vessel, or from exudation from the mucous membrane. The occurrence of large hæmoptysis is very rare at this period, and when it does take place, it arises from the rupture of a large, yet still permeable vessel, as it traverses a cavity. Hæmoptysis does not necessarily imply phthisis, for it may be caused by pulmonary congestion, arising from other causes, as disease of the heart; but when it appears in the form of bloody streaks, it always denotes phthisis.—*Gazette des Hôpitaux*, Nos. 13 and 19.

*On the Conversion of Purulent into Serous Collections.* By M. VELPEAU.

M. VELPEAU observes that more than twenty years since he indicated the nature and signification of this transformation, whereby a hydrocele may, so to speak, be formed from an abscess. Many times has he tested the truth of the observation. A chronic abscess has, perhaps, required opening three or four times. On the first occasion pus flows out, on the second, a serous pus, and afterwards only serum. Whenever we thus see serum replacing pus, we may conclude the cure is not far distant. The sac containing an abscess approaches a mucous membrane in its nature, and has no tendency to become agglutinated. Before this happens, its characters change into those more resembling a serous membrane, the functions and properties of which it then acquires, so that, at the time it is about to close, it is in no wise surprising we should only find serum.—*Union Médicale*, No. 8.

*On the Shivering of Pneumonia in the Aged.* By M. CHOMEL.

THE shivering of Pneumonia is remarkable, inasmuch as it is very intense, and of long duration, and hardly ever accompanied by the chattering of the teeth observed in the cold fit of ague. A shivering fit occurring after the age of from 55 to 60, almost always announces the onset of this disease; and this may be accompanied with alimentary or bilious vomiting, or more or less disturbance of the mind, without diminishing its semeiotic value. Pneumonia is the commonest of acute diseases in the aged, and one the possibility of whose existence we should always have in mind in doubtful cases, even when phenomena, referable to the nervous system, which ordinarily have no relation to it, are present. An old gentle-



man, on returning from the theatre, was seized with a shivering fit, and very considerable disturbance of the mind, which were succeeded next day by intense fever. Although there was neither cough or dyspnoea present, M. Chomel, in opposition to his colleagues, pronounced the case one of pneumonia, and soon afterwards a return of the intellectual powers, and the development of the symptoms proper to the disease, justified his diagnosis.

One form of the initial shivering may give rise to considerable embarrassment, namely, when a second attack occurs twenty-four or forty-eight hours after the first, which, when the ordinary symptoms of pneumonia are absent, may easily lead to the belief of the existence of an intermittent. A robust man was seized with a violent shivering-fit, which was followed by delirium, and a slight cough. Next day a second shivering, with embarrassed breathing occurred; and on the third day, small crepitation was heard. Treated thus far by quinine, he was now energetically treated by antiphlogistics, and although 66 years of age, easily cured. A double shivering is not a very rare symptom, and may be due to the successive development of the disease in the two lungs.—*Union Médicale*, No. 22.

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*On Cervical Adenitis, as observed in Military Hospitals.* By M. H. LARREY.

M. LARREY considers this term, or adenopathia, as less vague than that of ganglionic tumour or engorgement. The disease is of remarkable prevalence in the military hospitals of France, and this far more now than formerly, presenting itself too in robust young soldiers, who would have been exempted by the Council of Revision, had it been present when they were drawn by the conscription. It is observed more frequently in the infantry than the cavalry, certain corps presenting an especial liability, particularly if garrisoned in damp rather than in warm or temperate climates. The officers are nearly exempt. Its prevalence inflicts a serious loss in the effective force, by reason of the furloughs or discharges it necessitates.

In regard to the causes, M. Larrey is not disposed to attribute it to the scrofulous diathesis, other signs of this being absent in these otherwise healthy and robust men. Among the general causes, he enumerates those which exist in the faulty hygienic condition of the soldier's life; and among the local ones, the keen drafts of air the soldier is exposed to through the holes of his sentry-box at night, the incautious exposure of the neck when heated, and the pressure of the tight collar of the uniform to which young soldiers are not accustomed;—troops not exposed to this pressure, as the spahis and zonaves, not being liable to the swelling.

The disease may assume either the acute or chronic form, and is frequently attended with suppuration. In the chronic form, there is often great enlargement and induration. When simple, general, and local means do not suffice, M. Larrey is a strong advocate for the extirpation of the tumours, as originally recommended by Bégin. He says that the accidents attendant upon this operation are very rare, and has observed that in operations in this region, patients seem to be gifted with an instinctive feeling of the importance of quietude while under the knife. He believes the fears of admitting air into the veins during the operation to have been exaggerated, as, after numerous researches, he can only find six fatal cases of death resulting from this cause during the extirpation of cervical tumours, which, moreover, did not seem to be of a glandular nature. It is chiefly from the great success which follows the operation of extirpation, that M. Larrey is induced to regard the affection as in general a purely local one.

M. Gimelle, reporting on the paper, observed, that it is certainly remarkable, that while in civil hospitals, enfeebled subjects, who have undergone privations, and present signs of the scrofulous diathesis, are those in whom this affection is found, in the military hospitals it prevails among the strong, fresh-coloured recruits. He does not attribute so much influence to general, as to local causes; for the workmen who resort to large towns from the provinces, and are in an infinitely worse hygienic position than the soldiers, are not liable to it. The affection is, however, far more prevalent in cold, damp climates, near rivers. Thus, in the garrisons of Alsace it

is often found, while in Algeria it is seldom met with. *M. Roux* believes the affection to be far more frequent now than formerly, not only in military, but in civil life, for he has met with it much oftener during the last than the first twenty years of his career. He has made the same observation in respect to cancer of the lip and tongue: and is inclined to attribute much influence to the greater abuse of tobacco which prevails.—*Bull. de l'Acad.*, xv, p. 619.

*On the Reciprocal Influence of Pregnancy and Pulmonary Phthisis.*

By M. GRISOLLE.

DR. GRISOLLE observes that the opinion generally entertained that pregnancy temporarily arrests the progress of phthisis, which after delivery then proceeds more rapidly to its termination, is founded upon insufficient data, and arrives at an opposite one from the consideration of 27 cases, 17 of which were under his own observation, and 10 communicated to him by M. Louis.

In 24 of the cases, the signs of the disease first appeared during an early period of pregnancy, and in three existed at the time of conception. In all these cases the disease pursued just the same course as in non-pregnant women; but hæmoptysis, which *à priori* might be expected to be more frequent in such subjects, proved to be rather less so. In 13 of the cases which were observed to the end, death occurred in from 8 to 15 months from the appearance of the symptoms, giving an average of a third less than the duration of the disease in non-pregnant women of the same age. Pregnancy would seem rather therefore to accelerate than retard the progress of the disease.

In examining the effect of delivery and the puerperal state on the progress of the disease, M. Grisolle states that twelve women, in whom phthisis at the time of delivery had advanced to the second, and in most to the third stage, struggled against it upon an average for near four months, the symptoms following their ordinary course. Of 10 women, in whom it was in the first stage, or commencement of the second, in 3 the organic lesion continued slowly progressive, in 2 it received a sensible aggravation (as is the case, indeed, in convalescence from disease and various circumstances), while in the others such an amelioration took place as to enable the women to leave the hospital.

In regard to the influence of phthisis on the course of pregnancy, abortion is not of such frequent occurrence as might be expected. Of 22 women sufficiently observed, 3 aborted between the fourth and sixth months, and 3 about the eighth, the remainder going the full time, although in two thirds of them the tubercular cachexia was much advanced. A considerable difference, therefore, exists between phthisis and pneumonia, which induces premature delivery in more than half of the pregnant women it attacks.

With the exception of 1 out of 12 cases observed, delivery was very easy, the labour, even in primiparæ, being terminated with little suffering; a circumstance due more to the enfeebled and yielding state of the tissues, than to the small size of the child, which in more than a fourth of the cases was vigorous and full-sized.

Except in those who, having reached the last stage of phthisis, die a few days or weeks after delivery, the secretion of milk is usually established satisfactorily. But, in a week or two, or at most a month, it diminishes or ceases, and for however short a period the attempt at suckling may have been continued, it always aggravates the condition of the mother, and, if persisted in, causes the death of the child from insufficient nutriment. The autopsy, carefully made in all cases where the infants have so perished, has never revealed the existence of tubercle, affording additional proof of the rarity of the deposition of this, prior to dentition. Where a contrary opinion to this has been expressed, it has arisen from the observer having mistaken for tubercular infiltration a form of pneumonia, of little acuteness of character, in which hepatization affects only certain vesicles, a plastic matter being often deposited in their interstices, which the microscope shows has nothing in common with tubercle.—*Archiv. Génér.*, vol. xxii, pp. 41-51.

*On the Eye as a Sign of Death.* By M. LEGRAND.

A HEALTHY eye, during life, furnishes three images of the flame of a candle placed before it. It continues to do so for some little time after death, but they are then less distinct, less shining, and worse defined. In proportion as the liquids, which maintained in the eye its proper form and moisture, are evaporated, modifications in these images are more and more evident, the physical conditions of the reflecting surfaces undergoing greater and greater changes until they lose this power. Thus the third image, which may cease to be observable almost immediately after death, generally first disappears in consequence of the less diaphanous condition of the crystalline; and then the second, when this has become entirely opaque, or the cornea much obscured. Lastly, the first image, more and more confused in proportion as the transparent cornea becomes influenced, disappears; and death, already rendered probable by the disappearance of the second image, can no longer be called in doubt.—*Gaz. Médicale*, No. 16.

*On Ossification of the Capillary Vessels of the Brain.* By M. DELACOUR.

A MAN, æt. 81, who had long been in a half-imbecile state, died under M. Duplay's charge at the Hospital for Incurables. On examining the brain, considerable injection of both its membranes and substance was observed, as also a superficial *ramollissement* of three convolutions. The white substance retained its normal consistence, and a little serum was found in the ventricles. The cerebral hemispheres were in process of slicing, when on reaching the *centrum ovale* the scalpel met with resistance, and a number of little asperities could be felt by the finger, giving exactly the sensation of a rough beard. On both sides, a number of little points projecting one or two *millimètres*, occupied the whole extent of the centrum and the lower wall of the lateral ventricles, being more apparent in proportion to their proximity to the *corpora striata*. These two organs were found quite disorganized, and of a yellowish gray colour, their tissue being torn up in various parts, and their centre occupied by an osseous mass which grated under the scalpel. This body on the right side was of the size of a haricot bean, that on the other being a little larger. Both seemed formed by the agglomeration of little masses, were covered with asperities, and were plunged into the substance of the corpora. On following the little asperities found in the *centrum ovale*, it was proved, to the satisfaction of M. Duplay, that they were formed of very minute vessels completely ossified, each in fact being traceable to a vessel which, at a certain point, possessed supple walls and a free cavity. The existence of concretions in the *corpora striata* might have been suspected, but these were in fact formed by the juxtaposition of these little crystal-like bodies. No ossification was observable in any of the principal trunks; the cartilaginous or osseous patches, so frequently seen in the aged, not being even observable. There was no trace of either recent or former hemorrhage.—*Gazette des Hôpitaux*, No. 27.

*On Intestinal Worms.* By M. RAIKEM.

M. RAIKEM, in reporting to the Belgian Academy of Medicine upon two cases of *tænia*, forwarded to it by a M. Brasseur, took occasion to present a very interesting *résumé* of what is known on the subject.

He observed, that great discrepancy of opinion prevails still in the profession as to the importance of the presence of worms, especially *tænia*. By many, the symptoms they are said to produce are considered as accidental and attributable to other causes, and in general, the first suspicion of the existence of a *tænia*, is the observation of portions of it in the stools. Still the *ascaris lumbricoides* and *tænia*, in some cases, may induce convulsions and other spasmodic effects, and *ascarides* may cause great pruritus ani and pudendi. Bremser, in his classical work upon intestinal worms, attributes to them but slight pathological importance,

assigning the anxiety of the patients on the subject as the chief motives for submitting them to treatment. Although a long experience has convinced the reporter that most of the disorders of the health attributed to their agency, do not depend on their presence, yet there are quite sufficient cases on record to authorise us in admitting that the symptoms which sometimes accompany the presence of worms in the intestines, especially *tænia*, are not always imaginary, but are so real as to become dangerous, and even fatal.

Dr. Salathé (Strasbourg, 1803) relates a case in which a man entered the hospital, wasting away with some chronic intestinal affection, having some time before passed great number of lumbrici. After death eight *tæniæ* occupied the whole of the small intestines, and were surrounded with a tenacious, mucous matter. An elderly woman was seized with the most violent colic, during which she passed a *tænia solium* more than two metres long, but with no relief, and she died amidst horrid suffering in twenty-four hours. Guidetta (Florence, 1783) states, that at the autopsy, two lumbrici and another *tænia* were discovered. Other examples of the dangerous and fatal effects of worms are recorded in Bremser's work, in the '*Recueil d'observ. des Hôp. militaires*,' (tom. iii,) in the 34th vol. of Roux's '*Journal de Médecine*,' and by Lieutaud, Sennertus, and others.

Gerard de Vos (Utrecht, 1823) states, that among the inhabitants of Holland and Belgium, the *tænia solium* is the variety most often found, which, according to M. Felix Dujardin, is also almost exclusively found in England, Germany, and the East, while the *tænia lata*, or *botriocephalus latus*, is chiefly found in Switzerland, Poland, Russia, and, though seldom, in France. Among 206 *tænia* patients observed by Dr. Wawruch in Vienna, the *solium* was found in 200, and the three persons in whom the *botriocephalus* was found, consisted of a Livonian, a Bavarian, and a Russian. Still Dr. Marinus does not believe the *botriocephalus* to be so very rare in Belgium; for of eleven cases related by him, in proof of the efficacy of the pomegranate, (Ghent, 1830,) three were examples of *botriocephali*, and eight of the *tænia solium*.

Although it is not rare to meet with several entozoa of different genera, or several *tæniæ* of the same species, most authors assert, that the two species of *tænia* which infest men, are never found together in the same subject. This assertion is, however, confuted by undoubted facts. Its accuracy was denied by Dionis, and Brera (Cremona, 1811) relates the case of a Swiss settled at Bologna, who, under treatment for worms, evacuated a *tænia lata*, which species is to some extent indigenous to the inhabitants of Switzerland and the north; and then, after undergoing another course of anthelmintics, on account of the persistence of severe symptoms, passed a *tænia solium*. M. Breton, in the Med.-Chir. Trans., for 1821, relates the case of a child who, while employing the pomegranate, evacuated a *botriocephalus latus* 4 feet 9 inches long on the one day, and a *tænia solium* 9 feet 10 inches long the next. Dr. Wawruch furnishes the case of a goldsmith, who, after passing a *botriocephalus* at Geneva, became the subject of *tænia solium* two years after his establishment at Vienna. Rudolphi (1821) relates the case of a woman who passed several of both species.—*Gazette Médicale*, No. 22.

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#### On Clinical Instruction in Insanity. By M. FALRET.

THIS is an excellent paper, and we regret that our limits compel us to curtail it. Its object is to reply to the objections which have been raised to the clinical study of diseases of the mind, and to show how it may be most beneficially conducted, both as regards patients and pupils. It has been said that the practice would irritate and wound the feelings of the insane, and thus aggravate their position, and retard their recovery; but this view has been derived rather from the observation of man in his normal than in his diseased condition.

"There is a difficulty, when speaking of the insane, in divesting ourselves of the ideas derived from the study of man in general; and even physicians conversant with them make too direct an application of physiology to pathology. Doubtless the

insane present many points in common with those of sound mind; and whatever may be the nature or degree of their aberration, they always preserve some of the characteristics of man. There are those among them, who, in consequence of the slight extent of the affection, its recent invasion, or intermittent character, approach very nearly to the sane condition; but it is no less true that lunatics, considered generally, present certain characteristics which trace a distinct line of demarcation between them and men in the full enjoyment of reason. These characteristics, few in number it is true, applicable as they are to beings at least as diverse from each other as are men in the normal state, are sufficiently strongly marked to admit of their being briefly defined, so that the interval that separates the insane from the sound intellect may be plainly exhibited."

Three of such distinctive characteristics are enumerated by M. Falret. *First*, the insane person finds himself in complete disaccordance with external circumstances. Everything, seen through the prism of his perverted ideas, seems new. At first he feels astonished, and often afflicted; but as with increasing disease the world of new ideas gains predominance over the old one, false interpretations are received, and the insanity is confirmed. Every one conversant with the insane must have observed the delight and astonishment which convalescents experience with the return of reason, seeming now again to see the same objects with entirely different eyes. A *second* characteristic is the inward concentration of ideas which takes place. The patient becomes absorbed with that which constitutes his aberration, and is only temporarily to be aroused to the observation of circumstances foreign to it. This substitution of internal for external life is one of the fundamental characteristics of insanity. It is exhibited in a simple comparative experiment. Let a party of strangers be introduced into a workshop composed of persons in health. Every eye is simultaneously directed towards them, and a kind of interruption of business ensues. But let the same persons enter the workshop of a lunatic asylum, and it will be found that very few of the inmates are animated by curiosity, the great mass remaining motionless, or if they do instinctively look towards the visitors, it is only for an instant—the fantastic world in which they live having far greater attractions for them than such indifferent external circumstances. The *third* characteristic is a natural consequence of the last; for this concentrated internal life must necessarily destroy all that binds him to his kind. Changed in his mode of perception, and the victim of his morbid ideas, he can no longer entertain his habitual relations with friends or relatives, that is, with man. He feels no want of intercommunication with them; he has lost the power of *sociability*. Seeing, then, that lunatics are so differently influenced by external impressions, from man in his normal state, is it not reasonable to suppose that clinical instruction acts otherwise upon them than might be supposed by those who have considered man only in his sane state?

In reference to clinical instruction, the insane may be divided into three categories. Some, much pleased in speaking of themselves or their ideas, willingly offer themselves for observation; others, strongly concentrated or preoccupied, require solicitation, but so attached are they to the objects of their contemplation that they suffer no injury or annoyance from having these made topics of inquiry; and, in others, the intellect is too disordered or enfeebled to be unfavorably influenced by questions, and yet such are able to give an account of their feelings. We may say, as a general rule, that either the insane are too ill to reply to interrogatories, or, which is by far the most frequent case, they do so without annoyance or ill consequences, and frequently are well satisfied to find persons desirous of listening to them.

Another objection has been raised. It is said, that by interrogating the patient before the pupils, you lead him to suspect his insanity, or more or less explicitly inform him that this is considered to exist—to the wounding his feelings, and retardation of his recovery. In point of fact, however, the friends of the patient have, prior to his admission, familiarized him with this suspicion; and he himself, as his reason progressively gave way, had entertained it.

"We may add, that so far from this being a danger of clinical instruction, it

forms one of its advantages, for physicians who, like ourselves, have adopted the plan of always acquainting the patient with the nature of his disease, and accustoming him to look upon us in our medical capacity. Of course the proper moment is seized for imparting this sad truth, and the expressions conveying it are modified according to a thousand circumstances, as the susceptibility or pride of the patients, the condition of their intellect, the character of the disorder, &c. But as, under some form or other, the physician has already acquainted them with the character of their disease, it can do no harm to mention this again before the pupils. We are advocates of this principle, because then our relations with the patient are founded on a real basis, and to obtain success we are not obliged to have recourse to concealments and deceptions, and our conduct may acquire all the precision and frankness capable of favorably influencing those under our control. We are partisans of this principle, because it makes an appeal to any power which the lunatic may retain of struggling against his disease. What else do we, in the application of every principle for the treatment of the insane, than place these unfortunate beings in the most favorable condition for recovery? What condition can be more favorable, than the diverting to the profit of their cure that activity of their faculties which they employ injuriously in fomenting their disorder?"

M. Falret examines in detail the operation of clinical teaching upon all the varieties of insanity, and thus remarks upon its supposed injury in *partial insanity*.

"Appearances are here deceptive. By the very fact that they are mad, they differ in relation to understanding and sensibility from men of sound minds. It is too generally believed that the partially insane have some erroneous idea, or exclusive sentiment, implanted in an otherwise healthy intellectual and moral nature. Reflection upon the intellectual and affective faculties, and direct observation of these pretended monomaniacs, amply prove this to be an error, and one pregnant with serious consequences. Observation daily shows us, that to the predominant delirium are added other forms of delirium, illusions or hallucinations, bearing no relation whatever to it—that such patients have, in fact, a singular aptitude to become affected by some of these. So observation shows, that with the predominant delirium there may be conjoined a remarkable slowness, or an excessive rapidity in the flow, and a great confusion in the arrangement, of the ideas, besides other general phenomena which ordinarily escape the attention of the physician too much habituated to regard the dominant symptom as the entire disease. Were this the case, there would frequently be no difference between a monomaniac and a man gifted with grand and beautiful conceptions, or subjugated by an exclusive passion. The truth is, that the dominant idea of the monomaniac is the *relievo* of the mental condition, the *ground* of which is the general condition alluded to—sometimes rendered sensible to all by a remarkable rapidity in the succession of ideas, by excitement, or even a true maniacal paroxysm."

Even the convalescent patients, and those who have remissions of their disease, are found by M. Falret, as a general rule, to be uninjured by clinical teaching. Convalescents, like persons who have suffered from other diseases, often take a pleasure in talking about and describing dangers they have passed. Moreover, the satisfaction they feel in again recovering their moral and intellectual existence, leads them to detail, as far as their memory allows, the various perversions they had been liable to, as proving how different this disordered condition was from their habitual state. Nevertheless there are certain of these patients who have almost recovered, being in a state of transition, or of struggle with their disease, who require the greatest circumspection in their management. Such persons are irritable, and very unstable in their ideas, and have not sufficiently recovered consciousness to desire or be able to detail their experiences. Others of them are so timid or suspicious, that the simplest question covers them with confusion. Other cases occasionally manifest predominant ideas, that render it undesirable to employ them for clinical instruction. But all these are rare exceptions, and all acquainted with the insane are aware that true convalescents enter into the most apparently repugnant details without any difficulty whatever. One of the most certain signs of the cure of insanity is the

recognition, without difficulty or shame, that it had existed; and the cured lunatic revisits, not only without repugnance, but with pleasure, the establishment in which he was placed, and the persons to whom he was indebted for his restoration.

In the second part of this valuable essay, M. Falret details at length the precautions by the observance of which clinical instruction, so far from proving detrimental to the insane, may be rendered adjuvatory to their treatment. He much prefers going with all his pupils to the wards in which the patients are, to having these brought into the theatre. The patients are far less disturbed, and the pupils see them in greater numbers, and in every shade of the malady. He sees no reason for adopting Esquirol's plan of admitting small series of pupils only; and, in fact, imparts clinical instruction just as he would in an ordinary hospital,—questioning the patients in presence of the pupils, and teaching these how to observe, but reserving all details that might be improper before the patient, for a subsequent lecture. The pupils are not allowed at first to make any independent examination of the patients; for not only they, but physicians unacquainted with the subject, do not know how to profit by the opportunity, until they have been taught how to observe; and moreover might do mischief to the patients, by a system of interrogatories not justified by experience. The pupils are enjoined to treat the patients with the greatest kindness and respect, all irony or mockery being strictly forbidden, as wounding the patients' feelings, and setting a bad example to the attendants, who are already but too disposed to humiliate these unfortunate beings. A frank, natural manner, is that which is most agreeable to the patients, and a regard, almost amounting to deference, should be manifested; but nothing wounds them more sensitively than pitying them. The pupils must carefully avoid all appearance of curiosity or marked attention,—seeming to visit the localities rather than the persons. They must avoid fatiguing the patients with questions, speak chiefly of the ordinary business of life, and play the part of good listeners. All discussions with the patients, and all attempts to reason with them on the fallacy of their views, must be avoided. These are only a few of the maxims laid down for the guidance both of pupil and professor; but want of space obliges us to refer those interested to the paper itself. M. Falret states that he has always found the pupils conduct themselves as well as he could desire.—*Annales Médico-Psychologiques*, N. Ser., tom. i, pp. 524-80.

## S U R G E R Y.

### *On the Treatment of Gleet.* By Dr. CHRISTOPHER JOHNSON.

DR. JOHNSON regards the prostate gland as the chief source of the discharge, when this resists copaiba and the bougie. Having for several years employed a solution of nitrate of strychnia with success as a topical application in chronic ophthalmic catarrh, he was induced to try it in gleet unaccompanied by stricture, whether disease of the prostate was present or not, and has found great advantage from its employment. Further experience has led him to discover the great efficacy of the internal use of a combination of nux vomica and quinine; and he strongly recommends the following formulæ:—*Strychnia*, gr. ij; *Acid. Nitric. Fort.* gtt. iv; *Aqua*, ʒij. A drachm to be injected thrice daily. *Ext. Nux Vom.* gr. xij; *Sulph. Quin.*, *Ex. Hyoscy.* aa gr. xxiv in pil. xxiv. Two to be taken an hour before each meal.—*Amer. Journ. Med. Sc.*, N.S., No. 38, p. 542.

### *On Chloride of Sodium in Ulcers of the Cornea.* By M. TAVIGNOT.

M. TAVIGNOT published, seven or eight years ago, several cases exhibiting the great benefit he had derived from the application of chloride of sodium to ulcers of the cornea; and the subsequent experience of both himself and friends has strongly

corroborated the original statement. The usual formula is four parts of the salt to thirty of distilled water. The first effect is a sudden arrest of the progress of the ulcer, which, after remaining stationary for five or six days, then cleans and cicatrises. In obstinate cases he employs from six to eight parts of the salt. It should be dropped into the eye three times a day, and if the pain caused by it is rather more severe than that produced by the nitrate of silver, it lasts for a less period.—*Bull. de Thérap.* No. xxxviii, p. 228.

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*On Chloroform in Orchitis.* By M. BUISSON.

As surgeon to a venereal military hospital, M. Buisson has had ample opportunity of testing the value of the different modes of treating this painful disease; and he has come to the conclusion, that of all these, the local application of chloroform is by far the best, whether used in the simple, the blenorrhagic, or the rheumatic variety of the affection. It is chiefly in very painful cases that he resorts to it, after the use of leeches; but used as soon as pain appears, it may even act as an abortive. A compress of several folds is wetted with the chloroform and accurately applied to the testis, covering this with oiled silk, and placing the whole in a suspensory. The first day this is renewed every three hours, and continued the next day if required. For some minutes after it causes great heat and redness of the part, which is soon followed by a diminution of the original pain. The relief of pain is, in orchitis, the first step towards a cure of the disease; and with the proper combination of internal means this is accomplished in half the usual time, the chloroform being suspended as soon as an improvement is visible. The number of cases M. Buisson has thus treated now amount to about sixty, and he has found the remedy no less efficacious in what he calls ileo-scrotal neuralgia.—*L'Union Médicale*, No. 4.

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*Unusual Position of the Pneumo-gastric Nerve.*

M. GIRALDÈS related to the Medical Society of Emulation, that he had observed, with M. Cruveilhier, a rare anatomical anomaly upon a subject. The left pneumo-gastric nerve, instead of being in its normal position, was found in front of the carotid artery in nearly the entire length of its course; the nerve on the right side observing its normal relations. Such a deviation might embarrass a surgeon much in applying a ligature on the artery, unless the possibility of its occurrence was borne in mind.—*L'Union Médicale*, 1840, No. 57.

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*On the Treatment of Aneurism by Galvano-puncture.*

By MM. PETREQUIN and GIMELLE.

It is now some years since M. Petrequin advocated the treatment of aneurism by galvano-puncture, and he has left no means untried, as far as constant publication upon the subject has been concerned, of propagating the practice. He has, however, met with but indifferent success, which is not surprising, seeing that while the means has proved infinitely more uncertain in its results than the ligature, it requires, according to him, much tact in manipulating with the electricity, and causes the most intense suffering, which even chloroform, in some instances, has failed to prevent. It has been more frequently tried in Italy than elsewhere, chiefly in aneurisms of small vessels, and with only occasionally satisfactory results. We are not aware of its recent successful employment in London and Paris, although we understand M. Petrequin stating as much in one of his papers read before the Academy, wherein he makes no mention of the unsuccessful employment of electricity by Liston and Phillips, long before he himself had taken up the subject. Perhaps the best example hitherto published has been the successful employment of galvano-puncture in the treatment of a subclavian aneurism by M. Abeille, an account of which he forwarded to the Academy, upon which M. Gimelle has delivered in an able report. The patient was a lady, æt. 65, and in consequence of the ill-



success which had hitherto attended the ligature in this description of aneurism, M. Abeille resolved on resorting to galvanism. He employed twenty-two pairs of plates, of ten centimetres in diameter, and four needles. Notwithstanding that chloroform was given, the sufferings of the patient are described as having been horrible, several persons being required to restrain the consequent convulsive movements. After thirty-seven minutes of extreme suffering, the tumour was found to have become hard, resisting, and destitute of pulsation. Moderate compression was maintained above the tumour for ten hours. It had quite disappeared in thirty-seven days, and the cure, now three years old, has held good, the radial pulse being somewhat feebler than on the sound side.

M. Gimelle, in his Report, passes in review the various cases in which galvanism has been employed, and shows that several of these were unsuccessful in even M. Petrequin's hands, while other surgeons have met with suppuration and gangrene of the sac and other accidents. So impressed is he with the numerous dangers and ill-successes of this operation, that he wished the Academy to pass an absolute condemnation upon it. This, however, was considered as too extreme, since cases in which the ligature could not be employed, or had been so usually without success, might sometimes prove amenable to this procedure. This was the view taken by M. Velpeau, who having lost a patient from inflammation of the sac of a popliteal aneurism, for which he had employed galvanism, is naturally not prepossessed in favour of it. He believes that in cases analogous to that now related, it should be resorted to. M. Robert took the same view, believing it should be confined to cases in which the ligature is difficult or impossible. M. Laugier, judging from a case which had occurred to himself, in which the sac of a brachial aneurism was obliterated, considers that the operation may sometimes even be advantageously employed in aneurisms of the extremities.

For some of the details furnished by M. Petrequin of the progress of the investigation, we may refer to the '*Gazette Médicale*' and '*Bull. de Thérapeutique*' for 1846, 1847, 1848, and 1849, and to the '*Revue Médicale*,' 1846.—*Rev. Médicale*, 1850, vol. i, p. 138; *Bulletin de l'Acad.*, vol. xv, pp. 572-82.

#### *On Iodine Injections in Hydrarthrosis.* By M. VELPEAU.

WHEN recently dismissing two patients in whom he had successfully used iodine injections, M. Velpeau took a general review of the present state of the question of the propriety of employing them in dropsical joints, his observations forming a sort of Appendix to the celebrated Academic discussion, a few years since.

He observed, that the ill-success recorded by Boyer and other surgeons, as resulting from throwing injections into joints, much alarmed practitioners; the mischief which resulted, however, really depending upon the nature of the cases which were selected and the procedure adopted. M. Velpeau appealed against the abandonment of the practice on these grounds. For fifteen years he had employed iodine as an injection in hydrocele, without any dangerous inflammation being excited, so that he was encouraged to extend its application to various serous cysts; and in his '*Operative Medicine*,' published in 1839, he suggested that it might without danger be thrown into the peritoneal cavity, an opinion afterwards confirmed by its injection into congenital hydrocele and hernial sacs. It was only in 1840, however, that he ventured to inject the knee-joint in two instances, and that by accident, the tumour in the ham communicating with the cavity of the joint. The good success of these cases, and of others in which joints were purposely injected by M. Bonnet, of Lyons, led him, however, to adopt the practice in numerous other cases. At least fifty such, treated by different surgeons, are now on record, in none of which were any alarming symptoms developed. The dangerous and fatal consequences resulted, in Boyer's cases, from the joint being widely laid open, and irritating fluids several times thrown in; while in the modern operation the smallest possible trocar is employed, and, in the majority of cases, a single injection suffices. The pain and febrile action in some cases have been considerable, requiring for their

removal, in the opinion of some surgeons, leeching and abstinence; but M. Velpeau has always found them, as in the case of their appearance in hydrocele, if let alone, gradually disappear. Their occurrence at all is, indeed, rare. In general, a little redness, pain, and swelling are observed after the injection, and are soon followed by slight fever. Increasing during three or four days, these symptoms then become stationary; afterwards to diminish, and to entirely disappear about the fifteenth day, together with a part, or sometimes all the tumefaction.

M. Velpeau now considers there is not more danger in injecting a joint than the tunica vaginalis, the chief fear being, indeed, that the operation may fail. Injection, in fact, succeeds best in proportion as the cavity more nearly approaches a simple serous one. Thus, in cysts of the neck, where the membrane containing the fluid is everywhere surrounded by soft tissues, the operation never fails. In the tunica vaginalis, where the testis places the serous membrane in a less favorable condition, failures are sometimes, though very rarely, met with; while in the joints, in which the serous membrane is free only to a limited extent, and chiefly lies on solid or very hard parts, success is more rare. Still, as the employment of injection is not preventive of other means, is not dangerous, and scarcely more painful than a blister, we should always resort to it, when a simple collection of fluid resists ordinary treatment. As, however, such collections are usually capable of being dispersed, it is an operation not often demanded, and it should not be resorted to when the collection of fluid is dependent on caries, necrosis, &c. The fears entertained by some that ankylosis might result, are unfounded, the patient recovering the use of the part as soon as the swelling has disappeared.—*Gazette des Hôpitaux*, No. 58.

The French practitioners do not seem to have taken much notice of the writings of Dr. Borelli, of Turin, upon the employment of iodine injections in various affections, although he has been one of their most active advocates. In the last paper we have seen, he reiterates his opinion of their great utility, and furnishes additional cases. These consist of examples of *hydrocele* (he states, that hydrocele in boys and hydrocele of the cord in adults are always curable by simple puncture), *encysted tumours* after the evacuation of their contents, various forms of *acute* and *chronic abscess*, and *adenitis*. He objects to the use of resolvents, as being both tedious and useless. When matter is once formed, the abscess is rapidly cured by the injection. He relates, too, a case of *ranula* yielding to the employment of the injection, although simple puncture had repeatedly failed. Bouchacourt had, however, already published a similar case in the '*Bull. de Thérap.*' for 1843. After quoting a case of *fistula in ano* successfully treated by Van Camp at Angers, and relating one of his own, he states he has always hitherto failed in treating *fistulae* in connexion with diseased bone; but quotes cases from the veterinary practice of Professor Peroscino, in which the iodine was successfully used in fistula connected with diseased cartilage.

Dr. Borelli considers his practice in treating abscesses by this means as somewhat peculiar, inasmuch as he seeks to obtain union by the first or the second intention, according to the requirements of the case. The suppurative process being well determined, and the acute inflammation of surrounding parts repressed by cataplasms, after opening the abscess in its most dependent part, and evacuating the contents as far as possible, he introduces a small syringe through the aperture, and throws in pure tincture of iodine with some force, allowing it to remain in, when the pain is not too great, about half a minute. He waits three or four days to see whether the plastic effusion into the pyogenic cavity will effect its occlusion. This, however, is seldom the case, unless the abscess is very small and the engorgement of surrounding tissues slight. The injection, therefore, will require repetition every two or three days, according to the amount of reaction produced; and when this is in excess, emollient cataplasms are required. The author has never seen any ill-effect, local or general, following the employment of the iodine.—*Omodei Annali*, vol. cxxviii, pp. 79-154.

The question of injecting the abdomen with iodine in *ascites* is no longer one of mere hypothesis. M. Dieulafoy resorted to it three times in the same subject, with

the effect of producing a cure, and Dr. Leriche, of Lyon, has recently published two cases, in which a single injection after the complete evacuation of the fluid sufficed. M. Boinet also has recently presented a memoir to the *Société de Chirurgie*, in which he has collected eighteen cases of ascites from various sources, wherein different substances, as gases, water, iodine, &c., have been injected. In fifteen of these success followed, and only in one did the patient die, iodine seeming to be the especially preferable substance. M. Morel, reporting upon this paper, pronounced an almost unqualified opinion in favour of the practice; but MM. Vidal, Gosselin, Robert, and others protested against drawing any such hasty conclusions from cases the history of which had been imperfectly given, and have to be confronted with others in which a fatal termination has resulted, an example of which has recently occurred in Paris.—*L'Union Médicale*, Nos. 17, 18, 19, and 60.

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#### MIDWIFERY; &c.

##### *The Kite-tail Plug.*

THIS, which has long been employed by M. Bretonneau, of Tours, M. Trousseau regards as excellent in uterine hemorrhages, being both easy of application and withdrawal. It is formed of a thread about forty feet long, to which, at intervals of about six or seven inches, pieces of carded cotton (to be oiled before using the plug) are attached. M. Bretonneau prefers it to all other means of plugging in epistaxis.—*L'Union Médicale*, No. 25.

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##### *On Precocious Menstruation.* By M. PAUL DUBOIS.

WHILE drawing attention to a woman who had commenced menstruating at 9½, M. Dubois observed that warmth of climate is not the only circumstance influencing menstrual precocity. One of the most influential circumstances is the condition of the uterus itself, and this is especially seen from the fact, that both precocity and tardiness of menstruation are frequently hereditary, the influence of hereditariness upon the intimate structure, and even the outward conformation of organs being well-known. On several occasions he has found the menses become established very early or late, at much the same epoch in different members of the same family. Another influencing circumstance is the moral condition in which girls live. In towns and workshops, where young girls are constantly brought into company with individuals of the opposite sex, the menses appear much earlier than they do in girls who lead such different lives in this respect in the country.—*Gaz. des Hôpitaux*, No. 45.

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##### *Retention of the Placenta after Premature Expulsion of Child.* By Dr. M'SHERRY.

DR. M'SHERRY relates the case of a female, æt. 40, who expelled a dead seven-months foetus on the 26th of November, 1847; the funis, which was in a decayed state, breaking. As the placenta did not follow, ergot was given and repeated, without effect, at intervals for two days. There was little hemorrhage. Slight fever occurred, but in a few days she was enabled to follow her household affairs. On the 29th January, 1848, she was seized with a violent pain, and the placenta was soon expelled. She did very well.—*Amer. Journ. Med. Sc.*, N.S., No. 38, p. 400.

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##### *On the Influence of Iodine on the Development of the Infant.* By M. DELFRAYSSÉ.

IN a memoir recently presented to the Académie des Sciences, the author proposes, in place of diminishing the size of the child by subjecting the mother to a starving regimen, as recommended by Depaul, to accomplish this end by administering iodine

in the latter months of pregnancy, in minute doses. He orders 1 part of *Iodine* and 2 of *Iod. Pot.* to 30 of water, and gives from 6 to 8 drops of the mixture daily, some time before a meal. After testing its efficacy upon some female animals, he employed this substance in two appropriate cases in his practice. In the first patient, there was great contraction of the pelvis, necessitating premature delivery on former occasions. In two successive pregnancies she took, during the last two months, first, 6, and then 8 drops of the solution, every morning. Both children were born alive and did well, having all the appearance of seven-months' infants, one of them weighing 728, and the other 734 grammes less than former children had done. The only inconvenience was some diminution of development of the mother's breasts. In the second case, the patient had had five very difficult labours, none of the children living. The author ascertained that this arose from the narrowness of the pelvis, and upon the occasion of the next pregnancy, he administered the iodine. A living child was born, weighing 1250 grammes less than its predecessor.—*Bull. de Thérap.*, vol. xxxviii, p. 474.

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*Case of Therapeutical Fracture of the Coccyx.* By Dr. SIMMONS.

DR. SIMMONS was called to a woman in the last stage of labour with her third child, both the others having been lost during parturition. A dead child was soon expelled, having however a huge gash, as if the skull had been pierced by a sharp instrument. On examination the coccyx was found to present a nail-like point, which was turned firmly upwards and inwards towards the cavity of the pelvis. Called to her again during an earlier stage of her fourth labour, he endeavoured to draw the coccyx into a line with the sacrum; but finding this impossible, he resolved to break through the connexion of the two bones, and he succeeded in doing this by the firm application of both thumbs. An audible snap was heard, and the child was born (alive) with much less suffering than in former labours. Dr. Simmons attended the patient on two other occasions, on each of which he had greater difficulty in breaking up the connexion between the bones than before.—*Amer. Journ. Med. Science*, N.S. No. 38, p. 400.

[It is to be regretted that such meagre details only are furnished of such an unusual occurrence, no account being given of the period required for the reparation of the fracture, and the influence it exerted on the puerperal state.]

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*On Suppuration of the Thymus as a sign of Syphilis in the Infant.*

By M. PAUL DUBOIS.

THE effects of the transmission of syphilis *in utero* vary in respect to the epoch at which they manifest themselves, and the influence they exert on the health and life of the infant. This may be born apparently quite healthy, and the disease only reveal itself several days, weeks, or a much longer time after. It may again manifest itself during intra-uterine life, the child being born with obvious proofs of its existence, and succumbing some time afterwards, or surviving if promptly treated. Lastly, it is but too common for the infected fœtus to perish before birth. In the first two examples the nature of the case is plain, and the indications of treatment in case of fresh pregnancies obvious; but in the third we are too often left without this guide, since the death of the child may have resulted from very different affections, among which diseases of the appendages, and especially of the placenta, play a more important part than is generally believed. The fœtus not being usually expelled immediately after its death, such changes are effected upon both the skin and mucous membranes, as to render the detection of syphilitic lesions, even when these had existed, no longer possible, while death may be produced by the syphilitic poison without any of the usual pathological appearances. How little the statements of parents can be relied upon in these cases, every one knows; and the difficulty

of determining to submit one or both of them to an anti-venereal treatment, upon such uncertain grounds, is obvious.

M. Dubois believes he has discovered a special and characteristic pathological appearance in the *suppuration of the thymus gland*. Believing the appearance a mere coincidence, when he first observed it, several years since, in a well-marked case of infantile syphilis, he has since had in his hospital, in which numerous syphilitic women are confined, many opportunities of observing the like concomitance. On the other hand, the children who have died in the same establishment of causes quite unconnected with syphilitic disease, have never yet revealed this purulent condition. From what he has observed he thinks he is justified in concluding—1, that the presence of pus disseminated or formed into collections in the thymus of new-born children who have died of syphilis, should be regarded not as a simple coincidence, but as a result and evidence of the disease; and 2, that this appearance, in the absence of other explanation of the death of the fetus, should authorize us to prescribe that treatment which can alone prevent the return of a similar accident. He admits, however, that the subject calls for a much more extensive examination before these propositions can be absolutely affirmed, and hopes his medical brethren will lend him their aid in the investigation. One caution is necessary. The thymus secretes during the greater part of the fetal life a whitish viscous fluid, which by inattention may be mistaken for pus, especially as under the microscope this thymic fluid is found to contain mucous globules very analogous to those of pus. The fluid secreted by the thymus is, however, of an opaline, transparent white, very different when compared with the creamy, yellow, and opaque appearance of pus. From the pus generally being usually disseminated, and appearing in droplets when compression is employed, he believes its presence to be due to an inflammation of the canals and reservoir of the organ in question.—*Gazette Médicale*, 1850, No. 21.

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*On Engorgement and Deviations of the Uterus.* By M. PAUL DUBOIS.

THE discourse delivered by M. Dubois, during the late Academic discussion, was pre-eminently the best; and we regret that its great length prevents our presenting a fuller abstract of it.

Great discrepancy of opinion, he observed, prevails as to whether *anteversion* or *retroversion* is of more frequent occurrence; and from the attention he paid some time since to this question, he believes that perhaps there have occurred to him rather more cases of the latter than of the former. Confusion, too, has resulted from employing the terms *inflexion* and *deviation*, as if they were synonymous. The uterus may be bent upon itself without any change in its direction taking place, and, *vice versa*, although the two conditions may in certain cases be combined. Well-marked inflexion is usually congenital, existing in common with a series of other alterations; while deviation is generally accidental. Nevertheless an inflexion may occasionally be acquired, and even be produced, as a consequence of excessive deviation, being here, however, a mere secondary phenomenon. In true inflexion, the volume of the uterus is often less than normal, but the walls retain a proper density, while in deviation, followed by incurvation, the volume is often increased, the density diminished, and the sensibility augmented. Inflexions do not of themselves seem to induce any ill effects upon the general health; but by the obstruction of the menstrual flux to which they give rise, they frequently cause dysmenorrhœa. Even deviations and displacements, contrary to the general opinion, when not in excess, which they commonly are not, are almost harmless, unless the uterus itself, or the parts it comes in contact with, are the seats of inflammatory action. Long observation has convinced M. Dubois, that deviations of the womb are of such very frequent occurrence, that if they ordinarily led to the consequences they are said to do, and the means usually recommended for the prevention of these were had recourse to, a third part of the inhabitants of towns would be obliged to resort to the employment of such, or resign themselves to a hopeless sterility.

Moreover, he denies the agency of *engorgement* in the production of deviations, for if it were as operative as supposed, there could be scarcely a case of early pregnancy without deviation being produced. *Engorgement* is, in fact, not a primary circumstance, but an epiphenomenon manifesting itself in the uterine just as in any other tissue, which has been the seat of phlegmasia; especially when that phlegmasia, as in the case of the uterus, the amygdalæ, the testis or ovary, is very liable to reproduction. So, too, with regard to *simple or granular erosions*, to which Velpeau and others attach so much importance, they are phenomena consecutive upon a prior diseased state, and both as regards symptomatology and treatment are only of secondary importance. In the great majority of cases they have succeeded, not preceded, the mucous and purulent discharges of uterine affections attributed to their agency, and their artificial removal is not followed by a cessation of these.

M. Dubois regards a *uterine phlegmasia*, and generally a *catarrhal phlegmasia*, as the essential and primary pathological element in the great majority of uterine affections. But although originating there, the phlegmasia does not in some cases continue confined to the mucous membrane, but may involve the parenchymatous structure to a greater or less extent; and although whether superficial or deep-seated, it is usually confined to the cervix, yet occasionally it attacks the body of the organ, and gives rise to more or less *engorgement* of it also. In almost all cases, too, this uterine phlegmasia is produced by the operation of local causes, among which may be especially mentioned, abortion, difficult labour, insufficient repose after delivery, imprudences committed during the menstrual period, and immoderate sexual intercourse. But while making the foregoing statement, it is not intended to deny that there are uterine affections quite unattended with inflammation, which may yet give rise to many of the functional disturbances usually dependent upon it; and, indeed, Lisfranc, as well as most other pathologists, have admitted the existence of neuralgia of this organ.

The *treatment of chronic uterine phlegmasia*, was, some time since, based upon the belief of its tendency to induce a cancerous degeneration of the organ; and this it was which led Lisfranc to regard the epiphenomenon of uterine *engorgement* as the dominant and capital element of uterine disease, the removal of which was the primary object of treatment. At present it is believed that such degeneration only takes place in the predisposed, the phlegmasia merely rendering this organ the elective seat of its manifestation in such.

Although this phlegmasia may sometimes disappear under the influence of mere rest and appropriate hygiene, yet of all forms of chronic phlegmasia, it is, perhaps, the one most prone to become perpetuated when left to itself. This arises, on the one hand, from the limited amount of vitality with which the uterus is endowed, excluding those internal organic movements which in other organs may quickly resolve pathological phenomena; and on the other, from its frequent exposure to accidental or periodical excitement.

An important question presents itself as to whether the treatment should be entirely general, as recommended by MM. Baud and Gibert, or essentially local, as insisted upon by all the other speakers. It is rare for a phlegmasia to continue for several months without inducing a derangement of the general health, which, again reacting on the local malady, renders its cure more slow and difficult; and therefore, while in some exceptional cases local treatment may affect a durable cure, and in others general treatment will suffice, yet, in the great majority, it is to an association of the two means we must look—the relative importance of the one or the other varying with the temperament, mode of life, state of the disease, and other circumstances. In this last point of view, a great difference will be found between hospital and private patients; the habits and education of the latter having impressed upon the symptomatology of their uterine affections a particular character of nervous predominance, rendering their cure more difficult, and a resort to general means more urgent. If local medication, too, is applicable when the uterine lesions are evidently dependent upon a general derangement of health, it is especially called for in those numerous cases in which this is not primarily present. When uterine

affections have been prolonged, a disturbed state of the genital and digestive organs and of the nervous system is produced, manifesting itself by a semichlorotic condition, great impressionability to erratic pains, and a remarkable degree of emaciation and debility. A tonic and antispasmodic medication is here obviously indicated; while long-continued repose in the horizontal posture, so insisted upon by Lisfranc and his followers, should, unless severe suffering be present, be exchanged for active exercise, pursued, however, with discretion.

M. Gibert and others object to the use of *caustics* in the *local* treatment of this affection, as being useless and dangerous; but this arises from a want of consideration of the nature of the substances employed, and the pathological conditions requiring them. The irritation produced is superficial, the vitality of the tissues is low and their tolerance great, while the phlegmasia is of that chronic character requiring modification for the purpose of stimulating into activity the organic movements necessary for a cure. This cure by their agency is usually slow but assured; and their employment is one of the numerous examples of the success with which a more active phlegmasia may be substituted for a chronic one.

As regards the treatment of *deviations*, M. Dubois is of opinion, that *inflexions* are almost always incurable, but that they give rise, however, to little inconvenience, if not existing in an aggravated degree. Even *displacements* of the organ, when not in excess, and not complicated with phlegmasia, do not produce the symptoms so generally attributed to them. A sense of weight in the pelvis, that of a body tending to pass the vulva, or of a bearing down at the fundament, are not pathognomic of uterine displacement, but are found daily in cases of uterine phlegmasia, quite unattended with this, especially when the phlegmasia assumes a sub-acute form. We can, by pressure upon the inflamed cervix with the finger, at any time give rise to these sensations at will. When displacement of the uterus, therefore, does not exist in an excessive degree, pessaries, and similar means, so commonly resorted to, are not only useless but injurious. It is indeed for considerable *prolapse uteri* alone, that M. Dubois resorts to a pessary, employing then an ivory one. As he considers it essential for the patient's welfare, that she should be able to remove it every night and replace it every morning, it should not be sufficiently large to remain within the genital passage without external support, and he has contrived an ingenious form of applying this. In prolapsus, pressure from below will completely maintain the uterus in its normal position; but this is not the case with respect to *anteversion* and *retroversion* of the organ, in which, therefore, he never employs the pessary; but when they exist in excess, resorts to a modification of Hull's abdominal bandage. It is true that this does not correct the displacement; but by removing the weight of the superincumbent viscera in the erect position, it may prevent its increase. By the same agency the employment of this bandage proves of great utility in uterine phlegmasia, enabling the patient to take an amount of exercise so conducive to her recovery, which she otherwise often could not.—*Gazette Médicale*, 1850, No. 4.

## MATERIA MEDICA AND THERAPEUTICS.

### *Collodion in Erysipelas.*

DR. FREER, in a recent epidemic of erysipelas, applied collodion to the inflamed parts by means of a feather. Immediate relief and disappearance of the redness followed.

M. Briquet has recently employed the same substance in erysipelas of the abdomen occurring in a man æt. 40, who, while under the alum treatment for painter's colic, was seized with acute peritonitis, for which, among other means, 180 leeches were applied. The bites became surrounded with erysipelas, which rapidly extended over the abdomen, while a pleuropneumonia still farther added to the danger. The erysipelatous surface was covered with a layer of collodion. As the ether evapo-

rated, the patient felt a cool sensation, and a slight feeling of retraction of the skin, which at the same time became whitened. There were only a few points at which the disease seemed disposed to extend, and these were covered with collodion as soon as they showed themselves. In a week, the erysipelas in this way was quite got under, leaving the skin of the abdomen covered with the scales of the collodion.

M. Briquet is engaged in experimenting with this substance as an ectrotic in smallpox.—*Amer. Journ. Med. Sc.*, N.S., No. 38, p. 545; *Bull. de Thérap.*, tom. xxxviii, p. 322.

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*Amount of Morphine present in Opium.* By M. AUBERGIER.

AUBERGIER has performed a series of experiments with the varieties of *papaver somniferum*, cultivated in 1844 and 1845, in Algeria: from which he proves that the quality of the opium prepared therefrom, as estimated by its amount of morphine, depends partly on the variety of the plant, and partly on the maturity of the capsule from which it is collected. The milky juice is richest in morphine when the capsules are not completely ripe, and have a green colour; as soon as they have become yellowish-brown, the time for the opium crop has passed. Aubergier found that the opium from the white poppy yielded from 1.52 to 8.57 per cent. of morphia; that of the red, from 10.37 to 11.23 per cent.; and that of the seillette, from 14.78 to 17.83 per cent. The morphia was estimated by Payen's method; and as the solutions were decolourized by means of animal charcoal, a loss must have been encountered.—*Ann. Ch. Phys.* [3], xx, p. 303; and *Liebig's Report*, p. 479.

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*Adulteration of Quinine.*

STRESEMANN has observed adulterations of quinine with from 30 to 40 per cent. of salicine; Sckeyde, with from 10 to 18 per cent. of sugar of milk; and Winckler, with 40 per cent. of chalk.—*Liebig's Report*, p. 475.

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*On Subcutaneous Punctures in Articular Rheumatism.* By M. GUERIN.

FREQUENTLY joints which have become invaded by an attack of rheumatism long remain the seats of most obstinate pain. On a close examination we may assure ourselves that this pain is neither uniform nor general, but partial and localised at certain points. On handling the part we can even feel, opposite the immediate seat of pain, little knotty points which are exquisitely sensible to the touch. Such points exist even during the acute stage of rheumatism, but are much more easily recognised and isolated in the subacute stage. It is towards these points that the subcutaneous punctures should be directed, taking care, as in the ordinary application of the method, to raise a fold of the skin. The point of the instrument divides and liberates this tumefied and, so to say, indurated part; and the instant this is effected the pain ceases, and pressure can detect no trace of the nodosity thus destroyed. Whether a few drops of blood flow or not, the same result follows, so that the practice does not operate as an antiphlogistic. It is in fact only a *debridement*.—*Gaz. Méd.* 1850, No. 22.

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*On Chloroform in Nervous Affections and Cutaneous Diseases.* By M. DEVERGIE.

THE first case that occurred to M. Devergie, was one of obstinate hysteria, of the existence of which he was not aware until after the cure of a chronic lichenous eruption in the same patient. The hysteria was of the most violent and intractable character, yielding to no remedy, until, during a paroxysm, the patient was compelled to inhale a few drops of chloroform from a handkerchief. A peaceable sleep ensued, and the attacks never recurred. In various analogous cases, of somewhat



less severity, he has been enabled to effect the same results by giving twelve drops of the chloroform in a draught, it possessing a higher antispasmodic power than ether. So the ointment (4 parts to 30 of lard) rapidly relieves neuralgic pains.

In diseases of the *skin* it is of little or no use, as far as the treatment of the disease itself is concerned; but it gives great relief to certain symptoms, as the *itching* of lichen and prurigo, exerting little effect, even in this respect, in eczema and herpes. Even the relief of the itching, however, does much for the cure of skin diseases, by preventing scratching. Every one is aware of the horrid torment caused by *prurigo ani* or *puerendi*, and the torture at night in ordinary prurigo is dreadful. Not only does chloroform allay the itching, like camphor, but becoming volatilized, to a certain extent, it acts on the system at large, and thus assists in procuring sleep. As regards the local relief of the mere itching, perhaps camphor is equal to it. Like camphor, it may be used in all papular affections, but is of little use in others. M. Devergie generally prescribes 2 or 3 parts to 30.

[In pruritus, M. Cazenave prescribes 2 parts of chloroform to 20 of lard, ordering it to be well-mixed and kept in a wide-mouthed bottle. The employment of chloroform by other means than inspiration, has now been sufficiently frequent to exhibit its powerfully anodyne and antispasmodic properties, operating in some cases to permanent relief, when only given to mitigate a prominent symptom. We even find it recommended (*Arch. Gén.*, May, 1850) by M. Delieux, one of the professors at the Naval School, at Rochefort, as an *antiperiodic in ague*. Recollecting the febrifuge qualities attributed by some physicians to ether, and having witnessed the beneficial employment of small doses of chloroform in phthisis and pulmonary catarrh in the hands of M. Guillot, long prior to the discovery of its anæsthetic properties,—in which affections it not only relieved the pains in the chest, and the cough, but frequently moderated the hectic and night sweats, and thereby produced sleep,—M. Delieux resolved upon trying it in intermittent fever. The result, he states, of a limited number of observations, is to prove that though inferior to quinine and arsenic, it does possess considerable febrifuge powers. He given it in quantities varying from 12 to 22 grains (preferring employing it by weight to ordering drops) per diem, as a general rule, and on one occasion has gone as high as 37 grains. It should, before mixing with the water, be well shaken in a bottle with simple syrup, (1 part of chloroform to 15,) which much facilitates its solubility and administration. Well corked, the syrup will keep for use as wanted. The quantity to be given is divided into three or four doses, one of which is given every quarter or half hour, so managing that the last one shall be taken within three or four hours of the expected paroxysm. In very obstinate tertians or quartans, it may be required daily, the dose being increased on the day of the paroxysm. Decreasing doses should be continued some days after the last paroxysm.]

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*On the Solution of Iodine in Cod-liver Oil.* By M. FLEISCHMANN.

M. FLEISCHMANN states, that when he first met with prescriptions ordering two or three grains of iodine to be dissolved in four or five ounces of cod-liver oil, he was not aware of any work in which the proportion of iodine soluble in fatty oils is stated. Rubbing up one grain and a half with a few drops of the clear oil, he found it very soluble, and the dirty-brown colour which at first resulted entirely disappeared on the addition of the rest of the oil, so that no difference in appearance could be detected in the oil containing the iodine and in that contained in the bottle whence it was taken. Iodine being thus so very soluble in fatty oils, in concentrated solution of it might be kept in large establishments to prevent delay. —*Buchner's Report.*, Band iv, p. 254.

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*On the Ethereal Oil of Bitter Almonds.* By Prof. MITSCHERLICH.

OWING to the difficulty of entirely freeing this substance from prussic acid, physiological experiments hitherto performed with it have given very different

results. M. Mitscherlich has recently undertaken a series on rabbits, which confirm the observations of those inquirers who state that its dangerous effects are proportionate to the amount of prussic acid which still continues combined with it, and that when freed from this, and given in doses in which the unfreed oil quickly kills, it scarcely excites any more powerfully poisonous effects than do the other essential oils. The conclusions are:

1. Bitter almond-oil is a poison in large doses. It acts more feebly than ethereal mustard-oil, but more strongly than the ethereal oils of savine, cinnamon, nutmeg, fennel, turpentine, juniper, or balsam of copaiba. 2. It is reabsorbed in the stomach, and in part excreted by the kidneys and lungs. Given in small doses, it may, according to the experiments of Wöhler and Frerichs, be oxidised in the economy, and converted into hippuric acid. In larger doses the oxidation is incomplete, and a portion of the unchanged oil is found in the urine and expired air. 3. The most remarkable effects which fatal doses produce, are the rapid annihilation of voluntary motion and sensation; the respiratory movements and action of the heart, both of which are accelerated by this poison, still continuing. 4. It produces the same changes in the intestinal canal as the other ethereal oils do. 5. When not freed from prussic acid, it acts almost entirely by reason of its presence. The pure oil given in doses of a scruple, does not produce any essential symptoms, but in larger doses it acts as the other ethereal oils.

The fact of this oil being employed in the manufacture of confectionary and perfumery invests these conclusions with a practical importance.—*Buchner's Rep.*, iv, pp. 267-79.

*On the Destruction of the Odour of Musk by Camphor.* By M. FLEISCHMANN.

THE fact of musk, when mixed with some other substances (as *sulph.*, *antim.*, *aurat.*, syrup of almonds, wax, &c.), almost entirely losing its odour, has often been observed; but the attention of M. Fleischmann has been recently more particularly drawn to the subject, by his finding that a powder, composed of musk, camphor, and sugar, lost its odour after mixing. Repeating the experiment, he found that, as often as camphor was commingled with musk, it exerted this effect upon it; so, too, when musk was given with an oleo-saccharum, as cinnamon, &c., its odour became lost.—*Buchner's Reper.*, Band iv, p. 262.

*On Tannic and Gallic Acids.* By Dr. BUCHNER.

TANNIC acid has the fault of sometimes acting too astringently, while its effects are in general limited to the points where it is applied; as whether in the stomach, or at the surface of a wound, it immediately forms with the mucus, protein, &c., an insoluble compound, and does not gain access to the circulation. Part of it is, however, changed into gallic acid, which after the continuous use of tannic acid is found in the urine.

Gallic acid more resembles, in its pharmaceutical peculiarities, catechu than tannic acid. It has an acid rather than an astringent taste. It does not coagulate protein or mucus. It is readily reabsorbed from the stomach, enters the circulation, and is found in the urine. Homburger, of Carlsruhe, employs it in doses of two or three grains in hæmaturia, in the bloody diarrhoea of spotted fever, and in tubercular hæmoptysis, with excellent effect.—*Buchner's Rep.*, Band iv, p. 405.

[This chemical distinction may have some weight in it; but experience can alone decide the relative advantages of the two substances. We may mention, however, that Dr. Alison has recently published a paper ('*Lond. Med. Journ.*,' Jan. 1850) in which he speaks in the highest terms of the great benefit he has derived from the employment of tannic acid in a great variety of affections. The much more moderate price of this substance, if it be equally efficacious, is a very important consideration.]

*Prolonged Tepid Baths as Sedatives.*

M. ROSTAN, while ordering a tepid bath for two hours, to allay palpitation in a case of organically diseased heart, for which purpose digitalis had been of no avail, observed that he usually derives far more advantage from the employment of prolonged tepid baths as sedatives, than from the use of any internal medicines whatever.—*Gazette des Hôpitaux*, No. 43.

*External Employment of Liquor Chlorini.*

DRS. CRAMER and SCHNEIDER have published communications recommending the more extensive use of chlorine as an external application. The good he had seen result from the application of *liquor chlorini* to malignant pustule and similar affections, induced Dr. Cramer to try its effects in bad furunculous swellings, the progress of which was thus surprisingly expedited, and the extension of the ulceration much limited, as compared to what occurs under the use of poultices. So, too, he has derived great advantage in employing it in large abscesses and in buboes, the matter sometimes becoming resorbed; and, where this is not so, the progress of the case is still very favorably influenced. Great relief followed its application to the neck in a case of scarlatina, in a child, wherein suffocation seemed impending. He keeps compresses well soaked in the fluid constantly to the part. Dr. Schneider still more strongly recommends its use as a gargle in variolous diseases and in angina. He uses it diluted with water, and finds it exert a remarkable abortive power over variola, when affecting the tongue and throat, and angina in general.—*Casper's Wochen.*, No. 8, *Med. Zeit.* No. 8.

*Removal of the Bitter Taste of Quinine.*

Dr. THOMAS states that accident led him to the discovery that the bitter of quinine may be effectually concealed, while the efficacy of the drug is retained, by combining it with tannic acid; ten grains may thus be deprived of its taste by  $1\frac{1}{2}$  grs. of the acid.—*Amer. Journ. Med. Sc.*, N.S., No. 38, p. 541.

*On very minute Doses of Tartar Emetic, in Phthisis and Asthma.* By M. BERNARDEAU.

In vol. xxxi of the *Bull. de Thérap.*, M. Bernardeau gave an account of the great benefit he has seen derived from the administration of minute doses of tartar-emicetic in the hectic of phthisis. Since that period he has used it in other stages of tuberculization, and in several cases of asthma, with excellent effects. He gives from three to six pills in the twenty-four hours, each containing  $\frac{1}{16}$ th of a grain. By their use the cough, dyspnoea, and inordinate action of the heart become calmed, and in fact all the good effects of morphia, without its inconveniences, seem to be produced.—*Bulletin de Thérapeutic*, vol. xxxviii, p. 311.

*On the Incompatibilities of Iodine and Iodide of Potassium.* By M. DORVAULT.

M. DORVAULT believes that an enumeration of the chief substances with which iodine is incompatible may be of service. Among the metalloids, chlorine, bromine, sulphur, and phosphorus, and among the metals, antimony, copper, lead, mercury, bismuth, silver, and gold, combine with iodine, and give birth to compounds in which its dynamic action is more or less modified. To the above may be added, with the same remark, those with which soluble iodides are formed, as iron, manganese, and zinc. Among the incompatible chemical compounds are ammonia, with which it produces an explosive compound, and sulphohydric and cyanhydric acids, which it decomposes, transforming itself into iodhydric acid. Nitric acid oxidizes it. Moist sulphureous and arsenious acids, brought into contact with it, become super-

oxygenated while it is hydrogenised. It gives various results with the metallic oxides, properly so called, but usually produces insoluble iodides. From sulphates it liberates the sulphur, and seizes the metal. From the salts of antimony, copper, mercury, silver, &c., it forms insoluble iodides with their metals. Almost all organised substances may be considered incompatible, in consequence of the tendency which this body has to seize the hydrogen, giving rise to various compounds, of which iodhydric acid almost always forms part. Yet the iodine taken in this form into the economy retains in a great measure its dynamic action. Thus for the purpose of moderating the irritating action of iodine, opium or the salts of morphia, belladonna, &c., are often added; and experience teaches us, that in spite of the incompatibility of the alkaloids with iodine, the action of this substance is obtained. Still there can be no doubt that its activity is diminished from this cause.

The metals already named also produce insoluble iodides with *iodide of potassium*. If the iodine is in small quantities, potash is produced, and a double iodide if in large. The salts of these metals give rise to a double decomposition, producing a soluble salt of potash, and an insoluble iodide. *Organic* incompatibilities are much less frequent than in the case of iodine, as the avidity which this substance has for combination is already satisfied with the potassa. Apart from the acids, as the citric, acetic, and tartaric, there are not any organic substances manifestly incompatible with the iodide of potassium.

By reason of the incompatibility of silver with iodine and iodide of potassium, pills should never be silvered, nor should medicine be administered by means of silver spoons.

Some of the reactions may occur in the system itself, if the iodine be ingested or applied soon after other medicines chemically incompatible with it, and *vice versa*; and in some instances, the system remains impregnated for some days with the prior medicine. All those bodies which have the power of becoming localized in certain organs, and those which stagnate in the economy by virtue of their combination with its protein elements, among which are mercury, antimony, arsenic, &c., give rise to this phenomenon. Thus, if we give a preparation of iodine after a mercurial salt, we salivate. So, too, by external friction with iodine, after the use of mercurial ointment, or the *Empl. Vigo*, vesication of the skin is induced. In both cases iodide of mercury has formed, and besides this, caustic potash. Ignorant of this, many persons order a combination of iodide of potassium and mercurial ointment, whereas when the action of the two is required, we should employ the iodide of mercury, or associate the iodide of potassium with mercury already in a state of combination as a salt.

In some cases, the production of incompatible compounds by the administration of preparations of iodine, fulfils a well-defined therapeutical indication, as when they are given in the case of metallic poisoning.—*Bull. de Thérap.*, tom. xxxviii, pp. 404-7.

#### FORENSIC MEDICINE AND MEDICAL STATISTICS.

*On the Modifications which certain Parts of the Body undergo, in consequence of the Exercise of different Occupations, considered in their bearing upon the Medico-Legal Establishment of Personal Identity.* By M. TARDIEU.

M. TARDIEU furnishes, in this paper, a detailed account of the physical changes which the exercise of forty-eight different occupations impresses upon certain parts of the frame, and which, by indicating the fact of such employments having been followed, may serve, in some cases, to aid in the establishment of identity, and the detection of crime. We can only notice his general summary of the results of the investigation.

Considered according to their *nature*, the alterations observed may be arranged in four categories; viz. those proceeding from thickening of the epidermis, from changes in the structure of the skin, from a modification of the normal colour, and

from deformities. *Thickening of the epidermis* is the most common and direct effect of manual labour, whatever may be the nature of the procedures or the tools employed; and it is in the different forms which this thickening assumes, that the greater number of signs, distinctive of certain occupations, are to be found. It may vary, from a mere induration to a hard callosity, or prominent elevation, and it is important to observe, not only the differences in its degree, but also whatever is special and characteristic in its disposal. Thus, the simple thickening of the forearm of carders, and the hardened palm of drummers, blacksmiths, wheelwrights, locksmiths, and the like, differ much from the prominent, indurated, circumscribed callosity, as round sometimes as a corn, which is especially found in coachmen, hairdressers, copyists, or stonecutters; or the prominent and extensive indurations seen in different parts in washerwomen, engravers on metals, organ-players, turners, and joiners. In young workmen, too, whose skin is more delicate, in place of indurations, softer and reddish tumours are observed. Changes are not always confined to the surface of the skin. Softening, and sometimes destruction of the *dermis* may take place, and deep fissures be formed, as in bleachers, unladers of vessels, and polishers. These changes are sometimes brought about mechanically, as by the play of the polisher's file; sometimes by a permanent source of irritation, or the frequent contact of destructive substances. Thus, while immersion in river-water produces the *grenouille* in unladers, irritation is induced in the polishers by the vinegar, fatty oils, pumice-stone, and other substances they employ. In some occupations the *nails* are destroyed, as in the mother-of-pearl workers, and the spoon-polishers. The effect of a constant repetition of certain kinds of manual labour is also seen in the formation of tumours and cysts beneath the skin, and in its substance, in the unlader, the tailor, and the vermicelli-maker. Changes of *colour*, though observed less frequently, are nowise less characteristic, and are found in bleachers, dyers, burnishers, curriers, copper-workers, polishers, locksmiths, &c. As, however, changes of colour which much resemble each other may be due to very different causes, a mere inspection does not suffice, and chemical analysis, or other means, should be resorted to.

Besides these more superficial lesions, the exercise of certain occupations induces often true *deformities*, influencing sometimes a particular organ, at others an entire portion of the body, or even the constitution in general. In some cases there is a mere modification in the natural form of a finger or a nail. Thus the fingers of shoemakers, flowermakers, ironers, and glaziers have all analogous spatuliform shapes, but yet are very distinct from each other. In other cases the deformity consists in a change in the relative situation of organs. This is seen in the angular deviation of the fingers of nail-makers and cabinet-makers, the flexion of the fingers in ironers, and the serious contraction of the flexor tendons in nail-makers. More general deformities, affecting certain portions of the trunk or limbs, are observed in nail-makers, shoemakers, porters, tailors, and turners. This last description of change is of especial importance, in consequence of its permanence and its specific character, and also because it often produces true and incurable infirmities.

In reference to the object of this paper, the *seat* of these changes is as important to consider as their nature, this indeed giving them their distinctive character. Of the forty-eight different occupations examined, the *hands* in thirty-nine furnish, if not the only, the principal signs of their pursuit; several occupations, however, leaving their imprint on different parts of the body. In so large a number of different occupations, for distinctive signs to be furnished by the same organ, minute differences must be taken into account, and the most remarkable particulars alone indicated. The right hand is that generally distinguished; and when both hands are so, it is not uncommon to find a different description of alteration occupying the two. It is almost always in the folds of the flexion of the palmar surface, that the greatest degree of thickening of the epidermis is observed, and opposite the joints that the corn-like callosities are found. The entire hand, only its palmar surface, the whole or only certain of the fingers, undergo changes in the different occupations.

Peculiarities in the *feet* are much more rare, and are chiefly to be remarked in tailors and turners. Organ-players, who rest the instrument just above the knee, frequently have at this point a thickening of the epidermis of almost a bony consistence. The *arms* are chiefly remarkable in washer-women, carders, and metal-gilders. The *trunk* undergoes various deformities. These sometimes occupy the chest, as in the shoemaker, tailor, and brass-turner; sometimes the shoulder, as in the nail-maker, porter, and turner; or the hip, as in the mother-of-pearl-workers. Allusion is here made to characteristic deformities, and not to that arched form common to most artisans.

In order to justly appreciate these different changes, we should be familiar with the manœuvres of each occupation, and the habits of the artisan. The mere mode of holding the hammer differs in almost every employment, so that the nail-maker, cabinet-maker, joiner, book-binder, blacksmith, and stone-cutter present their distinctive signs. A no less striking example is given in the various descriptions of polishers. Not only must the nature of the tool be known, but the attitude of the workman also. So, too, in this respect the wearing out of clothes in certain determinate places has to be observed; and with regard to many occupations, as tailors, shoemakers, &c., information of this kind is sometimes of the greatest importance.

Some of these signs of identity are not of sufficient constancy and certainty to be considered as truly distinctive; while others, by reason of their fixedness and their peculiarity, are of true value. Those which consist in a simple modification of the epidermic secretion or coloration, disappear more or less readily under the influence of a temporary or continuous cessation from labour; yet even after a prolonged stay in hospital, traces of the occupation are often met with. The natural delicacy or roughness of the skin, strength or weakness of the constitution, the duration of the exposure to the effects of the occupation, and the adoption or neglect of precautions in certain employments, all tend to impress a character of inconstancy on these signs. Others, again, either because they are not sufficiently advanced, or are not special enough, appertaining in common to several occupations, do not possess sufficient certainty to be employed as medico-legal proofs of identity. Finally, there are others which are quite characteristic, being sufficiently constant and special enough to designate clearly and certainly, by the nature and seat of the alteration, the cause that has produced it, the kind of labour it is the consequence of, the tool employed by the artisan, and the attitude he has assumed—in fact, the occupation to which he belongs.

The occupations examined by the author are divisible into the three categories—1st. Those offering but *uncertain signs*, among which are wheelwrights, sempstresses, lead-workers, and vermicelli-makers. 2d. Occupations the signs of which are *certain but not constant*, as in carders, coachmen, hair-dressers, hair-workers, dock-porters; lace-workers, copyists, watchmakers, millers, mother-of-pearl-workers, bookbinders, and drummers. 3d. Occupations presenting *certain and constant signs*, as bleachers, laundresses, burnishers, nail-makers, shoe-makers, curriers, brass-workers, cabinet-makers, flower-makers, metal-gilders, engravers, organ-players, joiners, piercers of boot-holes, polishers, locksmiths, tailors, stone-cutters, dyers, turners, and glaziers.

It results from the study of this subject, that if characteristic alterations are sometimes absent, they are far more frequently present, and then constitute a certain means of recognising, as far as their occupation is concerned, the identity of certain individuals.—*Annales d'Hygiène*, tome xliii, p. 144.

#### *Case of Poisoning by Tincture of Cantharides.* By DR. NOALE.

It does not often happen that we have a case of poisoning related by the subject of it; but this one occurred in the person of Dr. Noale, who swallowed off an ounce of tincture of lytta (Venet. Pharmac.) in place of one of elixir of bark. This took place at seven o'clock in the morning, before setting out on a short journey. In half an hour he felt a general uneasiness, which seemed to originate in the stomach; this

soon increased, and was accompanied by prostration, slight shiverings, restlessness, and a desire to vomit. He drank off a large glass of water and a cup of coffee, without any relief. He endeavoured to proceed on his journey, but had hardly gone half a mile when he found he was obliged to dismount, so distressing was the sensation he experienced in the stomach, and so strong his desire to vomit. He took another glass of water, returned it, and pursued his journey, amidst an increasing nausea, distressing sensation in the stomach, gasping for breath, and a general chilliness. He was now compelled to return, and eagerly demanded more water, which was at once returned without any relief. He now, however, (two hours after swallowing it), first perceived in the matters vomited the taste of the *tinct. lytta*, and became aware of the nature of the case. He drank large additional quantities of water which was soon returned, still imparting the taste of the lytta. A friend inspecting the fauces found them abraded and blistered. He endeavoured to drink some Cyprus wine, but found the burning sensation along the œsophagus excited by it so intolerable that he only got down two ounces. He therefore now merely took additional quantities of water, and sought its rejection mechanically. When this happened, a considerable quantity of blood was also rejected. To relieve the intense sense of burning in the fauces and œsophagus, he took ice; but the vomiting now continued, sometimes accompanied with blood, and sometimes not.

Five hours after the poison had been taken, Dr. Roncali, a distinguished practitioner, saw him, and prescribed a drachm of laudanum in four ounces of *decoct. altheæ* and the application of ice along the course of the œsophagus and to the epigastrium, besides its internal use. Some hours after he had taken the remedy by spoonfuls, the vomiting became so continuous that nothing more could be taken by the mouth, and a scruple of laudanum was ordered in a clyster, and two hours after this the vomiting became appeased.

In the night he had fever, with intense thirst, but was able to retain the water he took to assuage it. He now too suffered from painful sensations along the urethra, pruritus of the glans penis, and temporary retention of urine; but this train of symptoms was soon dissipated by the application of ice to the urethra and perineum. About the middle of the second day he was seized with profuse salivation, the margin of the tongue and the gums becoming covered with aphthæ, and the teeth loosening just as after mercurialization. This class of phenomena increased during two days, but had quite disappeared by the tenth. By the third day after swallowing the poison, a sense of prostration of strength, with a feeling of emptiness of the stomach, and an irresistible desire for food, alone remained, the pulse having continued from the beginning deficient in power and frequency. The state of his mouth forbade solid food, but he found great benefit from some generous wine drunk from time to time. When his mouth had recovered, he took his usual food, and by the twelfth day was perfectly well.

He asks how can the progress of his case be explained, if the doctrine of the toxicologists is true, that tincture of lytta induces inflammation of the stomach.—*Omedei Annali*, vol. cxxv, p. 344.

. On *Kermes Mineral* as an *Antidote to Strychnia*. By M. THOREL.

M. THOREL, taking advantage of the practice of the municipal authorities in destroying stray dogs periodically by means of strychnia and nux vomica, instituted some experiments upon the antidotal power of kermes, having already observed the reactions which ensue on bringing a sulphuret in contact with strychnia. Although dogs commencing to exhibit the symptoms of strychnia-poisoning cannot, if they have been fasting, be made to vomit even by large doses of tartar emetic; yet by combining with it some kermes, free purging and vomiting are produced, and if the space of time has not been too prolonged, the animal recovers. He believes that the instances in which he tried it justify him recommending, that in case of poisoning by strychnia in the human subject, the following dose should be given:—*Kermes* 15 grains, *Tart. Emet.* gr. 1½, water and syrup of buckthorn, 2 oz. A second or even a third may be given.

A series of chemical experiments lead him to the conclusion that the action of the substance is twofold. A portion is decomposed, and forms, with the strychnia existing in the stomach as a lactate of strychnia, an insoluble sulphuret, while the undecomposed portion aids the tartar emetic in inducing expulsive action.

M.M. Bouchardat and Gobley, reporting on this paper, regard it as of some importance. They observe, however, that experiments out of the body show that the iodated iodide of potassium (*iodure de potassium iodurée*) exerts a far more powerful effect in precipitating an absolutely insoluble compound with strychnia, than kermes does. The relative efficacy of the two substances can only be tested by experience: and experiments on animals require to be extensively repeated, lest we may be deceived by exceptional circumstances. It is possible that all the advantages in M. Thorel's arose from the evacuations which were induced by the antimony and buckthorn.—*Journal de Pharmacie et de Chimie*, 3 Sér., vol. xvii, pp. 185-91.

#### Statistics of the French Lunatic Establishments.

ACCORDING to a recent law, every Department should possess its public asylum for pauper lunatics, join with another Department in providing one, or enter into an agreement with a private establishment. It seems that of the 86 Departments, only 60 have as yet complied with these provisions; of which number 35 have their own asylums, 12 have arranged with private establishments, and 13 provided accommodation in the hospices or poor-houses. The number which each establishment contains is not given; but it is stated that, on the 1st of January, 1849, the insane population of all the public and private establishments of France amounted to 19,765. Of the three public establishments at Paris we have the following account:

	1st Jan. 1849.	Admissions.	Cured or Improved.	Escaped.	Transferred.	Died.	1st Jan. 1850.
Salpêtrière .	1471	694	291	11	33	599	1231
Bicêtre . .	796	657	238	21	89	289	816
Charenton .	445	171	44	3	63	61	445
Total .	2712	1522	573	35	185	949	2492

The great mortality stated above arose, in a great measure, from the cholera, which severely ravaged the Salpêtrière. The admissions into the Bicêtre and Salpêtrière for 1849 thus amounted to 1351, a number approaching nearly to that of former years (1354 in 1848, 1230 in 1847, 1331 in 1846), and thus formally contradicting the assertion, that insanity had immensely increased since the last Revolution.—*Annales Médico-Psych.*, N. S., ii, p. 157.

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Om Forholdene ved flere af Udlandets Høspitals-Indretninger, hovedsagelig dem for Qvinder og Børn. Ved Dr. F. C. Faye. Christiania, 1850.

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Physician and Patient; or a Practical View of the Mutual Duties, Relations, and Interests of the Medical Profession and the Community. From the text of William Hooker, M.D. Edited by Edward Bentley, M.D. London, 1850. Fcap. 8vo, pp. 303.

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The Nature and Cure of Consumption, Indigestion, Scrofula, and Nervous Affections. By G. Calvert Holland, M.D. London, 1850. 8vo, pp. 206.

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PART FIRST.  
*Analytical and Critical Reviews.*

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ART. I.

*Die Operative Chirurgie* von JOHANN FRIEDRICH DIEFFENBACH. Zweiter Band.—*Leipzig*, 1848.

*The Operative Surgery* of J. F. DIEFFENBACH. Second Volume.—*Leipzig*, 1848. 8vo, pp. 864.

SINCE the First Volume of this work was analysed in the 21st volume of the 'British and Foreign Medical Review,' its author, with his great contemporaries, Lisfranc and Liston, has passed from among us. The last year of his life was actively occupied in the duties of surgeon and teacher, but he still found leisure moments to arrange and complete the greater part of the volume before us. It consists of Twelve Parts, ten of which were published before his death. The eleventh was nearly all printed, and the manuscript of the twelfth and concluding part was confided by the dying surgeon to his friend, Dr. Julius Böhrling, who prepared it for the press. Thus it has become a complete treatise on 'Operative Surgery,' with the exception of the subjects of lithotomy and lithotripsy. Upon these operations scarcely any notes were found among his papers. Dr. Böhrling tells us that, during the last month of Dieffenbach's life, he had a strong presentiment of approaching death, although, as he said himself, it was long since he had felt so strong and active. His greatest care at this time was for his book, and he often said to his friends, "I shall not live to see it completed." His forebodings proved but too true. In the performance of his duty, and surrounded by his pupils, the hand of death was laid softly upon him. He is gone; his book is his legacy to the profession. The man has entered the world of shadows; but his spirit he has bequeathed, by his pen, to us, and to those who shall follow us.

The whole of this volume is devoted to the Operations performed upon particular parts of the body: first, the head; secondly, the neck; thirdly, the thorax; fourthly, the abdomen; fifthly, the organs of generation; and

lastly, upon the limbs. Each of these divisions contains a number of chapters, which we shall now proceed to notice consecutively.

The "Operations performed on the Head," are treated in fifty-two chapters. Of these, twenty-six are devoted to the Operations on the Eye and its appendages, and were made the subject of two articles, in recent numbers of this review, in conjunction with the work of Desmarres on 'Ophthalmic Surgery' (see vol. IV, p. 30, and vol. V, p. 51). We need not, therefore, now allude to them further.

The first chapter of this section is on the Cephalhæmatoma of new-born infants. Free incision is recommended, including at least two thirds of the tumour. Dieffenbach says that he found the subsequent healing process to be much quicker, than when the small incision recommended by Chelius was practised.

In the succeeding chapter, upon Puncture of the Head in cases of Hydrocephalus, is a paragraph so thoroughly characteristic of the peculiar style of the author, that we cannot forbear extracting it:

"*Value of the operation.*—I think it of very little value. I therefore do not recommend it. In the sanguine days of my youth I performed it three times upon young children affected with chronic hydrocephalus—carefully punctured the head with a cataract needle, then applied bandages, but in all three death followed within eight days. Others, in whom escape of the fluid followed a small natural opening in the integuments which had been thinned by the inflammatory process, also died after a longer or shorter time. An operation should cure, not kill, and we should not, for the sake of experiment, scientifically destroy children, who might often enjoy their little existence several years. I will gladly bear the anger of physicians who support this operation, if I am only able, in any single case, to wrest the trocar from the hand of a young surgeon, just as it is hovering over a hydrocephalic head. Facts may be brought against me. It may be said, that one, two, three, four, or more surgeons have performed this operation successfully. In my youth, I read in 'Froriep's Notizen,' that a man who fell from a height and was pierced through by a railing recovered; a horse ran the shaft of a cabriolet completely through the thorax of a coachman, and the man survived; lastly, a sailor was cured after the anchor of a ship had passed through his abdomen. These are healing sports of nature, upon which man ought not to calculate. It is exactly the same, in my opinion, with the operation for hydrocephalus." (p. 9.)

Similar opinions are expressed in subsequent chapters with regard to the operation for the cure of fungus of the dura mater, and of encephalocèle, although palliative treatment by pressure is recommended in the latter disease. The operation of trephining, also, in cases of injury of the head, is recommended only in exceptional cases. Dieffenbach says, that of the many hundred cases of these injuries that have come under his notice, he has had reason to fear the operation more than the injury; and that in his early practice, when he frequently trephined, the proportion of recoveries was far less than after he had learned to act upon the following rules in all such cases, whether with or without fracture, stupor, convulsion, or extravasation:

"1. To make no incision.

"2. To bring together any existing wound in the integuments carefully with adhesive plaster.

"3. Never to trephine when extravasation is recent.

"4. Not to bleed to half so great an extent as in cases of penetrating wounds of the thorax, when it is necessary to maintain great depression for a short time, but to employ small bleedings at a later period.

"5. Penetrating splinters, as soon as they have become loosened, are to be removed by extraction through the smallest possible openings.

"6. Only to take away such pieces of bone as are deeply depressed, or penetrating into the brain. The surrounding borders should be cut away with the saw, not with the trephine.

"7. When depression is moderate, even on the occurrence of the accident, by no means to trephine, not even to make an incision.

"8. At a later period, when suppuration has commenced, only to make small simple incisions to evacuate the pus, and then extract the fragments of bone which have been spontaneously loosened." (pp. 19-20.)

Were we to discuss fully the above principles of practice, the space allotted to this article would be more than occupied with this subject alone. We therefore put our readers in possession of the opinions of our author without comment of our own, and propose to follow the same plan throughout the remainder of the article. The various steps of the operation are well described, much in the same manner as in British standard works on Surgery.

Three chapters follow, on Trephining the Frontal Sinus and the Mastoid Process, and on perforation of the antrum of Highmore; but we must leave these without further notice, and pass on to the chapters on Excision of the superior Maxilla. Dieffenbach states, that he has excised this bone entirely, or partially, to a varying extent, in thirty-two individuals, not one of whom died of the operation, or of its immediate consequences. Several of the cases are related, and among them one in which both superior maxillæ were excised. This patient, however, died of an apoplectic attack, fourteen days afterwards. The peculiarity of the method of our author is in the direction of the incisions which lay bare the bone. In this respect he differs from most French and English surgeons, and his plan, although at first sight objectionable, appears to possess numerous advantages. He cuts from the inner angle of the eye straight downwards along the side of the ridge of the nose, and divides the upper lip in the same direction. A cross incision is then carried above, so as to divide the inner commissure of the eyelids; and a flap is formed by separating the lid and cheek from the bone or bony tumour. If the tumour should not reach the edge of the orbit, the commissure is not divided, but the upper incision is made along the lower half of the eyelid to the temple, and the flap separated as before. In this way the bony parts to be removed are completely exposed; if any diseased portions remain they are visible, and may be cauterised; and bleeding vessels can readily be detected. These advantages refer to the operation, but those relating to the healing process are still greater. When the edges of the wound are carefully brought together by insect needles and twisted sutures, a simple thread-like cicatrix down the middle of the face is all that can be seen of the wound after a fortnight; and as division of the facial nerves has been avoided, the physiognomy is preserved. We all know, that after more ordinary methods, large cicatrices disfigure the mouth and cheek towards the ear, and that one side of the face is frequently paralysed. The latter is so serious an evil, that any plan by which it may be avoided, becomes worthy of attentive consideration.

The succeeding chapter is upon Excision and Exarticulation of the Lower Jaw-bone, and is of considerable length, a detailed account being given of the modes of proceeding, from the smallest removal of the alveolar process

to total extirpation of the bone. We have first excision of the alveoli, then of the portion of bone forming the chin, of the chin and one side of the bone, of the whole horizontal portion, of one side only by removal at the joint, and of the whole bone. Dieffenbach has performed one or other of these operations in twenty cases, and only one patient died soon afterwards. In five cases, osteosarcoma, which had been removed, returned, and was not cured by a repetition of the operation and application of the cautery. In the other cases the result was favorable. Some of the most remarkable cases are detailed, and among them one in which four successive operations were successfully performed at intervals of two months, for the removal of an enormous enchondroma. There is nothing peculiar in the methods pursued by our author, except that he uses the actual cautery very freely, to stop hemorrhage from the surface of the divided bone. The saw he prefers is of the shape of a knife with a slightly convex cutting edge.

The succeeding chapters are upon Opening the External Auditory Meatus in cases of Imperforation; upon Perforation of the Membrana Tympani, an operation which Dieffenbach regards as but of temporary utility; upon Catheterism of the Eustachian tube, of which he does not speak from personal experience; of boring the Earlap, which is performed in Germany, not merely for the purpose of inserting earrings, but as a method of counter-irritation in ophthalmic diseases; and upon Extirpation of the Parotid Gland. The last operation, Dieffenbach says, he has *often* performed, in cases of enormous enlargement of the gland, but does not give the number of cases. He has never thought of tying the carotid before proceeding to dissect out the mass; indeed, he says, he considers the ligature of this artery the more dangerous of the two; his mode of extirpation is by incision and careful dissection. He objects to the plan of putting a ligature round the base of any firmly adherent portion of the tumour, as he says the parts are so richly supplied with nerves, that tetanus is very apt to follow any such proceeding.

Short chapters follow upon extirpation of the submaxillary gland, division of the frænum linguæ, separation of adherent tongue, and the operations for the cure of ranula. A seton of silk, passed through the sac and tied loosely, when worn for several weeks, is said to be the easiest and best mode of obtaining a radical cure. It must be renewed from time to time. In the chapter upon Excision of portions of the Tongue, a mode of operating is described, which we can state from experience to be worthy of general adoption. The patient sits with the mouth widely opened, and the head supported by an assistant. The tongue is seized near its anterior border by a pair of hooked forceps, and drawn forwards. The assistant takes the forceps and keeps the tongue extended. A ligature having been prepared, about a yard in length, of six thick silk waxed threads, provided at each end with a large curved needle, it is passed from below upwards, through the tongue, from half to a whole finger's breadth from the borders, first on one side and then on the other. The needles are then removed, and the ligature so arranged, that while four or five inches from the ends are brought on each side from the upper surface of the tongue, the middle portion hangs as a loose loop below the chin. The assistant, who holds the forceps in his left hand, takes with his right one side of this loop and the corresponding end of the ligature. A second assistant does the same on the

opposite side, and both drawing in opposite directions, stretch the tongue laterally. The surgeon now takes the forceps in one hand, and cuts away the parts he wishes to remove. Then the ends of the ligature are drawn up until the loop comes close to the under surface of the tongue, and a knot is firmly tied upon the upper surface. Thus the principal suture is applied at once, and the ends serve to hold the tongue forwards, while other sutures are applied until the surfaces are completely united. All this, which appears so long in description, is very quickly completed, and both primary and secondary hemorrhage are effectually guarded against by the first strong ligature.

There is nothing in the following chapters upon Excision of the Uvula and Tonsils, the Operations for Salivary Fistulæ and Epulis, at all peculiar to Dieffenbach. A long and useful chapter is inserted upon the Extraction of Teeth, but for this we conclude, from the last paragraph, he has been indebted to a distinguished dentist, Dr. Hesse. Then, after some remarks upon the transplantation of teeth, follow the chapters upon Ophthalmic Surgery, to which we have previously alluded, which conclude the first division of the volume.

The second division, Operations on the Neck, commences with a chapter upon the removal of Tumours from this region; and others follow on Bronchotomy and Œsophagotomy, but we find nothing in them calling for especial notice.

The third division, Operations on the Thorax, opens with two conjoint chapters on the Extirpation of the Mamma and Axillary Glands. Rust and others, when removing a schirrus mamma, removed at the same time a considerable portion of the integument, and left the wound to heal by granulation, with the hope that the profuse secretion from the exposed surface might carry off any morbid matter existing in the blood. Dieffenbach says, that this idea appears a good one, but practice is quite opposed to it. At first the wound suppurates healthily, but as concentric cicatrization goes on, the surrounding skin becomes irritated and inflamed, the cicatrization of the edges stops, and in the best cases the wound becomes an obstinate ulcer with hard red borders. In these he has often seen hard schirrus developed, while the base becomes fungous; or, as more rarely happens, is converted into a hard, carcinomatous, bleeding ulcer. After healing by the first intention, should cancer reappear, it generally does so in some other part of the body; after healing by granulation, in the same part. Dieffenbach also objects to the plan of keeping issues open, before and after operations for cancer. The morbid poison in the blood is not discharged by the suppurative process, and can be no more mitigated or neutralised in this way than the poison of syphilis. An issue keeps up an irritation of the lymphatic system which is always injurious. The issues themselves are also often found to have taken on a carcinomatous character. There is nothing peculiar in his manner of extirpating these glands.

The following chapter is on the Operation for Empyema. Dieffenbach prefers simple incision to the trocar, as being less likely to wound the lung, should any mistake in diagnosis have been made, and a portion of lung be adherent; or should there be a mere layer of fluid between the opposed surfaces of the pleura, a condition not easy to discover before operating. When he has the choice, he makes the incision on the right side between the fifth and sixth, and on the left between the sixth and

seventh ribs, about half way between the sternum and vertebral column. He has performed the operation in thirty-six cases, and nearly two thirds recovered. Two of his patients were still strong and robust, sixteen and twenty years after being operated on. He says, that in general the result was satisfactory when the empyema was so evidently characterised, that any one could recognise it without any especial examination, and when the quantity of pus was very considerable, and escaped with some force and rapidity, on the opening being made. In cases of small collections of pus which escaped more slowly, the success was not so great. The thicker the pus, the more successful the operation. In every case in which Dieffenbach punctured the thorax on account of hydrothorax and hydropneumothorax, the patients died. He argues, that in cases of large purulent collections, the lung is compressed, adherent, and so far destroyed, that fresh inflammation is not excited by the operation; while in smaller collections disease is still going on, new inflammation is set up by the entrance of air, increased secretion takes place, and the patient dies. When the collection is simply serous, it depends upon general dropsy, and after evacuation the pleura becomes inflamed, the secretion purulent, and fatal hectic generally supervenes. When the blood has collected in the pleura after penetrating wounds, it is safer by a rigid antiphlogistic system to endeavour to procure its absorption, than to remove it by operation. It is only when the blood has become decomposed, and the pleura is secreting purulent matter, the original wound being in such a situation that free discharge cannot take place, that a second opening should be made.

In a chapter which follows, on the treatment of penetrating wounds of the thorax, immediate closure of the wound by strong needles and the twisted suture, with copious bleedings and antiphlogistic treatment, is strongly insisted upon. Lint and plaster do not suffice to close the access to air. In gun-shot wounds, the bruised edges should be cut away elliptically, and the new simple incised wound united as above directed. Perfect cleaning of the wound from clots of blood is the only preliminary measure to be adopted, as more harm is likely to result from efforts to tie wounded intercostal or mammary arteries, than from allowing them to stop spontaneously. The great object is perfect closure of the external wound, and, for a time, by repeated bleedings, to keep the pulse low and the face pale. On these principles, Dieffenbach states, his practice has been most successful.

Three chapters, of which we need only give the subjects, complete this division. They are on Trephining the Sternum, Puncture of the Pericardium, and Excision of portions of the Ribs.

The fourth division, Operations on the Abdomen, opens with the subject of abdominal suture, confining the term *Bauchnaht* or *Gastroraphia* to any suture employed to close wounds in which the peritoneal sac is opened, whether by accident or surgical operation. The common fear of this suture is ridiculed by our author, and its great utility insisted on; he believes further, that the danger is about equal, whether the peritoneum itself be or be not included in the suture. He uses either the simple interrupted suture, the twisted suture, or a combination of the two. The twisted suture is applied on insect needles about three inches long, and as thick as a knitting needle. Thick white cotton wick is preferred to silk or thread. In most cases the most perfect adaptation of the wound

is effected by a common suture above and below each twisted suture, the former not being passed so far from the edges as the latter, but the depth of all must be equal, or the edges will not be perfectly brought together. If one is passed through the peritoneum, all the rest must be so likewise. Long strips of plaster are passed from the vertebra on one side quite round to the other, to support the sutures. The sutures should be removed about the fourth or fifth day, as there is danger if they are allowed to cut through the peritoneum. After one is removed, a strip of plaster is applied, then another, and so on, in order to avoid sudden stretching of the cicatrix. The plaster is worn for at least a month, and then an elastic bandage. This careful adjustment of the wound leads to union by first intention, and is the best safeguard against inflammation, and the occurrence of ventral hernia.

After rather a long chapter on Paracentesis Abdominis, we have another on Abdominal Sections for the removal of fluids and foreign bodies from the Peritoneal Cavity, the relief of internal strangulation of intestine, the extirpation of tumours, and in cases of extra-uterine foetation. The operation is described in general, and its modifications in each of these cases, but we find nothing requiring further notice here. True Gastrotomy follows, the opening of the stomach itself for the removal of foreign bodies, or to form an opening through which the patient may be fed in cases of closure of the natural channel. The only case in Dieffenbach's own practice alluded to, is one in which he cured a gastric fistula, which had occurred spontaneously, by the repeated application of the cautery. Enterotomy, for the purpose of forming an artificial anus, having been treated in the first volume among the plastic operations, the remarks here are confined to its application in cases of foreign bodies fixed in the intestinal canal, when they have produced dangerous inflammation, and their position can be accurately ascertained. The wounds in the intestine and abdominal parietes, are to be closed by separate and distinct sutures. This leads to a chapter on intestinal suture—*Enterorrhaphie*. Dieffenbach insists most strongly on the principle that no mode of uniting the edges of a wound in the intestine can be a good one, unless the free surfaces of the peritoneal coat are brought together. It will not do to allow mucous membrane to protrude between the edges of the serous membrane, still less to attempt to unite opposed surfaces of serous and mucous membrane to each other. He argues, that when properly applied, the suture does not excite general inflammation, but that the opposed surfaces of serous membrane unite, the threads cut through the portion they include, and fall into the canal. We extract his account of the mode of applying the suture in cases of simple wounds of the intestine, in order to unite the serous surfaces together :

"To apply the suture, a fine, round, ordinary sewing needle must be used, and a very thin, waxed silk thread. The portion of intestine is fixed between the thumb and forefinger, after having been perfectly emptied by stroking downwards. The needle is then inserted before the commencement of the wound through the serous membrane, is passed between the muscular and mucous coats about three lines in length, and again brought out through the serous membrane. A puncture is then made, at the distance of a line or a line and a half from the point where it was first brought out, and from the edge of the wound. The needle is carried between the membranes, and over the wound, and the opposite edge is punctured. The third puncture is made from this side in the same manner, and so on from side to side, until the other end of the wound is arrived at. On both sides of the wound are now seen the separate interrupted stitches, when the sewing is to be carried on



backwards, so that the free interstices may be likewise covered, and both ends of the thread lie at the commencement of the wound. During the sewing, an assistant keeps the edges of the wound bent inwards by a pair of forceps. The thread is not drawn up very tightly, in order that it may not cut through quickly. Lastly, the ends are knotted together and cut off close to the knot. The intestine is then immediately returned into the abdomen." (p. 458.)

The method of Dupuytren, which Dieffenbach has also followed, is simpler than this, and perfectly answers the purpose of uniting the peritoneal surfaces together. An assistant, by means of the thumb and forefinger of each hand at either end of the wound, fixes the intestine, turning the edges of the wound inwards, and thus bringing the serous surfaces in apposition. The surgeon now applies the uninterrupted suture from one end of the wound to the other, the punctures being not more than a line from each other. As the edges are turned inwards, and the thread is passed between the muscular and mucous coats, adhesion of the serous coat is obtained as perfectly as by the method first described. These are the only plans by which, in Dieffenbach's opinion, protrusion of the mucous membrane, and escape of feculent matter, can be certainly prevented.

Passing over two short chapters on Puncture of the Gall Bladder and Extirpation of the Spleen, we arrive at the most important chapter of this volume. It is on Herniotomy, and occupies upwards of two hundred pages. It is divided into two parts; the first, on the Operations for Incarcerated Hernia; the second, on the radical cure of reducible Hernia. The first part commences with observations upon the danger of delay in cases of strangulation, the harm done by too long continued or rough application of the taxis and its auxiliaries, and the exaggerated importance attached to the epigastric artery by many surgeons. Dieffenbach says, that he has operated upon more than seven hundred herniæ, and lost a certain proportion of his patients, yet not one has died from loss of blood. Although he examined most of the dead, and found inflammation, exudation, gangrene, perforation, and other well-known appearances, yet he never found a large collection of blood in the abdomen. He uses the simplest instruments, and rejects all kinds of sheathed knives as dangerous.\* In some remarks upon the difficulties attending the operation, he gives amusing accounts of cases in which he overcame the opposition of patients or their friends. The stories are capital as told in his lively manner, but they would be spoilt by condensation.

The operation upon strangulated inguinal hernia is first described; each step of the operation—the first incision, the laying bare and opening the sac, the division of the stricture, and the return of the intestine—being separately considered in detail. With regard to returning omentum, such as is free and healthy only is returned. Such as is hardened, adherent, or degenerated, is cut away about half an inch below the external ring, several thick sutures having been previously passed through its base and loosely tied. Adhesions within the ring are not to be broken up, as this would lead to danger of internal hemorrhage. The sutures which prevent the return of non-adherent portions, are fixed by strips of plaster at some distance

\* In a subsequent chapter, he gives the number of operations as upwards of six hundred and fifty, and the deaths as forty. He divides the stricture with a narrow, curved, blunt-pointed bistoury, the edge of which is blunted to such a degree, that, although it will divide the fibrous tense stricture, it would not cut through an artery. With proper care, he regards injury to the epigastric as impossible.

from the wound. The danger of bleeding internally from varicose veins of diseased portions of omentum, is thus avoided. Only arteries of considerable size require ligature, as bleeding to some extent should be encouraged from a part so near the inflamed intestines. After all hernial operations, the wound is filled with charpie. Healing by the first intestine is never permitted, on account of the danger arising from the entrance of the secretions of the sac into the general peritoneal cavity. Antiphlogistic treatment is followed, and an emulsion of castor-oil with cherry laurel water, and enemata of water-gruel, are given from time to time, until the bowels are freely opened. All cold drinks are to be avoided, and for four or five days lukewarm gruel alone is allowed. The diet is altered with great caution, and a strong recommendation is given to avoid calomel, which is said to produce inflammation of the injured portion of intestine, and render the wound very unhealthy and slow to heal. Several cases are related in illustration of these principles.

With regard to the question of dividing the stricture without opening the sac, Dieffenbach says, it is a plan which can only be followed in exceptional cases, and not adopted as a general rule. Without opening the sac, adhesions of the protruded parts to each other cannot be destroyed; if returned, they are so while in a state of unnatural connexion. The last resource of nature, the formation of an artificial anus, is thus prevented. Even in cases of small recent hernia, the intestine may be adherent to the neck of the sac, and returned with it still in a state of strangulation. The secretions contained in the sac, also, on being returned into the abdomen, may excite fatal inflammation; and it is plain, that if a gangrenous or perforated intestine were so returned, the result must be fatal. The arguments in favour of this operation, that the epigastric artery and the intestine are not in such danger of being wounded as in the ordinary proceeding, fall to the ground, because injury either to the artery or the intestine is the fault of the surgeon. The opening of the sac which communicates with the peritoneum, and consequent danger from the admission of air into the cavity of the latter, is not so injurious as long-continued pressure and bruising of the sac and its contents. By the ordinary method, the diseased parts are brought into view and treated accordingly; in the other plan, the surgeon works in the dark, and returns a tumour, of the condition and contents of which he is ignorant.

In certain exceptional and extreme cases, however, it is of great advantage not to open the sac. 1st, in very small and recent; and 2d, in very large and old herniæ. Of the first, the most favorable cases are those of femoral, and of the second, very large scrotal hernia. Again, in cases of umbilical and ventral hernia in old persons with a very large abdomen, it is of great consequence to avoid the danger of the entrance of air, and of the secretions from the wound, into the peritoneal cavity. Guerin's attempt to perform a subcutaneous operation is regarded as most dangerous, and by all means to be repudiated.

A number of practical remarks, illustrated by cases, follow upon the operative proceedings in cases of hernia without sac, whether congenital or acquired; and in those of an empty sac, or rather of a sac which contains neither intestine nor omentum. Some interesting examples are given of the latter condition, the neck of the sac having been closed by the continued pressure of a truss, and become adherent, while fluid collected

in the cavity, and was scarcely to be distinguished from hydrocele of the cord. Incisions were made, and the sac filled with charpie; and it closed by granulation from the bottom. Various modes of procedure in cases of strangulation of inguinal hernia under unusual circumstances, are fully detailed, whether required from peculiarity in the condition of the external coverings, complication with superficial abscess, fatty or other tumours, or inflammation of the deeper tissues which has extended to the skin; or from alterations in the sac by thickening, or degeneration from the state of its fluid contents, or the presence of blood; also in various conditions of the intestine and omentum, and in cases of wounded intestine. The coexistence of hydrocele and of scirrhus, degeneration of the cord, and tunica vaginalis, is then illustrated, and the means of stopping bleeding after the operation described. The subject of inguinal hernia is concluded by remarks and cases upon acute and chronic strangulation of very large scrotal hernia, upon the return of hernia and sac together by the taxis, the contents still being in a strangulated condition, upon the coexistence of one or more hernial sacs, and the treatment of gangrenous intestine. The whole of this section, though well worthy of translation, and full of the results of long practical observation and experience, is not adapted for analysis. We would, however, recommend it to the attention of our weekly contemporaries.

With regard to the operation of femoral hernia, we need merely observe, that Dieffenbach regards Gimbernat's ligament as the most common seat of stricture, and divides it by a notch rather than by an incision. He particularly cautions us against deep incisions, and in cases where a notch in Gimbernat's ligament does not sufficiently enlarge the ring, he makes several notches in other situations, rather than deepen the original cut.

In cases of operation in umbilical hernia, he advises us always to endeavour to divide the stricture and return the intestine without opening the sac if possible, the danger being much less than if even a very small opening be made in the sac. Should it be necessary to open the sac, the wound is to be united by first intention; for even if some of the secretions of the wound enter the peritoneal cavity, there is less danger than from the continued entrance of air, and from the partial entrance of the secretions of the wound, altered by exposure to the air, which can never be entirely prevented. As much care should be taken in perfectly closing the wound, as in cases of penetrating wounds of the abdomen from accident; but Dieffenbach says that, notwithstanding all his care, by far the greater part of his operations from strangulated umbilical hernia were followed by death.

An interesting case of strangulated hernia of the foramen ovalis is related in a letter to Dieffenbach, from his friend Dr. Romberg of Berlin. The diagnosis of this condition during life, has been considered by most writers on hernia as impossible. Dr. Romberg's *diagnosis* appears to have been little more than a *suspicion* in the case he relates; but he deduces from the symptoms in this and other recorded cases the inference, that when symptoms of strangulated hernia, without evident cause, coexist with phenomena attendant upon pressure on the obturator nerve, the diagnosis is clear. In his case and others, there was severe pain in the upper and inner part of the thigh; but in one case he records, this symptom was absent. It is clear, therefore, that although the coexistence of

the nervous phenomena with those of intestinal obstruction, may lead to a correct judgment as to the nature of the case, such coexistence is not universal in strangulated obturator hernia.

In succeeding short sections on perineal, ischiatic, vaginal, rectal, vesical, and uterine herniæ, we find no important original remarks. In the next, on the treatment of strangulated omentum in general, the same principles of treatment are advocated, as in strangulated inguinal hernia containing omentum. The chief peculiarity is the objection towards returning into the abdomen the stump or remains after removal of adherent or diseased portions, and the fixing the stump outside the ring by ligatures passed through it and fastened to the integuments by strips of plaster. The reason for this practice is, because, even after ligature of all the larger vessels that have been divided, and apparent cessation of hemorrhage, as soon as the omentum is returned, and the pressure of the ring and canal is thus removed, more or less dilatation takes place with consequent effusion of blood in the cavity of the peritoneum. Cases are related in which operations were performed on omental herniæ, not on account of strangulation, but to remove the hardened or diseased omentum which prevented the proper application of a truss. We are cautioned, however, against following this practice in cases of very large herniæ.

The subject of hernia is concluded by a very useful chapter on the radical cure of Reducible Hernia. The methods recommended both in ancient and modern times, are fully and fairly reviewed. From this review the inference is deduced, that no method hitherto practised is to be recommended. The patient risks his life, not to avoid imminent danger, but to avoid the inconvenience of wearing a truss. The danger to life is great, and even if this be fortunately escaped, in almost every case the truss must still be worn, so that the risk has been run to no purpose. Simple compression, with a good truss, will effect all that can be obtained by any so-called radical operation. In children, compression really is a radical cure. In adults, it is so in some cases, and would be more frequently so if it were carefully and properly applied.

The following chapter on Anal Fistula, need not detain us. In excising piles, Dieffenbach first passes a ligature through the base, then seizes the pile with forceps, and cuts it away between the forceps and the ligature, which is then immediately tied. By thus bringing the edges of the wound together, all danger of bleeding is avoided. He prefers excision in general to tying, but performs the latter operation in cases of small, pedunculated, internal piles.

The operations for the cure of prolapsus ani, are described at some length. Four distinct methods are followed, according to the nature of the case. The preliminary treatment in all consists in a dose of castor-oil the day before, and an enema of warm water immediately preceding the operation. The patient is bent forward over a table, the nates being separated by an assistant.

1. *Diminution of the anal opening by excision of folds around it.*—After returning the rectum, one of the cutaneous folds which surround the anus like radii is seized by a pair of sharp hooked forceps half an inch from the opening; and the fold is then cut away close within the opening by a pair of straight sharp-pointed scissors. Four, five, or even six strips are successively removed in the same manner, so that the anus is

surrounded by a wreath of bleeding rays. The more lax the skin, the broader must be the strips removed. The wounds must be at equal distances from each other, for when two are close together, the small intervening portion of skin is easily destroyed by suppuration, and one wound results, which is so large, that the subsequent cicatrix and stretching of the skin are injurious. Cold water dressing is then employed. In slight cases where the prolapsus only consists of one or two folds of mucous membrane which can easily be replaced, this plan is to be adopted, and it is generally successful. In more severe cases it is either useless, or but of temporary utility.

2. *Excision of wedge-shaped pieces from the anus* is recommended when this part is greatly weakened, but not entirely paralysed, and when the prolapsus consists of large, fresh, unaltered folds of mucous membrane, which only project during the act of defecation. When the projection is only on one side, but one wedge is excised; when it is on both sides, or when it forms a sort of ring, a wedge must be removed from each side. The prolapsus is returned; a sharp hook is passed within the anus as far as the edge of the part returned, and carried through the membrane and the first fibres of the internal sphincter ani; the hook is drawn a little outwards, and the wedge cut away with a small scalpel. When the prolapsus is not great, the wedge may be small; but in more severe cases, Dieffenbach has often excised two wedges, an inch wide on each side. The edges of the wounds are immediately brought together by strong sutures, and cold-water dressing used. The result is most satisfactory, the cure complete, and relapse never occurred in Dieffenbach's practice.

3. *Excision of parts of the anal ring and of the callous prolapsus* is indicated when the opening is very much widened, the prolapsus old and large, the folds of mucous membrane dense, firm, and hard, or loose and spongy, with large permeating veins, the patient having suffered from continual hemorrhage. The operations upon old and hard, or soft and spongy, prolapsus differ, but in both the part must be protruded at the time of operating. In the first class of cases, the projecting fold is seized by a pair of forceps, and a strong suture passed through its base; the ends are kept on opposite sides, while the fold is removed by a pair of scissors, in a semicircular direction around the anus; the ends are then brought together, and serve the purpose of fixing and drawing forwards the edge of the wound, while fine sutures are passed to unite them. This is all that is necessary in cases where the anal opening is not very large. When it is so, a wedge must be excised from it, in the manner described in the preceding paragraph, the two wounds forming together a T. In very severe cases, it is dangerous to do too much at once, and it is far better to complete the operation after two or three intervals. The convolutions having been removed, and the wound healed, after a month or so, when the patient is quite well, the wedge may be excised.

4. *Extirpation of the spongy prolapsus*, which consists of the convolutions of the loose sanguineous mucous membrane of the rectum, is performed in the manner just described, when the prolapsus is partial, and the anal opening narrow. When it is total, and large, the operation is first performed on one side—that of the greatest protrusion—in a different manner. The part is seized by a pair of curved forceps, resembling those called entropium forceps, and the convolution is then cut

away by scissors, close to the edge of the forceps. The edges of the wound are immediately closed by fine sutures, as severe hemorrhage often comes on. The part removed includes the centre of the prolapsus, and extends at one end to the skin surrounding the anus, and at the other to the mucous membrane of the rectum. The rectum is cleaned by injections of cold water, the patient kept in bed, and cold applications used. An opiate is given to prevent defecation during the first day. Sometimes this alone effects a cure; in other cases it is necessary to repeat the operation on the opposite side, after the patient has quite recovered from the effects of the first. Dieffenbach relates some remarkable cases, in which patients who had been extremely reduced by continual bleeding and suffering, were thus restored to perfect health.

5. *Cauterization*.—When the external and internal sphincters of the anus are paralysed, the anal opening is often so large, that a doubled fist can easily be passed through it, and the intestine can only be kept within it by some mechanical contrivance. Removal of parts of the prolapsed intestine would be useless in such cases, as fresh protrusion would continually take place. Excision of parts of the borders of the opening also proves useless, as the cicatrices soon become soft and yielding. The actual cautery, however, suffices to effect a cure, acting partly upon the nerves, and partly by the formation of contracting cicatrices. The anus being protected by the introduction of a ball of charpie, a cauterizing iron, of the size of a large walnut, is applied upon the skin surrounding the anus, and repeatedly carried slowly around the opening, so that an eschar is formed, an inch or an inch and a half in width, and the skin appears as if burnt to leather. The iron must be carried close to the line of connexion between the skin and mucous membrane. The charpie is removed from the anus, a smaller quantity introduced, and a wad of cotton-wool laid upon the eschar. The patient is placed in bed, and an opiate given. Cotton-wool is applied until separation of the eschar at the edges commences. Stimulating poultices are then used. After complete separation, the suppurating surface is dressed simply, and castor-oil given. When cicatrization is complete, the patient is perfectly cured. Dieffenbach has found the opening so much narrowed, that bougies had to be used for some weeks; but this is rare, and the general result is most gratifying, both to surgeon and patient.

In cases of carcinomatous degeneration of part of the rectum, Dieffenbach adopts Lisfranc's practice of excision of the diseased portion of intestine, but has modified some of the steps of the operation. He has performed it in thirty cases of patients of both sexes, who suffered from this degeneration in different degrees, not one of whom died soon after the operation. In some cases the disease returned three months afterwards. In one, a very large cancer, with destruction of the external skin, and perforation of the bladder, appeared within a month; but the larger proportion of patients continued well many years afterwards. Relapse is said to be much less frequent after removal of carcinomatous rectum, than after excision of the mamma, or other organs affected by carcinoma. Of course there will be much doubt whether all the cases considered as carcinomatous really were so. The operation is only recommended when the carcinomatous degeneration has not extended over the whole of the rectum, and when the neighbouring parts, especially the bladder, are unaffected,

the separation of the hardened intestine appearing to be possible. It is not to be thought of when the patient is exhausted by long suffering, and secondary glandular swellings have appeared in the prostate or groin.

The patient being prepared and placed as before described, the first incision is made in a mode which depends upon the condition of the skin surrounding the anus. When this is healthy, a deep straight incision is carried through the anus from over the os coccygis to the centre of the perineum, about an inch anterior to the anus. The skin, being fixed by a hook, is then separated from the intestine on each side, the external sphincter being left attached to the skin unless it be diseased. When, on the other hand, the anus and surrounding skin are diseased, they are surrounded, according to their condition, either by two elliptical incisions, or by a circular incision. When the skin on one side only is carcinomatous, this is surrounded by an incision, while on the other side it is merely separated from the intestine. The diseased portion of rectum being thus laid bare, it is fixed by a double hook, and carefully dissected from the surrounding parts, either by a scalpel or blunt-pointed scissors, until the borders of the healthy portion of intestine are reached. This is also loosened from its connexions, in order that it may be more easily brought downwards. The intestine is then cut half through in its healthy portion; a strong suture is passed through the upper border of the wound to fix it, and the remainder of the intestine being then divided, the diseased part is removed. Cold water is injected; and the divided edges of the intestine are fixed all round to the skin, the ends of the sutures being cut away close to the knots. Dry charpie is introduced into the canal, and fastened by a compress and T bandage; but should bleeding come on, it is to be stopped by a ligature if arterial, or by injections of iced water if parenchymatous. As soon as the danger of bleeding is over, dry dressing is to be applied. When the external skin has been preserved, and the intestine united to it, union is rapid; but when the skin has been removed, or it has been necessary to unite the intestine to the sides of the wound, because it was impossible to draw it down as far as the skin, the processes of suppuration and granulation must be encouraged by injections of camomile tea, and the application of charpie wetted with this infusion, the canal being often cleaned by injections, and the wound protected from the fæces by the charpie. The contraction of the cicatrix often leads to stricture of the rectum, but this is overcome by the use of bougies of wax, or gum elastic. In women it is necessary to use the catheter, as the access of urine to the charpie would give the wound an unhealthy character.

These operations on the rectum conclude the fourth section. The fifth, Operations on the Organs of Generation, commences with a long chapter on Division of the Prepuce for Phymosis, and on Circumcision; another on the cure of Paraphymosis, and a third on Amputation of the Penis follow; but we must pass them over without further notice. The palliative treatment and radical cure of Hydrocele are treated at considerable length. Dieffenbach prefers the radical cure by incision, to that by injection, as the general rule. We need only name the titles of chapters on excision of the clitoris, and of the nymphæ, and on puncture of the uterus for the purpose of evacuating blood or secretions contained in its cavity. That on Extirpation of the Uterus is worthy of some attention. The various

modes of extirpating the whole of this organ are described; but Dieffenbach says he has done it but once himself, and cannot think of his operation without shame. It *may* be successful, and a man *may* win the great prize in a lottery; but the risks of failure are about equal in the two cases. On the other hand, judging from his own experience, and from that of others, he regards a partial extirpation of the uterus, that is to say excision of its diseased vaginal portion, as one of the most successful operations of modern surgery. The result, in many cases, is more satisfactory and enduring than that of removal of scirrhus mamma, still more than that of open cancer of the breast.

"When I have operated on an open mammary cancer, the disease almost always quickly returned in the same situation, or in another. When I have extirpated the neck of the uterus, filling almost the whole of the vaginal cavity, the patient recovered, and always lived at least one or two years, and these were the most unsuccessful cases. Many lived much longer, some with and some without a return of disease." (p. 794.)

The neck of the uterus is seized by a pair of hooked forceps, and drawn forwards. If it can be brought to the external opening, it is removed by the knife. If not, a pair of long, curved, probe-pointed scissors is used. After removal, the part is examined, and if it should not appear quite healthy at this point of division, the speculum is introduced, the vagina cleansed, and the diseased surface cauterized by means of an iron of the size of a small walnut passed through the speculum, which is then withdrawn, and the vagina plugged with cotton wool. Dieffenbach has never seen a case of severe bleeding from the wound; and, should such a case occur, he would prefer the cautery to the ligature, which, he considers, becomes dangerous by exciting uterine phlebitis. When the operation is performed on account of fungus or varicose state of the veins of the neck of the uterus, the cautery is always required; in the first case, because relapse is so common; and in the second, because the dilated vessels are, by its means, most securely obliterated.

Dieffenbach says he has performed this operation in a great many cases, and the result has often exceeded his expectations. Of the cases which he relates, the following may prove interesting to our readers.

"In a fair, pale, flabby, unmarried person, the vaginal portion of the uterus was enlarged to double its natural size, and a firm purple fungus protruded through the mouth from the left lip. I performed the operation principally on account of the copious and irregular hemorrhage, and because I saw that dropsy was imminent, the feet often becoming œdematous. I was able to draw the vaginal portion tolerably well down, and then remove it with curved scissors. The cautery was applied to the wounded surfaces, and the vagina plugged. The reaction after the operation was so slight that no bloodletting was necessary. The whole after treatment consisted in repeatedly cleansing the vagina with camomile tea and lead lotion, castor-oil being used internally. In four weeks the wound was cicatrized. The neck of the uterus had again become rounded, and appeared but little shortened notwithstanding the great loss of substance. After some months, menstruation became always regular. The patient was rosy, she married, and bore a healthy child. Four years afterwards I again saw her in robust health." (p. 789.)

In the succeeding chapters on Extirpation of the Ovary, the methods of those who have performed the operation are described, rather as a warning to young surgeons than as an example to be followed. The Cæsarean section is treated at some length, and a few words on synchondrotomy,



"of historical value, as one of the greatest surgical errors," close this division of the volume.

In the last division of the work, consisting of forty pages, the subject of Amputation is treated in general, and the various amputations and exarticulations of the upper and lower extremities are described. Our space does not permit us to give any analysis of this division, but we may show that Dieffenbach, as a general rule, prefers circular to flap amputations; that he brings the edges of the wound together as soon as bleeding is arrested, rather than waiting for some hours, as recommended by Liston; and that in selecting the period for amputation after accident, he would choose the earliest possible moment after recovery from the shock the whole system has experienced, and before reaction has occurred. When once febrile reaction has supervened, he would wait until free suppuration was established.

Our task is now concluded, and we do not lay down the pen without a certain feeling of melancholy when we reflect, that the hand which has been employed for our instruction will be so no more,—that the spirit which directed the hand in the hour of thought and the moment of action, has returned to the "God who gave it." Under the influence of this feeling we have made a simple rather than a critical analysis of a book which may be regarded as the last testament of a great man, and have left his opinions to be judged by our readers. The work is far from being perfect or faultless. Its various parts are of very unequal value. There is, above all things, a great want of exact numerical information as to the result of various operations performed by the author; but, taking it as a whole, considering, especially, the great value of the long section in the first volume on plastic surgery, we do not hesitate to characterise it, for minute practical information, and for the attractive vivacity of its style, as the best work on operative surgery with which we are acquainted in any language. The value of many portions can scarcely be overrated, containing, as they do, the opinions which long and varied experience had impressed upon an enthusiastic, dexterous, courageous, but thoughtful and cautious surgeon. Probably the correctness of many of these opinions could not be proved by numerical research in the note-books of the author, and it is to be regretted that he did not keep a more exact report of his practice; but so long as the impressions derived from experience be faithfully recorded, their value as an expression of the opinions of one whose opportunities for observation were as great as his industry, cannot be questioned. The mind of the departed surgeon is reflected in the pages of his book. The knowledge he had gained by years of labour is there spread before us. It is no slight matter, after a life of continual and active exertion, to leave such a monument as this to the judgment of posterity, such a talisman to affect the minds and actions of men long after one is turned to dust. Let not the lesson be lost. Let each of us do something for the science we profess, for the art we practise. Let us so act, that when the elements of our bodies are restored to their natural combinations, the creations of the mind may live immortal, and still continue their influence for good through all generations.

## ART. II.

1. *A Treatise on the Climate and Meteorology of Madeira.* By the late S. A. MASON, M.D., Inventor of Mason's Hygrometer. Edited by JAMES SHERIDAN KNOWLES. *To which are attached, a Review of the State of Agriculture and of the Tenure of Land, by G. PEACOCK, D.D. F.R.S. &c., Dean of Ely, and Lowndean Professor of Astronomy in the University of Cambridge; and an Historical and Descriptive Account of the Island, and Guide to Visitors, by JOHN DRIVER, Consul for Greece, Madeira.*—London, 1850. 8vo, pp. 388.
2. *The Medical Topography of Queenstown: being an Examination into the Climate, and the Influence it Exercises on Disease, especially Pulmonary. With some Notice of the Natural History of the Locality.* By DAVID H. SCOTT, M.D. M.R.C.S., Fellow of the Botanical Society of Edinburgh, Honorary Physician to the Fever Hospital, and Consulting Physician to the Dispensary, Queenstown.—Cork, 1849. 8vo, pp. 102.
3. *Hastings considered as a Resort for Invalids; with Tables, illustrative of its Temperature, Salubrity, and the General Character of the Climate, showing its Suitability in Pulmonary and other Diseases. Also, Directions for the choice of a Residence, and Hints as to Diet, Regimen, Bathing, &c.* By JAMES MACKNESS, M.D., Member of the College of Physicians, Consulting Physician to the Hastings Dispensary. Second Edition.—London, 1850. 8vo, pp. 187.

It appears, at first sight, a very simple matter, in any given case of disease, to select the climate most suitable to the patient's condition; but in truth it is not so. It is quite easy to lay down certain broad principles, founded partly upon theoretic grounds, partly on the results of experience; and by their aid to discriminate, after a very general fashion, the kind of climate best adapted to different classes of individuals. But when we seek to enter fully into the subject, penetrating somewhat below the mere surface, and endeavouring to ascertain, with tolerable accuracy, the special influences at work in different localities, with their bearing on the actions of the animal economy in health and in disease, we find our inquiries at once surrounded by numberless difficulties. These difficulties spring, chiefly, from two sources; first, from our very imperfect acquaintance with the peculiarities of the climate of most countries; and, secondly, from the exceeding complexity of the operations to which the body is subjected under the ever varying conditions in which it is placed. We shall endeavour to illustrate both of these, and thus, while briefly sketching what is already known, indicate the directions in which, as we conceive, further investigations should be made.

And here we may at once notice an obstacle which stands at the very threshold of our inquiries, rendering the first steps precarious and uncertain; we mean the impossibility of instituting perfectly satisfactory comparisons between the results obtained by different observers. A very slight variation in the position of the instruments, by changing or modifying the influences to which they are subjected, will produce marked differences in their indications. Setting aside the more palpable fallacies from warm exposure, direct sun-light, &c., which no careful observer will

of course overlook ;—shelter from currents of air and from rain, or exposure to one or both, a situation near the level of the sea, or at some elevation above it ; defence from radiated heat and reflected light, or the opposite ; these, and many other circumstances which are apt to escape notice, have a material effect in influencing general results, and will lead to more or less erroneous conclusions, when we attempt to compare the climate of various localities. For example, we have ourselves known two thermometers, placed within half a mile of each other, show, under particular circumstances, a difference of  $5^{\circ}$  or  $6^{\circ}$  at the same hour, for some days together. One of these instruments was in a sheltered situation, nearly at the sea-level, and defended from wind and rain ; the other exposed to one or both, according as the weather was fine or wet, and at an elevation of about 100 feet. Dr. Mackness gives a table (p. 14), which illustrates the same fact, and exhibits the difference in the temperature between two parts of Hastings, the mean at 8 a.m., for High Street, being, during the month of December, when the observations were made,  $2^{\circ}$  higher than that of Wellington Square.

Now, it is perfectly evident, that had one instrument only been employed in either of these two instances, the inferences derived from its indications would have expressed, not the climate of the locality generally, but merely that of the particular part in which it was situated. And this demonstrates the importance of multiplying observations, if we are to arrive at any thing like certainty. It shows also, how needful it is, that the conditions under which the observations were made should be accurately noted ; and in this there is commonly a great deficiency. Of the works, whose titles stand at the head of this article, Dr. Mason's is the only one in which we have full information on these points. Dr. Mackness tells us, that one set of observations were made in the High Street, the thermometer being placed about five feet from the ground, in a northern aspect, and perfectly protected from radiation. Another set were made "in the Croft, the thermometer being well placed, and the observations most carefully taken." A third at Rock House, on the West Hill, the thermometer being four feet from the ground, protected from rain and radiation. Here we are altogether left in ignorance of the height above the sea level, and of the shelter from winds. Dr. Scott is equally concise :

"The instruments employed were Syke's self-registering day and night thermometers, which were suspended in a northern aspect, and removed completely from all direct influence of the sun ; a similar instrument with a blackened ball, exposed to the direct influence of the solar beams for a period of one year ; Daniell's hygrometer, a Fahrenheit thermometer ; a pluviometer and barometer, at about 70 feet over the water's level."

Here no notice whatever is taken of the position occupied by the thermometers, in regard to radiated heat or reflected light, both most important : we are not told whether they were connected with the walls of any building or not : and we know nothing of their shelter or exposure to winds. Moreover, the immediately preceding sentence, in which it is stated, that the observations were taken at *two* situations, and therefore apply generally to the whole town, makes it quite uncertain as to the height above the level of the sea, at which the temperature was noted, seeing that the seventy feet mentioned above cannot refer to both.

Contrast with these meagre accounts the full details given by Dr. Mason,

which we quote at some length, because, in our opinion, they may well serve as a model for future investigators.

"The observations were commenced on the 1st of March 1834, and ended on the 28th of February, 1835. Sta Luxia Cottage is situated on the Mount Road, at a rather abrupt rise from Funchal, on the brow of a valley lying between two ravines. Its distance from the sea is about half a mile due north; and its height above the level of the sea 350 feet. The register thermometer, for the external temperature in the shade, was fixed in the garden, four inches from a stone pillar, one foot square, with a room above it,—the pillar being one of the supporters of the floor,—free from currents of air and reflected light, though exposed to radiant heat, and receiving the sun's rays obliquely from 2 p.m. to 5 p.m.; the thermometer obtaining its maximum from 2½ to 3 p.m.; aspect n.w. by compass. The register thermometer for the shade, in doors, was situated in a sitting-room to the east, on the s.w. wall, by compass; a closet, four feet square, intervening between the thermometer and external wall of the house. This thermometer was subject to reflected light from the oil-cloth on the floor and the opposite wall—white-washed—from 9 a.m. to 11½ a.m.; maximum temperature about 2 p.m. Both thermometers were surrounded by a fold of white writing paper. The intensity of the sun's rays was ascertained by placing a thermometer, of large range, having its bulb covered with black wool, on the mould in the garden, with a full exposure to the sun, from 9 a.m. to 5 p.m.; the thick wooden frame in which the thermometer was placed rested upon the ground, but the bulb, with the wool intervening, might be about an inch distant. This arrangement was by no means unobjectionable; but the irregularities to which it was liable were in a great measure balanced by the multitude of observations. The hygrometer was situated in a room to the west, between two windows constantly open from 6 a.m. to 6 p.m., and very free from currents of air, as they acted like folding doors. This room was, consequently, free from local humidity arising from the evaporation of water from the ground, &c. The winds were determined by a vane in the garden, placed upon a high flag-staff, and registered by the compass. The few observations on radiation were taken from the thermometer which registered the intensity of the sun's rays; and, in the same situation, a register spirit thermometer, the bulb covered with black wool, was placed on a stool six inches from the ground, in the same exposure, with the view of obtaining the maximum intensity of terrestrial radiation."

Dr. Mason then gives a full description of the instruments employed.

Dr. Mason (p. 177,) enumerates six points to which the attention of inquirers should be directed:—first, to temperature; secondly, to the dew point, or the distance from the point of saturation; thirdly, to the pressure of the atmosphere; fourthly, to the rapidity of evaporation; fifthly, to the range and intensity of solar radiation; and sixthly, to the different states of electric tension. On the first three of these points we are in possession of a considerable amount of information, imperfect, indeed, in many respects, and less available for practical purposes than it might be, for the reasons stated above; but still most valuable. Regarding the latter we have wonderfully little knowledge: of the last, indeed, so far as concerns its influence upon particular climates, it would be scarcely an exaggeration to say we know nothing. The works before us certainly afford no help; of Madeira we are told that, "according to Dr. Heineken's account, the gold-leaf electrometer is very slightly acted upon." (p. 52.) Dr. Scott laments that he can say nothing on the subject; and Dr. Mackness passes it by without notice. And yet it is a most important subject, and one, the investigation of which could not fail to bring out results alike interesting and instructive. But to this point we shall have occasion again to refer.

But even were our inquiries complete on the questions above enumerated, we should not be presented with a full and perfect picture of the climate of any place, until, to a knowledge of these, we had added an acquaintance with the topographical relations of the place of observation, the geological and mineralogical formations constituting the basis of its soil, the state of cultivation, the prevailing winds, the moral condition of the inhabitants, and the diseases most common in the locality, more especially as regards their type. This is a wide field, but it is full of treasures, and would well repay the labours of its husbandmen.

Proceeding now to that which we have allotted to ourselves as the main object of this article, viz., the influence of climate on the human body, in health and in disease, we shall follow, as closely as we may, the order above indicated in the subjects noted for inquiry; premising that, though we consider separately the various elements of the compound agency, it is essential that it be ever borne in mind that they never act alone.

First, then, *of the influence of temperature*. This may be demonstrated in several ways: by the exhibition of the physical peculiarities of the nations of different countries, with the diseases to which they are most prone, and the types of these; by the effects produced on strangers visiting the countries examined; or by the changes worked out in the animal economy by alterations of temperature, without removal from one climate to another. On each of these points we have a large mass of information from which our limits will only allow us to select a few of the most prominent features.

The action of *severe cold* is directly sedative. The tissues and vessels are contracted; the blood is driven from the cutaneous surfaces and accumulates in internal organs, probably thus producing, in part at least, the stupor and insensibility with which persons unaccustomed to it are apt to become affected. Its influence, heightened by long privation of solar light, is manifested in the stunted growth and physical and mental weakness of the inhabitants of arctic regions. Their diseases usually assume an asthenic form, and infectious disorders, when introduced among them, prove very destructive; on migrating to more southerly regions, they are very liable to attacks of a febrile and sub-inflammatory character, and bear depletion ill. More moderate cold, such as that which exists in the northern parts of the temperate zone, acts as a general stimulant to both body and mind, as is manifested in the robust forms and vigorous intellects of its inhabitants. In these climates, the functional activity of the lungs and the kidneys greatly exceeds that of the skin and the liver, and disorders of the thoracic organs are among the most common and most prominent of the disorders observed. Here an interesting question arises, which is thus stated by Dr. Mason:

“The quantity of oxygen in a given space must vary according to the density of the atmospheric air. The greater the density, the larger will be the proportion of oxygen. Thus a dry air at 32°, and under a pressure of 30 inches, would contain more oxygen, in a given space, than a similar portion at 62° under the same pressure and hygrometric condition. How will this hold with regard to the action of the air in the process of respiration? Will a larger portion of oxygen be brought in contact with the lungs, in proportion to the specific gravity of the atmospheric air? This is a complicated question, and requires some consideration before it can be satisfactorily solved. What influence has temperature alone, with regard to the

quantity of oxygen brought in contact with the lungs? Will more oxygen be afforded when a person inhales air at  $32^{\circ}$  than at  $62^{\circ}$ , under the same pressure and hygrometric condition? Does a person inhale the same volume of air at  $32^{\circ}$  as he does at  $62^{\circ}$ ? If, when in contact with the lungs, the air inspired be always nearly equal in temperature to that of the body, the difference of specific gravity, arising from temperature, will have no effect, as regards the quantity of oxygen in a given space; the temperature, and consequently the expansion, being always the same. If this be correct, a less portion of air ought to be inhaled at  $32^{\circ}$  than at  $62^{\circ}$ . If the *same* quantity be inhaled, what effect does the expansion produce with regard to the distension of the air-cells, as further distension would take place at every temperature lower than that of the body?"

It is not easy to answer these questions definitively; but it must be remembered that, under all circumstances, the quantity of air taken in at one inspiration is not enough to penetrate far into the tubes, its entrance into the air-cells being accomplished in accordance with the law of diffusion of gases; and the greater the difference between the air within and the air without the lungs, the more rapidly and more perfectly will this diffusion take place. Be this as it may, it is quite evident, that the *cooling* effect of air at  $32^{\circ}$  must greatly exceed that of air at  $62^{\circ}$ , seeing that each portion inhaled must abstract from the body heat enough to raise its temperature nearly to the same degree as that of the lungs. In reference to this, Dr. Mason again inquires:

"In the healthy condition of the body, are the respirations more frequent in winter than in summer, to compensate for the greater loss of caloric, by a greater supply of oxygen? or, does the hygrometric condition of the atmosphere make up for this deficiency?"

We are not aware of any direct observations that have been made to ascertain this point; but seeing that the action of cold is generally of a stimulating character, we are inclined to believe, that the respiratory, as well as the other movements of the body, are absolutely quickened. The influence of various hygrometric conditions of the atmosphere will fall to be noticed immediately. Dr. Mason continues:

"Where, from disease or other causes, there exists an inability to generate a sufficient supply of animal heat, the determining of this question must be of considerable importance; for, in such a case, either the body must be reduced below the natural standard of health, or the temperature of the air inspired may not be raised to its ordinary condition; either of which circumstances may exercise a considerable influence on the function of respiration, as chemical action is so dependent upon the influence of temperature. This would be one cause for sending a patient, possessing such an organization, to a warm climate, that his animal heat might be maintained at the natural standard of healthy action." (p. 118.)

Under the influence of a *high temperature*, the nervous and vascular systems are excited to increased activity; the functions of the skin and intestinal mucous membrane being especially called into play, while the lungs have less work to perform. In the dark races, the natives of inter-tropical countries, the peculiar organization of the skin exhibits a special provision to meet the requirements of the climate; its black colour and largely developed secreting structures serving to diminish the effects of the heat, and to constitute it a most efficient depurating organ. In the white races, who are not thus constituted, the liver takes on a compensating action, the secretion of bile being both increased in quantity and altered in quality. On emigrating from these hot latitudes to others in

which a low temperature prevails during a great part of the year, there is a great, and often fatal tendency, to the development of tubercular disease of the lungs, and to affections of the kidneys and bowels; and the same is found to hold true of the lower animals also.

The influence of the more moderate heat, which characterises the climate of the warmer parts of the temperate zone, is so essentially modified by other co-existent conditions, that it is needless to delay our progress by dwelling upon this part of the subject. We proceed, therefore, to our next point, viz., *the influence of different hygrometric conditions of the atmosphere*. And here, at the outset, we are met by a similar question to that which has just now occupied our attention: does the hygrometric state of the atmosphere alter in any way the quantity of oxygen brought into contact with the lungs during respiration? The expansion produced by vapour will, of course, diminish the quantity of oxygen existing in a given space, in proportion to the temperature, supposing the air to be fully saturated; and if not fully saturated, in proportion to the actual quantity of vapour present. Thus, dry air will contain more oxygen in a given space than air almost, or altogether, saturated with vapour. But will this produce any effect on the respiration, so far as the oxygen consumed is concerned? We think not, because so soon as it gains access to the lungs, the temperature of the air is raised to that of the body, and it becomes, at the same time, fully charged with moisture from the effects of pulmonary exhalation. Its own physical condition, therefore, is at once altered, and it acts in that altered state.

But if thus unimportant in this respect, the case is very different when we come to consider the effects produced by the varying degrees of dryness or moisture of the air on other functions of the body. A quantity of watery fluid is continually passing off from the lungs and skin by the three processes of exhalation, evaporation, and transudation. The entire amount, in health, is estimated by Seguin at 18 grains per minute, of which 11 grains pass off by the skin, and 7 by the lungs. These processes can be and are greatly modified by the state of the surrounding atmosphere; and it is of much importance to be fully acquainted with the results thus produced, because they have a direct and very essential bearing on the question of the choice of climate for different invalids. From the lungs the watery fluid always passes off in the form of vapour, the expired air carrying it away in solution. Much, therefore, so far as this process is concerned, must depend upon the hygrometric condition of the atmosphere; for the quantity exhaled from the lungs must bear a certain relation to the temperature of the air inspired, and the quantity of humidity held in solution, or to the distance from perfect saturation. From the skin the fluid escapes in two ways, either insensibly by evaporation, which is a purely physical process, or in the form of sweat, by the vital action of transudation; and these, too, are greatly affected and modified by the state of the surrounding air. Dr. Madden has shown, by experiment, that pulmonary exhalation may be entirely arrested, and even the opposite process of absorption of watery vapour by the lungs set up, under certain circumstances;\* and co-operation by the skin will, of course, be absolutely prevented by the action of air at the same temperature as the body, if fully charged with vapour. But transudation will still continue

\* *Priz: Essay on Cutaneous Absorption*, p. 56.

to be carried on, and that often to a very large amount. Dr. Mason relates an instance which serves as a good illustration of these facts :

"A man entered one of the *estufas* at a temperature of 160°, the air being saturated with water and spirituous vapour from the wine. Pulmonary exhalation was suppressed ; and, after remaining 3½ minutes, perspiration by transudation was excited to such an extent that sweat streamed from every part of his body. The respiration was increased in frequency, and he appeared so exhausted as to be scarcely able to stand. He continued panting for eight or ten minutes after he came in contact with the external atmosphere, which at the time was from 65° to 70°."

This is an extreme instance, and one which cannot occur excepting under conditions artificially produced ; but modified results of the same kind are often observed, and it is probable that part of the unhealthiness of low-lying damp countries, near the tropics, where the temperature is high and the air unduly moist, is owing to this cause. In warm weather, as we all know, free evaporation from the surface has a most cooling and refreshing effect, and hence there is less feeling of oppression when the wind is blowing than when the atmosphere is perfectly still ; but this evaporation cannot but be impeded in proportion to the moisture of the air ; and in some measure, as this retardation takes place, does the system, for the time, lose the benefit of the removal of that portion of fluid which is necessary for the maintenance of perfect health. The immediate result is a feeling of languor, oppression, and discomfort, continuing and increasing, until the skin takes on a compensating action, and the system is relieved by perspiration more or less profuse. Dr. Mason observes, that in Madeira, at the close of the evening, as the increased humidity comes on, the sweat may be seen standing on the foreheads of very many individuals ; and almost all complain of the heat, although it may be from 3° to 5° less than in the middle of the day ; when the air being farther removed from the point of saturation, neither of these conditions are observed, though the temperature is higher. Hence, in these climates, it is a good practical rule to close the windows and doors towards evening, before the damp air enters. "In my own house," says Dr. Mason, "where the windows and doors were regularly closed at a proper period, this oppression was never complained of, nor were the ladies obliged to fan themselves without intermission :"—a good illustration of the advantage of a little science in the affairs of every day life.

We have next to consider what is the effect of *cold moist air*. As far as pulmonary exhalation is concerned, it will act in much the same way as dry air at a much higher temperature, because with the increased heat which it acquires in the lungs, its capacity for moisture is increased, and consequently the process of exhalation is carried on vigorously. But then, in doing this, it makes, at the same time, considerable demands upon the body which the other does not, abstracting each moment such an amount of vital heat as will suffice to raise its own temperature to the required standard. On the skin, too, it must, to a certain extent, act in like manner. The stratum of air in immediate contact with the body will be warmed to a certain degree, and thus favour the cutaneous evaporation ; but its two qualities of coldness and dampness are both unfavorable to transudation, which is a vital action, and accordingly perspiration is checked and impeded, and if by any means it be produced, as during



vigorous exercise and so forth, there is great chance of its remaining on the surface, to chill and injure the health. Dr. Mason, in more than one place, draws a comparison between the action of the cold frosty air of England and the hot dry *testi* of Madeira; but in this he appears to us to have overlooked, somewhat singularly, the depressing influence of the cold on the nervous and circulating systems, and the constringing action on the skin, which, by driving the blood from the cutaneous capillaries, must diminish, *pro tanto*, the amount of evaporation. Indeed, so far as we can judge, both from physiological inferences and from observation of disease, a cold damp air appears to us the very worst to which man can be subjected; we do not see one good quality that it possesses, and would warn all invalids to flee it by all means.

The effect of *hot dry air* cannot be better illustrated than by reference to the account given by Dr. Mason of the influence of the wind just mentioned, the *testi*, as it is named in Madeira. It blows from the E.S.E., passing to the island directly from the coast of Africa. Not a cloud is to be seen during its continuance; the whole atmosphere is of one uniform unvaried blue, very light blue gray, of a peculiar character, as though viewed through what a painter would term a very thin warm aerial haze. It almost invariably lasts three days. Its extreme dryness, notwithstanding that in its course to the island it has passed over three hundred miles of ocean, is manifest from the fact that, while the mean dryness for the year in Madeira, from 9 a.m. to 9 p.m., amounts only to  $3^{\circ} 91'$ , Dr. Mason found it, during a *testi*, reach to  $22^{\circ} 5'$ . During its prevalence, the surface of the body is dry, perspiration appearing to be thoroughly arrested; the lips and nose feel as though they suffered from a recent cold. Delicate people, or persons in health lately arrived, find their breath hot and their lips parched; while their faces, and such other parts of the body as are exposed to the air, feel as though they were frost-bitten, and assimilate their sensation to what they have experienced in a northern climate, when exposed to a keen easterly wind on a frosty day. Pains in the head are generally complained of, as well as great thirst and general languor, with a feeling of faintness, loss of appetite, and inability to take their accustomed exercise, whether within or without doors: these results are accompanied with an unusually irritable state of the mind. The moment this wind approaches, the change which it effects on the condition of the atmosphere is perceived, and every one feels as if he were suddenly transported into another climate. Both natives and strangers drink plentifully of lemonade and other cooling drinks, which, however, produce only a momentary effect in allaying the painful sensation of thirst.

The prevalence of east and north-east winds in this country, during winter and spring, renders us sufficiently familiar with the effects of *dry air at a low temperature*. Its desiccating influence on the pulmonary and cutaneous textures is, of course, great; because, being already far removed from the point of saturation, it will acquire the power of dissolving much additional vapour, when its temperature is exalted by contact with the body. Hence the withered, shrunken, and harsh look of the exposed skin, which is almost universally observed; and hence, also, the feelings of rawness in the chest, and the irritable cough, to which the subjects of pulmonary disease are so liable under these circumstances.

This would seem the proper place to take some notice of the *fall of rain*, and *rainy days*, which are commonly enumerated among the elements of climate. We do not dwell upon them, however, for the following reasons:—the quantity of rain falling in any district is, in a very remarkable manner, subjected to influences of a purely local character. Thus, at Kinfauns, in Scotland, the amount shown on an average of five years was 25·66 inches; while in its vicinity, on a hill 600 feet above the level of the sea, the amount was 41·49 inches. And, generally, the vicinity of high lands greatly increases the quantity of watery precipitation. But, independently of this, which of course is only another indication of the necessity of attending to more than the mere air, it appears from some curious information respecting Indian meteorology, lately communicated to the Royal Society by Colonel Sykes, that very little faith is to be put in the indications of a single rain-gauge, however carefully placed, as representing the average fall in a place or district, since the variations in the amount shown by several rain-gauges within a circuit of a few miles, are much greater than can be accounted for by their mere difference of elevation, or other local circumstances. And, still further, it is well remarked by Dr. Mason, that the statements of observers present us with a very unfair picture of the climate of any place, more especially in countries subject to frequent occasional showers, from the fact, that no information is given of the actual *number of hours* during which rain has fallen; the whole day being set down as rainy, provided a strong shower fell during any part of it, though the greater portion of the time might have been fine, or merely overcast. Dr. Mason's tables, illustrative of this branch of the subject, are worthy of all praise. Table vi shows when any part of the day and night was fine, cloudy, or variable, and when any rain fell in each month, with the number of hours actually fine, cloudy, or rainy during the same periods; and also the number of days on which thunder occurred. In Table vii, the same results are arranged under the respective seasons.

The *rate of evaporation* is also closely connected with this branch of our subject. Depending as evaporation does for its very existence on caloric, the rapidity with which it takes place must be in a great measure regulated by the temperature of the air; but it is also modified by the state of the atmosphere as to the quantity of vapour already present in it, and by the variations of barometric pressure. A dry, warm air, with low barometric pressure, affords the conditions most favorable to its rapid and extensive production; accordingly, the minimum amount is found during winter, and the maximum in summer. Observations, therefore, on this point, are valuable as assistances in ascertaining the true state of the atmosphere; but they have another and more practical bearing. During the process of evaporation, a considerable quantity of caloric becomes latent, thus lowering the temperature of the surrounding air. It is in consequence of this that we often experience a smart frost, though of short continuance, immediately after a thaw. The icy caverns of the Jura, and the remarkable one mentioned by Sir R. I. Murchison, at Illetzkaya, in the steppes of the Kirghis, near to Orenburg, seem owing to the intense evaporation of moisture, caused by the warm and dry external air. And, but for an admirable provision in nature, the depression of temperature in the neighbourhood of large bodies of water would be

very great indeed; for, in the evaporation of a given bulk of water, every 100th part converted into vapour would lower the temperature  $10^{\circ}$  Fahr., did not the surrounding air supply caloric to modify the result.\* And so, also, with the living body, the evaporation of cutaneous perspiration has great influence in diminishing, or even counteracting, the injurious effects of great heat. Where, therefore, rapid evaporation is found to be the law of a district, there we may expect these results to follow.

The next point to which we have to direct our attention is the influence of *atmospheric pressure*. And, first, as to the quantity of oxygen presented to the lungs at each inhalation. It is admitted that, in a given space, there is a greater quantity of oxygen at a pressure of 31, than at one of 28 inches, other circumstances being equal; and as there is no condition of the *body* which can have a counteracting influence upon this, as it had in the former examples of temperature and hygrometric state, the lungs must have a proportionally larger amount brought to bear upon their lining membrane. "This will account," says Dr. Mason, "for the languor and lassitude experienced by many individuals in humid weather, when the barometer indicates a low degree of atmospheric pressure; the quantity of oxygen inhaled being less than when the atmosphere is dry, and the pressure of the incumbent mass considerable." We confess that we are inclined to regard this as somewhat problematical, at least as to any extent of such influence; believing that the sensations experienced result rather from the physical conditions of pressure affecting the body directly, and not through the interference of more rapid or slower chemical changes. Thus, to us it appears exceedingly probable, that the sense of fatigue in the limbs, which is felt by persons walking in elevated regions, is partly due to that cause which was first suspected by Humboldt, viz., that the mechanism of the joints, and the equipoise of the limbs, are disturbed by the low atmospheric pressure; a supposition which receives a considerable measure of support from the experiments of the two Webers upon the hip-point. These observers found that, after the two bones had been detached, by cutting the capsular ligament through, the pressure of the air will still retain the head of the thigh-bone firmly in the socket, from which it sinks down when the air is artificially rarified underneath, the joint thus becoming a sort of air-pump, in which the head of the femur acts as a piston.†

Be this as it may, it is abundantly evident, that man can live, and enjoy good health, in places very variously situated as to the average amount of atmospheric pressure; nor are we as yet in possession of any information which would lead us to suppose that a continued low pressure, such as that which must exist in the city of Mexico, which is 7460 feet above the level of the sea, or at the town of Potosi, which is at an elevation of 13,260 feet, has any peculiar influence either upon the nature or course of the diseases, or the length of life of the inhabitants. The case is somewhat different as regards sudden and great variations in the atmospheric pressure, which it is scarcely possible to suppose can be altogether devoid of effect upon the circulating and respiratory organs. Dr. Holland, in the very suggestive chapter to which reference has just been made, observes, that at two or three periods he had noted a more than usual fre-

\* See Thomson's Introduction to Meteorology, p. 103.

† See Holland's Medical Notes and Reflections, p. 483.

quency of apoplectic or paralytic seizures, and that at the same time there were frequent and rapid changes in the barometer, often with great depression of its level; and he seems inclined to regard the connexion of the two as something more than casual. He noted also, at the same periods, "the very common occurrence of lesser affections of the head,—vague and uneasy sensations, oppression, vertigo, and what may be termed a feeling of want of proper balance to the frame,—all indicating some cause present which tends more or less to disturb the equality of circulation through this organ." And yet the experience of Mr. Green, the celebrated aéronaut, as communicated to the same author, is conclusive against the necessary production of any serious, or even any marked effects under rapid and very great diminutions of atmospheric pressure :

"Mr. Green informs me, that he has found none of these individuals [more than 400 who had ascended with him at various times] sensibly affected, otherwise than by the sudden change of temperature, and by a noise in the ears, compared by some to very distant thunder; the latter sensation occurring only during rapid ascent or descent of the balloon, and, when greatest in degree, far less distressing than that produced by descent in a diving-bell. He has never felt his own respiration hurried or oppressed, except when exerting himself in throwing out ballast, or other management of the balloon, or when suddenly passing into a very cold atmosphere. His pulse is occasionally quickened ten or fifteen beats, but this only when some such exertion has been sustained. He mentions to me expressly, that in no instance have his companions experienced vertigo or sickness, thus rendering doubtful one of the statements current on this subject, and showing how little the two great functions of circulation and respiration are disturbed under circumstances where much effect might have been anticipated. Though the inference is limited to two persons, yet it may be worth while to mention the great experiment made by Mr. Green and Mr. Rush, in September 1838, in ascending to the height of 27,136 feet, or 5 $\frac{1}{4}$  miles above the level of the sea, the greatest elevation ever reached by man, and very exactly corresponding with the highest ascertained summit of the Himalaya mountains. The barometer fell from 30° 50' to 11° during this ascent; the thermometer from 61° to 5°. The first 11,000 feet were passed through in about seven minutes; yet, under these remarkable circumstances, Mr. Rush suffered no inconvenience but from cold, and Mr. Green little other than from the toil of discharging ballast and gas at different intervals, which hurried the respiration during the time." (p. 484.)

From all this we may, it appears, pretty safely conclude, that in considering the influence of climate, barometric pressure, excepting in so far as it is connected with other conditions, may be regarded as a subject of but secondary importance.

Regarding the *range and intensity of solar radiation*, we do not, as yet, possess much available information. Dr. Mason's observations are interesting, chiefly because they are at variance with the inferences of Professor Daniell. That very accurate observer concluded, from his experiments, that the force of the sun's direct radiation decreases as we approach the equator, and increases as we advance to the poles. In another place he observes, it is not the absolute heat of the sun which increases from the equator to the poles, but the difference between the direct rays and the shade; and in support of this he adduces the result of observations made at Bahia, on the coast of Brazil, where the maximum effect in July was only 47°, while in June, in this climate, it was 65°. He also refers to some of Dr. Scoresby's experiments in the Arctic regions, where the thermometer, in the direct rays of the sun, stood at 120°,

"of historical value, as one of the greatest surgical errors," close this division of the volume.

In the last division of the work, consisting of forty pages, the subject of Amputation is treated in general, and the various amputations and exarticulations of the upper and lower extremities are described. Our space does not permit us to give any analysis of this division, but we may show that Dieffenbach, as a general rule, prefers circular to flap amputations; that he brings the edges of the wound together as soon as bleeding is arrested, rather than waiting for some hours, as recommended by Liston; and that in selecting the period for amputation after accident, he would choose the earliest possible moment after recovery from the shock the whole system has experienced, and before reaction has occurred. When once febrile reaction has supervened, he would wait until free suppuration was established.

Our task is now concluded, and we do not lay down the pen without a certain feeling of melancholy when we reflect, that the hand which has been employed for our instruction will be so no more,—that the spirit which directed the hand in the hour of thought and the moment of action, has returned to the "God who gave it." Under the influence of this feeling we have made a simple rather than a critical analysis of a book which may be regarded as the last testament of a great man, and have left his opinions to be judged by our readers. The work is far from being perfect or faultless. Its various parts are of very unequal value. There is, above all things, a great want of exact numerical information as to the result of various operations performed by the author; but, taking it as a whole, considering, especially, the great value of the long section in the first volume on plastic surgery, we do not hesitate to characterise it, for minute practical information, and for the attractive vivacity of its style, as the best work on operative surgery with which we are acquainted in any language. The value of many portions can scarcely be overrated, containing, as they do, the opinions which long and varied experience had impressed upon an enthusiastic, dexterous, courageous, but thoughtful and cautious surgeon. Probably the correctness of many of these opinions could not be proved by numerical research in the note-books of the author, and it is to be regretted that he did not keep a more exact report of his practice; but so long as the impressions derived from experience be faithfully recorded, their value as an expression of the opinions of one whose opportunities for observation were as great as his industry, cannot be questioned. The mind of the departed surgeon is reflected in the pages of his book. The knowledge he had gained by years of labour is there spread before us. It is no slight matter, after a life of continual and active exertion, to leave such a monument as this to the judgment of posterity, such a talisman to affect the minds and actions of men long after one is turned to dust. Let not the lesson be lost. Let each of us do something for the science we profess, for the art we practise. Let us so act, that when the elements of our bodies are restored to their natural combinations, the creations of the mind may live immortal, and still continue their influence for good through all generations.

## ART. II.

1. *A Treatise on the Climate and Meteorology of Madeira.* By the late S. A. MASON, M.D., Inventor of Mason's Hygrometer. Edited by JAMES SHERIDAN KNOWLES. *To which are attached, a Review of the State of Agriculture and of the Tenure of Land, by G. PEACOCK, D.D. F.R.S. &c., Dean of Ely, and Lowndean Professor of Astronomy in the University of Cambridge; and an Historical and Descriptive Account of the Island, and Guide to Visitors, by JOHN DRIVER, Consul for Greece, Madeira.*—London, 1850. 8vo, pp. 388.
2. *The Medical Topography of Queenstown: being an Examination into the Climate, and the Influence it Exercises on Disease, especially Pulmonary. With some Notice of the Natural History of the Locality.* By DAVID H. SCOTT, M.D. M.R.C.S., Fellow of the Botanical Society of Edinburgh, Honorary Physician to the Fever Hospital, and Consulting Physician to the Dispensary, Queenstown.—Cork, 1849. 8vo, pp. 102.
3. *Hastings considered as a Resort for Invalids; with Tables, illustrative of its Temperature, Salubrity, and the General Character of the Climate, showing its Suitability in Pulmonary and other Diseases. Also, Directions for the choice of a Residence, and Hints as to Diet, Regimen, Bathing, &c.* By JAMES MACKNESS, M.D., Member of the College of Physicians, Consulting Physician to the Hastings Dispensary. Second Edition.—London, 1850. 8vo, pp. 187.

It appears, at first sight, a very simple matter, in any given case of disease, to select the climate most suitable to the patient's condition; but in truth it is not so. It is quite easy to lay down certain broad principles, founded partly upon theoretic grounds, partly on the results of experience; and by their aid to discriminate, after a very general fashion, the kind of climate best adapted to different classes of individuals. But when we seek to enter fully into the subject, penetrating somewhat below the mere surface, and endeavouring to ascertain, with tolerable accuracy, the special influences at work in different localities, with their bearing on the actions of the animal economy in health and in disease, we find our inquiries at once surrounded by numberless difficulties. These difficulties spring, chiefly, from two sources; first, from our very imperfect acquaintance with the peculiarities of the climate of most countries; and, secondly, from the exceeding complexity of the operations to which the body is subjected under the ever varying conditions in which it is placed. We shall endeavour to illustrate both of these, and thus, while briefly sketching what is already known, indicate the directions in which, as we conceive, further investigations should be made.

And here we may at once notice an obstacle which stands at the very threshold of our inquiries, rendering the first steps precarious and uncertain; we mean the impossibility of instituting perfectly satisfactory comparisons between the results obtained by different observers. A very slight variation in the position of the instruments, by changing or modifying the influences to which they are subjected, will produce marked differences in their indications. Setting aside the more palpable fallacies from warm exposure, direct sun-light, &c., which no careful observer will

filament of gold leaf. The wind was s.w., accompanied with a steady driving rain and fog during the whole of a November day; and I found it impossible, by the most delicate condenser, to discover the least symptom of electrical action in the wire. I accordingly gave up experimenting, and commenced reading in the same room; when, after some hours, (the two balls of the apparatus, namely, the giving and receiving one, being at one inch distance from each other,) I heard a smart explosion, and going to the conductor, was highly gratified at a succession of powerful explosions taking place between the two balls, increasing in rapidity until they became one continuous stream of fire, which again diminished in power, and immediately afterwards recommenced with the opposite electricity. These successive changes lasted throughout an interval of *five* hours, during which time the stream given out exceeded in power any I ever witnessed, excepting during a thunder-storm. I think the largest animal would have been struck dead if brought into contact with it. This effect was the more surprising, as no apparent change had taken place since the morning; the same wind, driving fog and rain, the barometer, thermometer, hygrometer, all the same." (Thomson, p. 116.)

During the extraordinary *dry-fog* of 1783, which continued from the 29th of May until nearly the end of July, the atmosphere was highly electric, and, what it is interesting to know, even though it may be but a mere coincidence, influenza became epidemic at the same time, 40,000 persons being immediately attacked by it at St. Petersburg, after the thermometer had risen 30°.

Dr. Holland observed striking electrical phenomena to accompany the sirocco in the Mediterranean; and we are greatly inclined to believe that the discomfort, almost universally felt during the prevalence of the east wind, has really an electric origin. We have already seen, that the desiccating influence of this wind is great, and we know that the conversion of fluid into vapour is accompanied by electrical changes; if we also knew, from sufficiently extended data, the general electric condition of the atmosphere when such wind prevails, we might arrive at some definite conclusions, and perhaps be led to means that would prove counteractive.

That this is not an improbable supposition, is manifest from facts already well established:

"An atmosphere," says Dr. Holland,\* "proved by other phenomena to be highly charged with electricity, produces in many persons sensations resembling those of slight incipient fever; vague alternations of chill and warmth on the skin, general languor of the frame, debility and aching of the limbs, oppression or other uneasiness about the head. In other instances, the feelings created in the muscles of the trunk and limbs have more of rheumatic character; the resemblance being such as to justify a suspicion, that some of the muscular affections, often so termed, are actually derived from this cause. In some persons, the susceptibility is so great, that even the approach of a thundercloud produces bodily feelings akin to those just described, together with a sense of fulness and pricking about the eyes, and a slight tingling over the whole body, which I have often noticed in such cases.

"One of the best tests of the actual operation of atmospheric electricity on the body is, as I think, that mixed sensation of heat and cold which most persons must recollect at some time to have felt,—or rather, the consciousness of sensations which cannot clearly be defined to be either. Concurrently with such state of atmosphere, which the thermometer does not in any way interpret to us, there generally occurs more or less of the lassitude before described; the muscles are readily fatigued; some degree of headache is often felt; and other vague uneasiness of the bodily feelings, varying much in different habits, and doubtless influenced by the condition of health at the time. Though these effects are in

\* Medical Notes, p. 487.

general more distinctly experienced previously to, or during, thunder-storms, yet are they also sometimes attested in other states of weather where no such storms occur. Certain winds, very common in our own climate, will sustain, even for weeks together, this peculiar character of atmosphere; in degree sufficient to be marked by the results just described, and having still more singular and obvious influence on other animals inferior to man, and on vegetable life. These winds, which may be described generally, as coming from all eastern points of the compass, but more especially from the quarter lying between north-east and south-east, deserve inquiry under all the aids which modern science can afford. Their various effects on the human body, and on all living organization, are in no wise explained by the temperature or weight of the air. The great dryness of some easterly winds may give better reason for certain of the phenomena, but will scarcely explain the peculiar sense of muscular aching, uneasiness, and langour, they produce in many habits; the almost instant perception of their effects by some, even without any exposure to the external air; and as rapid consciousness of change when they cease. Such sensations belong much more to what we know of electrical agency, than to any other cause we can assign; but they need observations more exact than have yet been made, and a careful comparison of these with the physical properties of the winds in question, which future research may also better determine."

It appears to us, that we have here indicated one of the greatest desiderata in furtherance of a correct and fruit-bearing knowledge of the influence of climate. If, in addition to the ordinary tables of temperature and hygrometricity, we had also accurate registers of the winds, and the varying electrical conditions of the air, these, combined with carefully kept records of the prevalent diseases, and the peculiarities of their symptomatic manifestations, could not fail to afford materials for important deductions.

We have thus, after a somewhat glancing and superficial fashion, noticed the six chief points originally laid down as claiming our attention in regard to the nature and operation of different climates; the others, which were at the same time notified, are to be ranked rather among modifying agents, than among the first elements, seeing that their operation is chiefly evinced by the changes which they induce in the actions or the conditions of the preceding. And here we are again met by that fundamental truth with which we prefaced our inquiries,—viz., that the different agencies do not, and cannot act alone. For the purpose of investigation we separate and analyse the compound—climate; in practice we meet and have to deal with it as a whole. For example, we cannot by sending a patient to the south, secure the simple action of increased temperature alone; with that he must also be subjected to changed hygrometric and barometric conditions, to increased solar radiation, and probably, also, to different intensities of electric tension. So that in choosing a locality, it is needful to be acquainted with those modifying agencies, as well as with the other primary conditions. A glance at the map of isothermal lines, will show how essentially the annual temperature of a place is affected, by circumstances of locality, &c. And these are so well and graphically arranged, in a single picture, by the illustrious discoverer of that system, that we cannot do better than quote his own words, which will bring them at once before our readers:

"In enumerating the causes which exercise a disturbing influence on the form of the isothermal lines, I have distinguished between those which *raise* and those which *depress* the temperature. To the first belong,—the vicinity of a western



coast in the temperate zone; a divided or intersected configuration of the land, with projecting peninsular, and deep re-entering bays and inland seas; aspect, or the position of the land relatively to a sea, free from ice, extending within the polar circle, or to a considerable mass of continental land situated beneath the equator, or at least within part of the torrid zone; prevalence of southerly and westerly winds on the western side of a continent, in the temperate zone of the northern hemisphere; chains of mountains acting as screens or protecting walls against winds from colder regions; infrequency of swamps or marshes, which retain the ice in spring and early summer, absence of wood on dry sandy soil; constant serenity of sky during the summer months; and lastly, the vicinity of an oceanic current, bringing water of a higher temperature than that of the surrounding sea.

"Among the *cooling causes* which modify the mean annual temperature, I consider elevation above the sea level, especially when not forming an extensive table land; the vicinity of an eastern coast in the higher and middle latitudes; the compact and massive form of a continent, having a coast line little varied by indentations; an extension of the land in the direction of the pole, far into the frozen regions (there being no intervening sea free from ice during the winter); a geographical position in which the tropical portions of the same meridians are occupied by sea, implying the absence under those meridians of extensive tropical land, powerfully heated by the sun's rays, and giving out great heat by radiation; chains of mountains which, by their direction and precipitous form, impede the access of warm winds; the neighbourhood of isolated peaks, causing the descent of currents of cold air on their declivities; extensive forests, preventing the heating of the ground by the direct effect of the sun's rays, and, by means of the vital organic action of their leafy appendages, causing great evaporation, while, by the extension of these organs, they increase the quantity of surface cooled by radiation, thus operating in a threefold manner, by shade, evaporation, and radiation; extensive marshes which, in the north, form a kind of subterranean glacier in the plains, lasting until the middle of summer; a cloudy summer sky, or frequent mists, which impede the action of the sun's rays; and lastly, a very serene and clear winter sky, favouring the escape of heat by radiation."

Regarding the *eudiometric* condition of the atmosphere, we have, as yet, said nothing. For all practical purposes, the relative proportions of its constituent gases may be regarded as uniform, the slight variations in the quantity of carbonic acid not being sufficient to produce any appreciable effects. According to Liebig, ammonia is always present. It may undergo oxidation, probably from electric influence, and thus produce the nitrous and nitric acids which are sometimes found in rain; for that such a result can be effected, is proved by the experiment of Cavendish, who, by passing electricity through atmospheric air in glass globes hermetically sealed, obtained the red fumes of nitrous acid. The late lamented Dr. Prout has suggested the probability of the occasional presence of seleniuretted hydrogen, as a product of volcanic eruptions, and imagined it might be the originating cause of some epidemics. The peculiar substance *ozone*, recently recognised by Schönheim, has also acquired much importance from its supposed connexion with similar occurrences. Both these points demand further investigation.

Besides these, the atmosphere is the vehicle of all manner of odours and exhalations, of the deadly nature of many of which we had tremendous proof in the last epidemic of cholera. Much of the salubrity of sea air, we believe, is dependent upon its freedom from these contaminations; for though it has been supposed that its inherent chemical qualities are somewhat different from those of the atmosphere over land, the fact requires

\* Humboldt's *Cosmos*, by Sabine, vol. 1, p. 315.

proof. We are indebted to Dr. Mackness for the following account of M. Roubandi's experiments : \*

"M. Roubandi made several experiments in order to determine whether the atmosphere of the coasts contained either free or combined muriatic acid, and whether either of them existed in the atmosphere at some distance from the coast. In order to determine the first question, he suspended, some feet in the air, and at a few paces distant from the sea, during calm weather, a large glass balloon, filled with a freezing mixture of snow and sulphuric acid. The atmospheric vapour which condensed on the outer side of the balloon, produced a colourless, inodorous liquid, which suffered no change by keeping six months. Neither nitrate of silver, protonitrate of mercury, chloride of barium, or oxalate of ammonia, produced any change in it. It appeared perfectly similar to distilled water. Lime and baryta water were the only re-agents with which this liquid became slightly clouded; and after standing some hours, they occasioned a slight deposit, soluble in nitric acid. The same apparatus, placed at the same distance from the sea when it was rough, condensed a liquid which produced, with the following re-agents, the annexed effects: 1. With nitrate of silver: an opalescent tint, which, on standing some hours, formed a light precipitate, possessing the characters of chloride of silver. 2. With nitrate of suboxide of mercury: white flocks, which precipitated to the bottom of the vessel. 3. With lime and baryta water: turbidness, and eventually a precipitate, soluble in nitric acid. 4. With litmus paper: no change of tint. 5. Chloride of barium, ammonia, acetate of lead, and oxalate of ammonia, produced no appreciable effect.

"During a calm season, but when the sea was rough, the fluid obtained by means of the same balloon, at the distance of about fifty feet, gave no precipitates with the forementioned re-agents; but, when the wind blew from the sea towards the balloon, the liquid gave more or less of precipitates with them.

"The same experiments were repeated on the sea during a calm period; the balloon was suspended four feet from the surface, in a vessel at one hundred paces from the shore; the condensed liquid, evaporated to one third of its bulk, produced no effect upon the above-mentioned re-agents. M. Roubandi then, with some variation in the mode of making the experiment, attempted to determine the extent to which the saline particles of the sea are carried from the shore; and, from various experiments, he concluded—1st, that the air on the sea-shore, and over the sea, contains neither muriatic acid nor muriates; 2d, that, when the sea is rough, and especially when the wind is violent, particles of sea-water, in a state of great tenuity, float in the air, especially on the shore where the waves break, and that these particles are carried to greater or less distances according to the violence of the wind, and the degree to which the sea is agitated; 3d, that, without attempting to determine the distance with great precision, it may be admitted that, at Nice, where the south wind is seldom violent, the saline particles are rarely carried more than one hundred paces inland."

How, then, are these gatherings of knowledge to be applied in practice? In other words, how are we to proceed in choosing for our patients the climate most suited to their wants? We reply,—by considering, through the aid of physiology and pathology, what those wants really are. Will it be of service to encourage, or even stimulate, the exhaling processes of the skin and lungs? Then a warm and dry, or, under particular circumstances, a cold, dry, bracing air is the one most suitable. Is the liver inclined to strike work, or the skin to take on diseased action? Then, assuredly, it will be unwise to send our patients to those hot countries, where the one or the other, or both, will be called upon for extra activity. Do the delicate lungs need soothing, and the removal of all causes which would irritate? Then an equable, mild, and moderately

\* Climate of Hastings, p. 29.

moist air affords the conditions most favorable to recovery. And so, in each case brought before us, by reflecting on the requirements of the diseased frame on the one hand, and comparing them with the known climatic conditions of any given place on the other, we shall arrive at something like certain and definite conclusions,—the more certain and the more definite, in proportion as our knowledge both of climate and disease advances towards perfection. On one point we may already speak with some measure of confidence; and that is, the proved importance, in a hygienic point of view, of *equability* of temperature and other conditions. To persons in health, rapid and great changes may be innocuous, or even act as salutary stimulants; to the diseased they are uniformly injurious. And, therefore, *cæteris paribus*, those climates which are the most equable are the best fitted for the homes of invalids; for it is ever to be borne in mind, that one very essential object of change of climate is to secure to the sufferer a greater freedom of out-door exercise,—to place him where he can have the free air of heaven about him, pure, soft, and health-bearing, instead of the many-times-respired atmosphere of the sick room.

And here,—though it be somewhat foreign to the general scope of this article, which has dealt with man rather as an animal than as a spiritual being, concerning itself with the body rather than with that which informs and elevates the material structure,—we would for a moment refer to the mental and moral influence of the open air, an influence which we believe to be closely connected with our innate longing after the Infinite. Shut up within doors, and hemmed in by the four walls of his room, with one or two outlooks across the street, or up to the smoky gray which passes for blue sky, the sick man is surrounded by images which speak of limitation and of end. The room may be neat, or elegant, or luxurious, fitted with all that can minister to comfort or gratify the commoner senses; but it is, after all, but a small bit of space, cut out from the immensity, soon known, soon appreciated, soon wearied of. You reach the end of it in a few strides; you learn its lesson with briefest study. Now, change the scene, and let him walk abroad; how different the sensations! The breath of wind that touched his brow came to him from an unknown distance, and is gone again he knows not where, nor how far. The waves that ripple at his feet, gleaming and sparkling in the bright sunlight, are connected, drop by drop, with waters that bathe the homes of other men, and reflect from their bosom the unknown shapes and colours of far distant lands. A thousand, ever varying, forms of animal and vegetable life are around him; and above there is that glorious creation—"the sky which is for all," and which, as is so eloquently said by one of our best word-painters, "though so bright, 'is not too bright, nor good, for human nature's daily food,' but is fitted in all its functions for the perpetual comfort and exalting of the heart, for the soothing it and purifying it from its dross and dust. Sometimes gentle, sometimes capricious, sometimes awful, never the same for two moments together; almost human in its passions, almost spiritual in its tenderness, almost divine in its infinity, its appeal to what is immortal in us is as distinct as its ministry of chastisement or blessing to what is mortal is essential."\* There is nothing contracted, nothing narrow here. We look on and on, but the depth is still before us unfathomed and unfathomable; and the spirit feels that it

\* *Modern Painters*, vol. 1, p. 202.

is in the presence of that which, like itself, is the workmanship of the INFINITE.

It now merely remains for us to say a few words regarding the respective merits of the works whose titles stand at the head of this article. Our sense of the value of Dr. Mason's performance will have been already gathered, from the frequent references we have made to it. We may, indeed, safely characterise it as a model of care, diligence, precision, and clear-sighted laborious investigation. It has, moreover, a certain melancholy interest of its own, for it comes before us as the legacy of one who may almost be said to have died in gathering up its treasures.

"Contending with an extensive derangement of the pulmonary functions"—we quote from the Editor's preface—"he resolutely cast aside all solicitude for his own health, and, without intermission or pause, completed a series of difficult and fatiguing observations, with the noble view of rendering a benefit to society. The exposure and privations which he would have imperatively prohibited a patient from encountering, he fearlessly and enthusiastically contended with in his own person, undeterred by the most trying fluctuations of temperature, the prostration attendant upon a constant strain of the mind, and the watching which broke in upon that ordinary rest which even the robust cannot forego without some degree of suffering. To none would he for a moment depute the task which he had undertaken; and, when all around him were enjoying repose or courting it, this martyr, as he may be called, to meteorological investigation, passed the night with his instruments and journal, noting down the minutest change which the atmosphere underwent, from the first sinking of the sun to the first indication of its rising."

There are few such examples as this, nor can we wish there should be, for life is a precious gift, not to be thrown lightly away; but while we blame the rashness, we may still honour the devoted zeal of the weak-bodied but strong-hearted son of science.

The chief feature in the climate of Madeira, which is brought out by Dr. Mason, is its humidity; during the greater part of the time over which his observations extend, the air was almost saturated. Mr. M'Euen's observations, however, which were made at a subsequent period, viz., from December 10, 1848, to June 1, 1849, show a greater degree of dryness. (p. 203.) Hence, we may conclude, that there is considerable variety in this respect, and gain an additional argument for the necessity of frequent and long-continued investigations. We learn, also, from our author, that another very common opinion regarding Madeira is erroneous, viz., the absence of dew at Funchal. Mr. Wilde, in his very interesting narrative, remarks: "So slight are the dews falling in the town, that clothes are frequently hung out to dry during the night!" (p. 81.) Dr. James Macaulay observes: "The brilliancy of the heavens, the serenity of the air, the genial mildness of the atmosphere, render the nights, especially 'when the moon, with more pleasing light, shadowy sets off the face of things,' more inviting even than the day to be abroad in. The absence of chilliness and damp here permits one with safety to enjoy this." (Wilde, p. 82.) Dr. Heineken states that, "at the level of the city no perceptible dew is produced; but up the mountains it is profuse." These are very tempting assertions, which might lead to much mischief, if followed to their practical conclusions by invalids; for hear our author:

"Nothing can be more erroneous than this statement; as, when the nights are at all clear, the quantity of precipitation is immense; so that, by exposing a common-sized dinner plate, in a clear evening, several drachms of fluid may, at any

time, be collected in only a few hours; while the shrubs and ground-plants are quite wet with moisture, as from a strong shower of rain, and frequently remain in that condition till after eight o'clock the following morning. My friend Mr. Blewitt has also confirmed this fact by experiments upon the higher ground; and the same results take place at Sta. Luxia. Indeed, I could scarcely say whether the quantity of precipitated moisture, collected at the latter place, was greater or less than what I observed during my residence at Mrs. Mair's lodgings, near the level of the sea. Add to this Dr. Heineken's remarks a little above, 'that to make an observation after sunset *was, at least nine months in twelve, INCOMPATIBLE WITH HEALTH.*' Why? Not from a great depression of temperature; for, by the tables, it will be seen that the range of the thermometer is inconsiderable, and that the temperature does not decrease very much till an hour or two before sunrise. There is no other cause, then, that I know of, which would be at all incompatible with health, except the great humidity and precipitation of vapour." (p. 85.)

On the other hand, the equable warmth and other good qualities usually attributed to the climate of this island, are fully borne out by Dr. Mason's tables, and it still stands eminent as a most eligible home for those consumptive patients whose cases are likely to derive benefit from living in a *warm moist air*. When a *dry air* is required, no place can be less fit than Madeira; the unfortunate subjects will only be comfortable during the prevalence of the Teste, that is, for about twelve days or so during the year. It is well to bear this in mind.

Dr. Scott's little work on the climate of Queenstown is a most creditable production. The tables are particularly valuable, being evidently constructed with great care, and embracing full details regarding temperature, hygrometric condition, and barometric pressure. We learn from them that mildness and equability are the distinguishing features of this place, which is also well sheltered and protected from cold winds. In his comparative tables, however, Dr. Scott is less to be relied on, because he has not availed himself of the most recent information. In his preface, indeed, he refers to the works of Drs. Martin and Madden on the respective climates of Undercliff and Torquay, and states that they would have been available had they come under notice earlier; but that preface is dated October, 1849, while both the works in question were published in the early months of the same year, and reviewed by us in our July number. He has thus been led into statements which are calculated to mislead: *e. g.*, in his table of the mean temperature of the seasons, the mean winter temperature of the Undercliff is given as 42·5, and of Torquay as 39·3, that of Queenstown being 44·6; whereas, the correct statement would have been—Undercliff 41·8, Torquay 44·0. Again, the difference of temperature between winter and spring is thus given: Queenstown 5·8, Undercliff 6·0, Torquay 11·5; the last two should have been 7·8 and 6·0. Such inaccuracies are to be lamented, because by a little additional trouble they might have been avoided.

We regret that we cannot speak as favorably as we could have wished of Dr. Mackness's 'Climate of Hastings.' The meteorological tables are seriously defective, from containing no hygrometric observations whatever; and the sketches of consumption and other diseases are meagre in the extreme. We ought to have had a better book from the author of 'Dysphonia Clericorum.'

## ART. III.

*The Transactions of the American Medical Association.* Vols. I and II.—*Philadelphia*, 1848-9. 8vo, pp. 400, 956.

THESE 'Transactions' are the chronicles of the proceedings of the first two Annual Meetings of the American Medical Association, forming two goodly volumes, the contents of which have little resemblance to those bearing a similar title in this country. It is true, the advancement of medical science is the ultimate object of publication in both cases; but the efforts of our transatlantic brethren to this end are as yet but in a comparatively early stage of development. In a country in which medical education, practice, and literature are in so inferior a condition as they at present hold in the United States, the earliest efforts of the lovers of science must necessarily be employed in examining the causes of, and devising the remedies for, so lamentable a state of things. This work the Association has undertaken with courage and ability; and a large portion of the present volumes is taken up with an account of the results of their inquiries, and a detail of their suggestions. Already the National Medical Convention, which organized the present Medical Association, had published a code of Medical Ethics, which met with much approbation both in America and Europe, and formed a fitting preliminary to more extended operations. The Association is composed of delegates of medical societies, colleges, hospitals, lunatic asylums, and other institutions from all parts of the union, and holds its meetings at different cities in succession, after the manner of the British Association for the advancement of science. The two meetings which have taken place at Baltimore and Boston, were attended by several hundreds, comprising a great number of professional names of eminence. The two volumes now before us, containing an account of their proceedings, are chiefly filled with reports given in by the respective committees upon the present condition and future prospects of the profession, and upon the progress of medical science. Some of these are well deserving of notice.

*Reports of the Committees on Medical Education.*—This is properly deemed, by a body of enlightened men, having the elevation of the *status* and increase of the utility of their profession at heart, as the most vitally important subject that can engage attention. We were not prepared for some of the disclosures here made, and certainly feel somewhat more reconciled to the delays which have taken place in the reform of our own institutions, after contemplating the infinitely worse condition of those of our neighbours. We will summarily indicate the chief defects:—1. The Student commencing attendance on lectures is not required to give any proof of preliminary education. 2. The courses of lectures are only of three or four months' duration for two years; the various subjects, too, being treated of, not successively, but simultaneously. 3. Medical colleges obtain charters with great ease in the different States, and there are already thirty-eight such institutions, invested with the double power of teaching and conferring the degree of M.D. A fierce competition prevails among these bodies, and various inducements are held out to students by some of them, such as low prices and long credit, in order to swell the number

of their pupils. 4. Not only is the duration of the session ludicrously short, but several subjects are ill-taught, and others not taught at all. Thus, practical chemistry and pharmacy meet with little favour; while in few of the institutions are lectures delivered on botany, or on comparative or pathological anatomy; and practical anatomy itself is so neglected, that some of the colleges announce in their circulars, attention to it will not be made obligatory! The number of professors engaged in teaching in the European schools is more than double that employed in the American ones, and sometimes four or five times as great. 5. Hospital attendance seldom forms any portion of the system of instruction, and the reporters say, that even in large cities like New York and Philadelphia, not more than one in ten of the pupils attend regularly at the hospital. Moreover, in most of the colleges all attempt at such teaching is eschewed, and a wretched substitute called "College Clinics" resorted to, at which, perhaps, the pupil may see the same patient but once or twice, when he comes for advice. 6. The system of examination is bad; for the professors examine their own pupils, and decide upon their admissibility. In some of the institutions these examinations are quite private, each professor testing the capabilities of the candidate alone and apart from his colleagues. "Three out of seven, or two out of six, negative votes are usually considered necessary for rejection. Thus a candidate may be wholly unprepared in one or two of the most important branches—anatomy and surgery for instance—and yet receive his degree!" Lastly, if a pupil finds himself unable to undergo even such an ordeal as this, he need not despair, but may at once set himself up as doctor, only two or three of the States imposing any examination, or even any education at all, as a necessary preliminary to practice. After perusing the above statement, our readers will, we doubt not, feel somewhat amused at the ignorance or effrontery that could pen the following paragraph:

"In England the loudest complaints are made, not against the higher order of medical men, whose time and means, and inclination, have led them to seek their education at the best sources, but against that numerous body of active practitioners, who belong acknowledgedly to an inferior order, and who are known as 'medical men,' or 'licentiates,' or 'practising apothecaries.' An endeavour is being made in that country, to change the existing order of things in this respect; and while we hold that our own physicians, graduates of almost any of our medical colleges, are fully equal, and in many respects far superior to the general practitioners of England, we think that a change is imperatively demanded with us also." (Vol. ii. p. 347.)

Happily for us, the public is of a different opinion, for while in this country actions for malpraxis are very infrequent, in America they are of almost daily occurrence. When we perused an account the other day, that these had now arrived at such a pitch, that several surgeons refused undertaking surgical cases without receiving guarantees against such actions, we felt disposed to commiserate with our professional brethren; but now that we find from this report, that 1600 diplomas are granted annually (the rejected candidates scarcely bearing any appreciable proportion) by colleges, most of which are unprovided with hospitals, and some of which do not even insist on dissection, our condolence is most unfeignedly addressed to the public; and we feel but little surprise, that, in a country thus provided with legitimate practitioners, charlatanism

is more rife than in any other, and believe that legislative interference for its suppression would be of little avail. As might be supposed, this facility of obtaining degrees has inundated the country with doctors infinitely beyond its requirements. The reports calculate, that the proportional supply of graduates is five times greater than in France, and that while in London there are 600 persons able to pay to each practitioner, and in Paris 413, there are but 375 in New York. It is very doubtful whether these figures are exact; but at all events, the fact is certain, that the United States is very much over-supplied, and that a natural consequence has been "to beget a species of rivalry and competition for practice, which is calculated to degrade the profession still farther." The first step, therefore, insisted upon by the Committees, is the placing some restraint upon this *ad libitum* manufacture of diplomas, by increasing the requirements and severely testing the qualification of candidates. In a country, too, which avowedly tolerates, and even in one or two of its States encourages, illegitimate practitioners, and one in which legislative restrictions would be more difficult of application than in any other, it is obvious that it is to improved education alone that the profession can look for the advancement of its interests. The Medical Association has, therefore, directed much attention to this point; and although some of their recommendations seem scarcely sufficiently searching, yet they are probably as much so as is at first practicable, where so many interests have to be opposed or conciliated. What some of the obstacles to be contended with are, may be judged of by the fact, that so learned a body as the Medical Faculty of Harvard University, have published a formal defence of the preference they give to four months' courses of instruction over the six months' recommended by the Association.

*Reports of the Committees on Medical Literature.*—We have looked at these with curious interest. The two reports are each divided into two sections, the one treating of the Periodical Literature, and the other of Original Medical Publications and American Reprints.

*American Periodical Literature.*—It appears that a medical periodical, the 'New York Medical Repository,' was established as long ago as 1797. The oldest of existing journals, and certainly by very far the best, is the 'American Journal of the Medical Sciences,' commenced in 1827. Both in respect to its original articles and reviews, and the general tone that animates it, this publication fully justifies the high encomium bestowed upon it in one of the reports before us. We cannot say much in favour of those of the others (amounting, it seems, to twenty in number), which have come under our notice; but we prefer letting the authors of the Report for 1848 characterise them:

"Taking into consideration the usual difference of type in the original and borrowed matter, and the very liberal extracts which the reviewers commonly make from the works before them, it will be found, that a very large part of all the journals is made up of quotations, and to a considerable extent of the same quotations, whatever may be the particular journal examined. The Committee have been struck with the fact, that the same articles have been presented over and over again to their notice, in many different periodicals, each borrowing from its neighbours the best papers of the last preceding numbers, so that the perusal of many is not so much more laborious than that of a single one, as would be anticipated. The ring of editors sit in each other's laps, with perfect propriety, and



great convenience, it is true, but with a wonderful saving in the article of furniture.

"In making these remarks, it is not intended to undervalue the great amount of intelligence and industry embodied in these periodicals, or to make any return of ingratitude to the faithful servants of science and humanity, who, in the midst of innumerable distractions, and often at an absolute sacrifice of their material interests, are giving their time, and health, and substance to the demands of the most exacting department of mental labour. The task of filling a vessel which had no bottom used to be thought a severe punishment enough for regions where the art of torture was a science; but to fill a quarterly, or monthly, or weekly receptacle with the pure distillation of two or three brains which have been tapped once, thrice, or a dozen times a quarter for an indefinite period, is more than mortal stamina can support. The natural inference is, that no journal should be established which has not a pretty wide intellectual constituency to support it, unless it wishes to live upon the common stock, without contributing a fair proportion in its turn." (Vol. i, p. 257.)

In the Second Report, the cheapness of the medical periodical literature is made a subject of congratulation. We must confess that, when we consider the mode of construction above described, the fact of neither editors nor contributors generally deriving any emolument, and the absence of the grievous taxes which weigh so heavily upon literary productions in this country, this great relative cheapness is by no means obvious. Thus we are told that the 'American Journal' publishes 1400 pages of reading for 5 dollars, the 'Medical Examiner' 900 pages for 3, and the 'Boston Journal' 1000 pages for 3 dollars. But does not our own Journal, the editor and contributors to which are all remunerated, publish more than 1100 pages for 6 dollars? And are not our voluminous weekly periodicals, the getting up of which entail great expense, published at nearly the same ratio? We are, at all events, best satisfied with the English system, and think the estimate of price, by mere quantity, somewhat fallacious. It is true we cannot compete with our transatlantic brethren, when they confer upon us the questionable favour of republication,—at 3 dollars a year!

The portion of the reports, however, to which we have turned with most curiosity, were those relating to the "Original Medical Publications," having been scarcely aware of the existence of any such. The result of our examination is that, although one of the reports is retrospective, with the exception of a few works on practical surgery, one or two on midwifery and medical jurisprudence, and a few on the fevers of the country, books for students seem alone to have occupied the attention of medical authors. Contrasted with the fertility which has been exhibited by the medical press in Europe during the last half century, the sterility which has characterised that of America is as remarkable and as retributive as that which distinguishes the general literature of the country. The profession there is beginning to feel the rank injustice of the course pursued towards the authors of this country, and to observe the suicidal effect which the policy has upon its own native literature; and we are rejoiced to learn, that the Medical Association has appointed a committee for the purpose of memorialising Congress upon the subject of international copyright. The extent of the evil to be removed is adverted to in the first of the reports we are now considering:

"It cannot be denied, that the great *forte* of American medical scholarship has

hitherto consisted in 'editing' the works of British authors. The committee are not disposed to disguise the fact, that this business has been carried on in a very cheap and labour-saving fashion; a tacit alliance between writers and publishers has infused the spirit of trade into the very heart of our native literature. The gilt letters of the bookbinder play no inconsiderable part in the creation of our literary celebrities. Sometimes the additions by the 'American Editor' have been real and important, oftener nominal and insignificant. The following calculation of the proportion added to different recently-published works, taken at random, will show the average amount of material so contributed. The editor's proportion was, in two instances, 1-4th; in two more, 1-8th; in one, 1-9th; in another, 1-10th; in others, 1-15th, 1-17th, 1-19th, 1-20th, 1-28th, 1-59th, 1-65th, 1-90th, 1-107th; and in one instance, such a sprinkling as a single penful of ink might furnish, and leave enough to spare for a flourishing autograph. The fairest fruits of British genius and research are shaken into the lap of the American student; and the great danger seems to be, that in place of the genuine culture of our own fields, the creative energy of the country shall manifest itself in generating a race of *curculios* to revel in voracious indolence upon the products of a foreign soil." (Vol. i, p. 287.)

Having observed this healthy tone of feeling upon the subject, we were not a little surprised at the extraordinary defence set up in the following passage of the Second Report, emanating, however, from a different hand:

"In contemplating our medical literature, three prominent aspects are presented; first, its mixed and fused condition; second, the great predominance of English authorities; and third, its general character of excellence. The medical literature of our country, beyond all others, is made up of rich and copious infusions from every quarter of the ancient and modern civilized world. Into our capacious reservoir are poured the best effusions of the French, German, Italian [*via* England be it observed], and English press. In this we show, that science is truly cosmopolite, and that we prefer truth, no matter in what soil it may have been nurtured, and under whatever sun it may have been quickened into life, to error, though warmed into life, and animated into luxuriance, by our own skies. Nor do the materials brought from other shores remain inert and cold amidst the profusion of our own collection; but mingled together, a rapid action of composition and decomposition ensues, till new currents are set in motion, and fresh creations arise." (Vol. ii, p. 411.)

Upon persons who thus hug their chains, argument and remonstrance would be lost, and we abstain from offering either: and we are only glad to find by the above-mentioned resolution of petitioning Congress in favour of a copyright act, that the majority of the Association shows its anxiety to rid itself of this half-bred mongrel description of literature.

*Reports of the Committees upon Medical and Surgical Science and Practice.*—The great bulk of both volumes is composed of Reports upon the progress of the various branches of Medical and Surgical practice; and although the instructions of the committees were only to report upon these as far as America had contributed to them, they soon found that this would be limiting their operations to a startling extent, even in some cases to the returning *nil*; and the result is, that by far the greater portion of what is recorded is not American, at least save by appropriation; and the Reports, in fact, form only another variety of the Retrospects published in England and Germany. We may abstract a few of the particulars contained in them, with which the English reader is not already acquainted.

*Poisoning by Tartar Emetic.*—Mr. Gleaves, of Tennessee, reports an interesting case, which is believed to be singular from the great quantity

(a tablespoonful) taken without death resulting, and from its exciting a development of pustules. A young man in good health took it at 3 p.m., and though he drank large quantities of warm water, and tickled the fauces, vomiting did not occur for an hour and a half. Within the next two hours he vomited three times, and had profuse watery evacuations. Dr. Gleaves found him cold, pulseless, speechless, and apparently insensible, the action of the heart being feeble and intermitting. Under the use of laudanum, decoction of galls, sinapisms, and brandy toddy, reaction was established in about seven hours, and he then became tormented with thirst and a burning sensation in the fauces, stomach, and bowels, the matters vomited being tinged with blood. On the third day the fauces were found covered with pustules, and the two following days these spread over the body and extremities. He gradually recovered. (Vol. ii, p. 103.)

*Extraordinary Success of Lithotomy.*—In the first report on the Progress of Surgery, it is stated that lithotripsy makes little way in the United States, which it is supposed may be in some measure due to the unprecedented success of Professor Dudley, of the Transylvania University, in his lithotomy operations. Dr. Bush states, in the 'Western Lancet,' that up to the beginning of 1846, the Professor had operated upon 185 cases of stone, of which number 180 are reported as successful; the success, too, not arising from the cases having been selected, since, out of 188 subjects presented to him, 185 were cut! Dr. Bush attributes these results to the thorough preparation of the general system made by Dr. Dudley preparatory to the operation; but we trust we may not be wanting in courtesy in stating, that some fallacy must prevail in the statement of his result, which seems to us utterly incredible.

Dr. Nathan Smith, in the second report on Surgery, thus speaks of *lithotripsy in the young*:

"It has been generally thought, that lithotripsy is ineligible in very young subjects, on account of the narrowness of the canal, and the unmanageableness of the patient. The chairman of this committee, however, has performed lithotripsy, with Jacobson's and Heurteloup's instruments, on infants two years of age, in no less than four instances, and on several other very young subjects. In one respect he has found the operation more certain than in adults. The bladder expels the fragments more promptly. He has been somewhat discouraged with the operation on old subjects, from the fact, that fragments small enough to pass with facility, have been retained for months, probably on account of the columnar condition of the bladder. He has performed lithotripsy twice on the paralysed bladder, not a drop of urine ever being discharged except by the catheter. The removal of the stone was effected partly by bringing away, at each operation, with Civiale's modification of Heurteloup, a pinch of the calculus in the beak of the instrument, and partly by washing away the debris with a syringe, through a large, double-barrelled catheter." Vol. ii, p. 227.)

*Deformity after Fractures.*—Professor Hamilton, of Buffalo University, has collected into a tabular form the results of the treatment of fractures, whence he draws the deduction, that the knowledge of the great frequency with which deformity occurs may serve to protect some of the defendants who are now so frequently prosecuted for malpraxis. Certainly, transatlantic surgery is not so much in the ascendant in this matter, as it appeared in the statistics of lithotomy just quoted. Dr. N. Smith says:

"These tables embrace 136 cases, collected from various sources, including 9 managed by empirics. Twenty or thirty of the instances were observed in the

persons of medical students, and they 'being generally from the better classes, it is presumed would have received the best treatment which the country could furnish.' The results are sufficiently humiliating, and will surprise, perhaps, even those who have not formed a high estimate of the resources of our art. For instance, only 2 cases of fracture of the shaft of the femur out of 15, occurring in adults, have resulted without deformity; of 22 cases of fracture of both tibia and fibula, 17 are imperfect; of 13 instances of fracture of both radius and ulna, 6 are imperfect; of 14 cases of fracture of the humerus, no less than 9 exhibit imperfect results." (Vol. ii, p. 222.)

*Dislocation of the Shoulder.*—Dr. Smith furnishes an account of the method he has employed for many years in reducing this dislocation :

"Its peculiarity chiefly consists in the application of the counter-extension to the opposite wrist, extension being made from the wrist of the dislocated member. Steady traction from the two wrists, in the horizontal direction, will be observed immediately to erect the head, neck, and chest, and to restore the symmetry of the two sides of the body. This at once calls to our aid the action of numerous muscles. The object of counter-extension is, of course, to fix the scapula of the injured side. Traction from the opposite side most effectually does this; first, by erecting the spine, which otherwise yields to the extension, and, more directly, by communicating support to it through the clavicles which, in front, fixed the two scapulae together, and behind through the muscles and tendons, which effect the same on the back. Support one scapula and you necessarily sustain the other.

"We accomplish reduction in many instances by simple traction, for a few minutes, from the two wrists. In difficult cases we place the patient in a chair, pass a sheet or towel over the top of the scapula, and tie it beneath the chair. The knee of the surgeon is then placed in the axilla, and traction is steadily made from the two wrists, till the muscles are observed to yield and the head to be disengaged. Then the surgeon directs the arm to be depressed, while at the same moment he urges his knee into the axilla by extending his foot. We have succeeded thus in cases of two months' standing, and where other methods, in judicious hands, have failed." (Vol. ii, p. 224.)

*Obliteration of the Aorta.*—This case has been recently detailed by Dr. West in the 'Transactions of the Philadelphia College of Physicians.' The man, æt. 32, was remarkably muscular and athletic, having the superior half of his body more developed than the lower. He died suddenly from the rupture of an internal aneurism; and it was found on tracing the aorta beyond the origins of the great vessels, that its cavity was entirely obliterated beyond the *ductus arteriosus*. At the point of obliteration it presented a well-defined and regular contraction, which looked as if it had been produced by a ligature thrown around the artery. Beyond this the vessel resumed very nearly its natural dimensions for the rest of its course, giving origin to its usual branches, the upper pair of intercostals coming off immediately below the stricture. The internal mammary arteries, which pursued a very tortuous course along the thoracic parietes, were as large as the internal iliacs, as were also the epigastrics,—these vessels constituting the main channels for continuing the connection of the circulation above and below the stricture. (Vol. i, p. 273.)

*Prevention of Anchylosis after Fracture of the Condyles of the Humerus.*

—Dr. J. C. Warren observes, that the usual mode of treating this accident favours the production of its worst consequence, the loss of movement in the joint. Angular splints intended to prevent motion are applied for a fortnight or so, and then passive movements are made in order to procure the free use of the joint. But the fractured piece has become sufficiently

fixed to produce partial ankylosis, and so much pain is caused by the passive movement, that this is often omitted until permanent stiffness has taken place. He recommends, 1st, that no splints should be applied, and proper means taken at first to prevent inflammation; and 2d, that the patient should be accustomed early to daily movements of extension and flexion. 3d. When the action of the joint becomes limited, he overcomes the resistance daily by force, until all tendency to stiffen ceases. (Vol. i, p. 174.)

*Cerebral Disease after Ligation of the Common Carotid.*—

"Within a year or two past, attention has been in a particular manner directed to derangement of the cerebral functions following ligation of the common carotid. These cerebral symptoms are attributable either to cutting off the direct supply of blood to the brain, or to disease consequent upon the altered condition of the circulation in that organ. Nearly one fifth of the recorded cases of the operation in question are found to have exhibited it in a greater or less degree; and the frequency of its occurrence has been singularly overlooked by practical surgeons. Two cases have been forwarded to the committee by Dr. Mettauer, of Virginia, in which it was observed. In these the vessels were taken up, in one instance, for an anastomosing aneurism of the antrum and nasal cavities, and in the other for the cure of a false aneurism. Both patients had lost large quantities of blood previous to the operation. In each case partial hemiplegia of the opposite side to the artery which was ligatured was noticed in a few hours, and was followed by convulsions and delirium. In one of the instances death occurred on the eighth, and in the other on the tenth day. Autopsic examinations showed softening of the medullary substance on the side opposite to that on which the vessel was tied, while the hemisphere corresponding to it was healthy, though pale and bloodless." (Vol. i, p. 175.)

*Innocuity of Chloroform in Obstetrics.*—The "Committee on Obstetrics," in allusion to the fact that no fatal result has ensued from the employment of chloroform in obstetrics, although probably it has been more frequently thus used than in surgery, observes:

"In a communication with which the committee were favoured by Professor Channing, a very ingenious answer to this question is offered, which is here submitted to the consideration of the Association. The condition of the surgical patient, says Professor Channing, when subjected to the anæsthetic influence, is widely different from that of the parturient female. In the former, the agent is used as a preparative to the operation, to prevent pain; pain is not present, it has as yet exerted no influence on the nervous system, that system has not as yet been exhausted by suffering: it is in its integrity, and has of course its greatest capacity of impression, the greatest amount of sensibility, with the least power of resistance. The whole nervous power is brought in relation with the cause affecting it, and yields to the impression. For the most part, the mind consents to the same thing, and no moral resistance is made. How is it in labour? Here pain is present, and has been for a long time; the nervous system has been greatly taxed; its power has been, so to speak, used up. Impressions upon it are weaker than they would be under other circumstances, the system comes readily under the anæsthetic influence, very little ether or chloroform is necessary, the consciousness need not be destroyed, sensibility need never to be entirely abolished. Neither of these states is desired or aimed at by the judicious accoucheur." (Vol. ii, p. 247.)

Besides the Reports whence we have made the foregoing extracts, there are others upon the recent history of anæsthetic agents, public hygiene, the adulteration of drugs, (a practice pursued to so fearful an extent by the English drug-exporters, as to have required a special enactment for its abatement,) and indigenous botany. The last of these occupies nearly 300 pages of the second volume. Appended to some of the Reports are a

few original papers, namely, two or three on recent Epidemics, two cases of Retroflexio Uteri, Dr. Buck's account of his treatment of Œdematous Laryngitis by Scarification, and of his mode of demonstrating the fascia of the perineum and sheath of the penis, and a paper by Dr. Dubois on "Ophthalmitis Postfebrilis." We have already noticed Dr. Buck's treatment of Œdematous Laryngitis (*Brit. and For. Med.-Chir. Rev.*, vol. III, p. 536), and will here quote his description of what he terms "*a new feature in the anatomical structure of the genito-urinary organs not hitherto described.*"

"The penis and scrotum are to be circumscribed by an incision at the distance of three fingers' breadth all round, and crossing the perineum at the anterior margin of the sphincter. The dissection of the skin and subjacent cellular and adipose tissues is to be made towards the penis, on the level of the fascia lata laterally, and of the perineal fascia posteriorly, and carefully continued to the body of the penis, as far as the corona glandis. By this means the penis, as well as the suspensory ligament, is denuded of its loose moveable investments.

"An incision is then to be made along the dorsum of the penis, exactly in the median line, splitting through the suspensory ligament, and extending forward to the corona, between the dorsal vessels and nerves that run parallel on either side. The adhesions of the sheath along the dorsum are firm, and require careful dissection; the blood-vessels and nerves being raised with it, serve as a guide to show the line of adhesion. The dissection being prosecuted laterally as well as inferiorly, and at the extremity, the entire corpus cavernosum is enucleated, the muscles of the perineum being raised with the sheath. It is now clearly seen, that the suspensory ligament from above, and the perineal fascia from below and laterally, form one continuous membrane with the sheath, enclosing the corpus cavernosum in its cavity, and embracing the corp. spong. ureth. between two layers, one of which passes above, and the other below it. The excavated base of the glans adheres inseparably to the outer surface of the sheath, while, by means of its inner surface, it caps the summit of the corp. cavern. Its adhesions are most strong at the extremity of the corp. cav., along its dorsal surface, and at the insertions of the erector and accelerator muscles. It is thickest around the corona, along the dorsal surface, and where it forms the suspensory ligament. Zones of vessels run at regular intervals in the direction of the circumference of the penis, from the dorsal trunks to the corp. spong. between the layers of the sheath. The cavity formed by the sheath, and occupied by the corp. cavern., is limited posteriorly by the triangular ligament.

"That portion which covers the perineal muscles, and has been described by authors under the names of the superficial fascia of the perineum, inferior fascia, and ano-penis fascia, arises laterally from the ascending rami of the ischium, and descending of the pubis, as far forward as the inferior edge of the symphysis, where the two layers meet and form the suspensory ligament. Posteriorly it is continued over the transverse muscles, and, folding around their edges, is prolonged upwards into the ischio-rectal fossa. It also sends off from its upper surface membranous septa between the accelerator muscles in the middle, and the erectors on either side, to join the triangular ligament, and thus forms three distinct and independent sheaths that are confounded anteriorly with the common sheath investing the corp. cavern." (*Vol. i, p. 369.*)

Admitting the exactitude of the descriptions of Velpeau and Colles, Dr. Buck conceives, that he has thus first shown the peculiar relations of the sheath to the corpus spongiosum urethræ and glans penis.

*Dr. Dubois on Ophthalmitis Postfebrilis, as observed after Typhus Fever in the City of New York in 1847-8.*—As far as the author is aware, this is the first occasion upon which severe ophthalmia has followed typhus in

America. Mr. Hewson published five cases which occurred in his practice as early as 1815, all recovering under arteriotomy, and calomel and opium. In 1827 Mr. Wallace published a paper in the 14th volume of the 'Medico-Chirurgical Transactions,' on a "peculiar inflammatory disease of the eye following fever." It usually appeared after relapsing fever, and out of forty cases the right eye was affected in thirty-eight, both eyes suffering in only two cases. The disease presented two stages, in the first of which amaurotic symptoms were alone present, and in the second inflammatory ones were added, weeks or even months sometimes intervening between these two stages. Accounts of a similar disease, as it appeared in Dublin in 1826-7, and in Glasgow in 1843, are furnished by Dr. Jacob in the fifth volume of the 'Irish Medical Transactions,' and by Mr. Mackenzie in the 'Medical Gazette,' 1843. The fever which preceded the present ophthalmia occurred at New York in 1847-8; and Dr. Dubois observes, "there are strong points of resemblance between the several epidemics of fever, though occurring at different times, and in places widely separated, viz., the suddenness of the attack, the energy with which the disease was developed, its comparatively short duration, great liability to relapse, its contagiousness, and the fact that no special morbid lesions were discovered after death."

In the present ophthalmia, the first symptom was always the appearance of *muscæ volitantes*, which were usually soon followed by dimness of sight and uneasiness in the eye; the proper inflammatory symptoms were some days, or even several weeks, before they set in; but then very acute ophthalmitis, with much pain, intolerance of light, and loss of vision, were rapidly developed. When not seen in its early stage, or if the history of its progress is unknown, it may be readily mistaken for ordinary iritis, or in its more advanced stage for the chronic choroiditis of Tyrril. Twenty-nine patients were admitted at the New York Eye Infirmary between September 1847, and May 1848, the youngest being 2 and the oldest 49 years of age. Thirteen were between the ages of 10 and 20. There were sixteen males and thirteen females. The right eye was affected in fifteen, the left in eleven, and both eyes in three. The period after the fever at which the ophthalmia appeared, varied from several days to seven months, the patients, with the exception of six, who were still suffering from the debility consequent on fever, being apparently in good health. In several cases the *muscæ* appeared while the patients were still ill with fever. The interval between the amaurotic and inflammatory stages varied from a few days to two weeks. In only three cases did the patients escape a relapse of the fever, and in one case there was a second relapse. The cure occupied from ten days to seven weeks. Nearly one half were cured in from two to three weeks.

"I have never met with a case among the better class of people. I have heard of two medical gentlemen, who, after recovering from fever, were troubled for some time with *muscæ*; but as yet the disease has extended no further."

*Treatment.* Depletion, by means of *cupping*, was resorted to in the majority of cases and with decided relief, it being repeated several times, at intervals of two or three days. Venesection was never resorted to; and seeing that the disease usually occurred in pale and feeble subjects, recently recovered from a fever which itself has required supporting treatment, Dr. Dubois deprecates the active depletion counselled by Hewson and

Mackenzie. He agrees, however, with all preceding writers, in estimating the high value of *mercury* in a disease where such vital parts of the organ, as the retina and iris, are so seriously implicated. Mr. Wallace did good service in recommending *tonics* in this disease, but he much exaggerated their efficacy, considering cinchona rather as a specific than as a valuable adjuvant. Other observers have not found quinine possessed of the efficacy Mr. Wallace attributed to it. In most of his cases, however, great debility, either from the prior disease or the treatment, prevailed. We will terminate our account of Dr. Dubois's little paper with the tribute he pays to a work well-deserving of it, and one which we think is scarcely sufficiently appreciated by the profession.

"In the treatment of the diseases of the eye, as in that of most local affections, the state of the patient's system has been too much neglected; and I cannot permit this opportunity to pass, without expressing my opinion of the great obligation the profession is under to the late Mr. Tyrrell, for so ably enforcing by argument, and illustrating by examples, as he has done in his valuable work on the eye, the necessity of ascertaining whether your patient is, as he so aptly expresses it, 'above or below par.' A want of attention to this point has been the means of losing many an eye, that otherwise might have been saved. Those much engaged in treating diseases of the eye, must frequently have met with cases, particularly those of syphilitic iritis, presenting all the appearances of active inflammation, which cases, under the ordinary routine treatment by depletion, mercury, &c., have gone on from bad to worse, and which have afterwards yielded like magic to a generous diet and the use of quinine." (Vol. i, p. 382.)

In concluding our notice of these volumes, although approving of the general tone and spirit (save as to the exceptions we have mentioned) which have prevailed among those engaged in assembling the materials, we must be allowed to observe that, if the Medical Association is ambitious of assuming a position among the various scientific bodies that issue their *Memoirs* and *Transactions* in Europe, a very different class of literary productions must be looked for at their hands. They must not content themselves with merely recapitulating in reports and retrospects what has already been published,—a work that might be accomplished by a far less massive organization,—but must themselves largely contribute to the general stock the results of experience and the fruits of meditation.

#### ART. IV.

*A Practical Treatise on Diseases affecting the Skin.* By the late ANTHONY TODD THOMSON, M.D. F.L.S., Physician to University College Hospital, &c. &c. Completed and Edited by EDMUND A. PARKES, Physician to University College Hospital, &c.—London, 1850. 8vo, pp. 440.

WHEN a medical man enters the chamber of sickness, he is expected there but for one purpose, viz. to *do* something. Before him are placed misery and pain, suffering and misfortune; and those who have summoned him within the sphere of these sorrowful handmaidens of our humanity, have predetermined the purpose for which his presence has been sought. Disease is there to meet him, and its cure or its palliation is required. The world recognises him only by these powers of his high and noble art; and in proportion to his real or supposed possession of these powers, does he



enjoy its confidence and estimation. Often, as it becomes mournfully evident, that as regards a *cure*, the physician's hands are powerless, and that art can offer no passport for another sun, is he yet deemed responsible for an alleviation from pain, for a closing euthanasia, ere—

“Pain, work, and warfare be for ever o'er.”

“Deeds, not words,” should, then, be the motto of the healing art; for whatever may be the amount of that close and persevering study of the science of medicine, of that precise and formal ratiocinative consideration of what alone can lead to a proper *action*, a physician may have credit for among his brethren, it will pass for nothing with humanity at large, unless the above important factors be worked out into their fullest products—alleviation or cure. If a man *practises* his profession, these products he must come prepared with, to the full extent that the science of his day can offer; since, without them, for the most profound pathology the world has very little respect. So dominant, indeed, in the public mind, is this aspect of the matter in respect to medicine, that it is apt to jump at once to the conclusion of the argument,—treatment,—with but slight consideration of the long and intricate series of links in the chain of ratiocination, without the perfect establishment of which the scientific pathology of our day denies that a satisfactory therapeutics can be affirmed. It is in this tendency that the stronghold of *quackeries* chiefly lies. The inquiry made of them simply is—what do they profess to *do*? Unlike the delinquents of the “*piombi*” and “*pozzi*” of old Venice, the *question* is never put to them to elicit the how, the wherefore, or the why. Their pretence “to do” something is enough; and well might it be said, though “more in sorrow than in anger,” “*Juvenis, tua doctrina non promittit opes: plebs amat remedia.*”

Almost below contempt (viewed in a scientific light) as is this disposition to seek and dwell upon the *ends*, without a recognition of the necessary *means* for their attainment, so prevalent in the public mind in respect to medicine, yet it appears to us not the less unwarrantable and further from his duty, for the practitioner in our art to rest satisfied with the mere intellectualities (if we may so term them) of medical science, and to lose sight of the great ends, to which the latter must ever tend.

“Man,” remarks an able writer of our day, “may be said to have been created for two purposes—to *know*, and to *do*. We can conceive of a mind utterly passionless, gazing with piercing transparency of vision upon truth; but yet unimpelled by motives to any sphere of action whatever. A being thus formed might possess the most commanding intellect, but it would never be fitted to fulfil any destiny. The intellectual and the practical side of humanity, however, are not severed entirely from each other.” (Morell's *Speculative Philosophy of Europe*, vol. ii, p. 334.)

Vast as is the literature dealing with medicine, and various the forms of minds and the purposes of those by whom it is produced, it is not to be wondered at, that such literature shall be stamped by very different characters, in relation to these acts of *knowing* and *doing*; or that these great evidences of our personality shall be thus seen to assume a multiplicity of shapes. If the book is a truthful index of the man who produces it, how many men must there not be, who, after all the education which they have received, or which at least has been proffered them, yet remain wrapped in the fallacy that we see enveloping the public,—a belief in an *end* without an adequate *means*, a means judged able to be arrived

at or obtained *per saltum*, as if, in our art, cure and alleviation were products of intuition, instead of being results deduced from a long process of patient observation and logical inference.

There are other works, again, which seem to indicate that certain followers of our art revel in the intellectualities and the scientific elucidations of medicine, and repose in them as the mere exposition of a set of beautifully constructed logical formulæ; evincing but little desire to interweave, with all this exemplification of the *knowing*, the thread of that principle which can alone endow the structure with that noble vitality, whereby the science of medicine is made capable of *doing* service to mankind. Such a want is apt to throw a coldness, a languor, the feeling of an absence of some living or motor power, over labours which are the products of minds of very high and trustworthy order. The contrast felt on passing, whether it be from the lamentable productions of the class first alluded to, or from those of the purely intellectual school last mentioned, to the works of those amongst us who appear to combine the highest attributes of the *knowing*, with the most vivid conceptions of the *doing*, is very remarkable. Without intending to draw any invidious or immediate comparison between the writers of this latter school, yet we may safely say, that in no case is this contrast felt to be greater, than when we turn from the writings of Alison, Latham, Brodie, Holland, Watson, Stokes, and Graves, to those of either of the classes we have before alluded to. It has been said, that we can indicate the habits of a man from the company he keeps; it is not the less true, we think, that we can arrive at the form and capacity of his mind from the books that he generally peruses. Let us know, with respect to the student, that the works we have just mentioned, and others like them, stamped with the double principles of intellection and of action, have been and yet are the "lamp to his feet;" and we may fairly predicate, we believe, that his path will be above mediocrity. It is this combination which is most deservedly held in the greatest estimation by all the higher minds practically engaged in our profession; and we could not adduce better evidence in support of our position, than that which is afforded in the following citation from one of the eminent authors just alluded to:

"It is much to be lamented, that any eminent master of pathology, who, while he is concerned with the nature of disease, has seemed at home, and in earnest, and satisfied with his work, pleased to instruct, and gaining favour for his instruction as he goes along, should come at last to the treatment of disease as to a humbler and less worthy portion of the physician's care. For this ought not to be. Medicine, as it begins to touch upon higher interests, even the interests of life and death, should feel itself in alliance with higher motives than any which can be thought to quicken its pursuit as mere science. For now it claims a sort of moral respect in the handling, it calls upon the conscience as well as the intellect for more caution to avoid error, and more fearfulness of overstepping the truth." (Latham's Clinical Medicine, Preface.)

We would refer our readers, for further illustration, to the pages preceding the above lines in Dr. Latham's admirable book.

The valued and lamented author of the greater portion of the work at the head of this article, was one eminently belonging to those who saw in medicine its purpose. A mere intellectual medicine he never rested in; the great end was ever in his view; nor was the belief in an attainment of the end dreamt of, without a recognition of and adherence to those means and processes, by which alone a satisfactory termination could be insured.

With all our respect for Dr. Thomson, however, we do not mean to affirm that he combined with the high development of the *practical*, the same relative amount of the *intellectual*; or that he could dive into the same abstract depths, or so comprehensively generalise and seize first principles, as some whom we could name, and in whom the practical element is nevertheless very prominent. We are supported in this belief by the biographer of his earlier years, who says:

"Throughout all this dawn, his character was the same. He was distinguished by no brilliancy of talent; but by the more sure, and in a majority of instances, the more useful qualities connected with steady patient progression." (Memoir, p. xv.)

And this represents, we believe, the tone of his after career, in which are seen the laborious exertions of a mind belonging rather to the practical than to the reflective type. We would by no means wish, however, that too forcible a meaning should be attached to this opinion, to the disparagement of Dr. Thomson; for we have found ample evidence in the work before us, of his mind having been endowed with some of the broad and characteristic features of the philosophic pathologist. These are exemplified in his frequent desire to throw back many of the less important varieties of cutaneous affections, endowed by many writers with special appellations and distinctive characters, into the more general types into which they most evidently merge; and in the regard paid to the constitutional states of the organism, as affording broad divisions for the systematising of these affections, which are linked with them in the chain of causation. To say nothing of many others of Dr. Thomson's works, we would yet remark, that those who are acquainted with his 'Lectures on Botany,' and with the general aspect of botanical works at the period these 'Lectures' were published, must be ready to admit his possession of the truly scientific spirit. It may have been that the practical element was more in dominance than the reflective in after life, because of the peculiarity of his early career at Chelsea, marked by the publication of his '*Conspectus Pharmacopœiæ*' and '*London Dispensatory*.'

The practical tendency of the present work is especially seen, not only in the prominent position which therapeutics—speaking generally—hold in it, but in the minute details which are given in reference to the agents to be employed in treatment. No vague hints as to general treatment merely satisfy the author; but specific instruction in hygiene, and as to drugs, their doses and combinations, &c., are laid down in detail, and formulæ are given, together with abundance of cases illustrative of the treatment recommended. This renders Dr. Thomson's work valuable to the student and junior practitioner, to whom it is very easy, though often but slightly useful, to say as some might, "We have frequently found a combination of the bichloride of mercury and hydrocyanic acid useful, together with an ointment composed of the iodide of arsenic, and the biniodide of mercury with hemlock," &c. &c., drugs, be it remembered, often endowed with no little potency of action, and some of which have been but rarely if ever used before by them. The readers we have alluded to would often be most thankful if such hints as the above were displaced by available formulæ of the agents recommended. In no class of diseases are the most energetic weapons, powerful for evil as well as for good, more unhesitatingly lauded, than in diseases affecting the cutaneous surface; and

in none are so many out-of-the-way agents, given in the most unlikely combinations, more frequently employed. Some dermatologists, too, entering into no specific details with regard to the few agents which they really have found useful, or to which we strongly suspect they almost confine themselves in practice, quite bewilder us by enveloping the simple mention of them in a cloud of other resources put in force by their brethren: *e. g.* "We have generally found the ointments of the iodides of mercury, &c. &c., occasionally the pitch ointment, &c. &c., you may employ an ointment of creosote, &c. &c., Rayer advises, &c., Cazenave is said to employ, &c., Schedel recommends, &c. &c."—and thus extends this interminable paragraph, until we come to regard it, like the seaman's chest in a storm, "everything at the top, but nothing at hand." Dr. Thomson, whilst by no means neglecting frequent mention of what the treatment of others has consisted in, lays fullest stress on, and amply illustrates, that of which he has had most experience, and which he has found most valuable. A main support to the utility of all this, is of course the fact, that as a practical authority on the treatment of diseases affecting the skin, the author deservedly ranked high; and during the last five years of his life, his practice increased very considerably in this branch of therapeutics. (Memoir, p. xxiv.)

"For more than thirty years Dr. Thomson had paid special attention to diseases of the skin; he had observed them with great care and accuracy; and had learned to treat them with singular success. He was well known to the profession, in connection with this subject, by his edition of Bateman, which he illustrated with a valuable atlas, and by his writings in the 'Cyclopædia of Practical Medicine.' " (Preface, p. iv.)

"Through his influence with his rich and benevolent patients in Chelsea, a dispensary had been formed in that parish. To this he was appointed one of the medical attendants. He saw there a great variety of disease; and he found out, in some particulars, his own deficiencies; more particularly, as he was accustomed to say, in *diseases affecting the skin*. He was one who never evaded a difficulty, but met it face to face. He resolved to make himself fully acquainted with that class of diseases; and he succeeded by close attention in alleviating, if not curing, some of the most obstinate. The dispensary acquired a local reputation for complaints of that class; and Dr. Thomson made a large collection of notes and cases, even while hurried from house to house, and often not for days being able to sit down to a single repast." (Memoir, p. xx.)

"At the close of his active and useful career, it was his greatest wish, that his experience should be made useful in aiding his professional brethren in the great object of all practical men, *viz.* the safe and rapid cure of disease." (Preface, p. iv.)

For more than a year previous to his fatal illness, Dr. Thomson had been engaged on the present work; and at the time of his death, three hundred pages of it had been printed. With the exception of the chapters on *Acne* and *Sycosis*, these pages consumed all the manuscript; thus leaving the whole of the *Syphilides*, and the *Tubercular* and *Hæmorrhagic* affections, to be written, together with the *Hypertrophies* of, and *New formations* on, the Skin. The author's nephew, Dr. Parkes, being known to be familiar with his opinions and methods of treatment, was requested to complete the work, and introduce it to the public. This task has been very ably accomplished; and besides an account of the diseases of which Dr. Thomson had not treated, an Appendix has been added, illustrative of some points of the microscopic and pathological anatomy of the eruptions in the Exanthemata, of Lichen, Pityriasis, &c. &c.

Having in our Sixth Number, in a review of Dr. Simon's work, more particularly dwelt upon the microscopic and pathological anatomy of eruptions and alterations of the skin and its appendages, we shall in our further remarks at present limit our attention chiefly to points of interest in relation to the general pathology and therapeutics of these affections. In an extract we have already given from the memoir of our author, prefixed to the work before us (p. 20), it is particularly observed, that he was accustomed to term the maladies we are engaged with, "diseases affecting the skin." On a very superficial view, there may not appear much design in this; but in truth there is in it a deep meaning. It is the intention of Dr. Thomson to imply, that cutaneous disorders are not to be regarded as isolated diseases by and in virtue of themselves, but as evidences of other and more fundamental lesions or constitutional disturbances, one of whose characteristics is, that the skin is sooner or later drawn within the vortex of morbid influences, and manifests its involvement in the chain of diseased phenomena by certain signs, these signs being one or other of what are constantly denominated "Diseases of the Skin;" the disease present, then, not being truly one of the skin, but of something else, of which the cutaneous manifestation is but a symptom or an effect, and which a precise and analytic pathology can reduce to a systemic cause. That this holds good with respect to by far the greater number of the so-called "diseases of the skin," there can be no doubt; though at the same time it must be admitted, that the amount and kind of precision with which we can refer back the particular cutaneous manifestation to such a constitutional cause is very variable. The difference may be well observed, on comparing the chain of phenomena linking the exanthem of scarlatina to the previous infection of the general system by a specific poison obtained *ab extra*, and giving rise to a febrile disorder itself infectious; and that connecting the cutaneous signs of lepra and psoriasis with a particular constitutional taint, which we well know can be hereditarily transmitted, and which is believed to be the cause of the appearance of such signs. That a considerable number of cutaneous affections are always regarded in the philosophic aspect which Dr. Thomson indicates, is no doubt the case, and their treatment is directed accordingly; but there remain others which are not always so viewed, even where an evident connection exists between them and a constitutional taint or the presence of a poison in the system; or else the connection is but tacitly admitted, and scarcely ever acted upon in the treatment to which they are subjected. This error Dr. Thomson's labours will, no doubt, have some influence in correcting. Still there will remain a certain number, in relation with which the most precise pathology can as yet associate no constitutional vice or systemic disturbance; which, in the present state of our science, we are authorised in regarding as really diseases of the skin or of its appendages, and not as mere symptoms or effects of an antecurrent or synchronous abnormal condition of the nervous system or the circulating fluids; and of which it seems that the treatment may be properly restricted to the superficial parts affected.

A broad line of demarcation can be at once drawn, then, between the skin-manifestations of the exanthemata; of some non-contagious, non-infectious, febrile disorders, like roseola, miliaria, urticaria, &c.; of the hæmorrhagic class, which includes scorbutus and purpura; and of the

syphilides, on the one hand ;—and those of certain local forms of eczema, psoriasis, porrigo, herpes, &c. &c., on the other. No cutaneous affection should be placed in the latter division, which cannot be shown to be entirely due to a local irritation *ab extra*, or to a cause having its origin in the superficial parts themselves ; and, at the same time, it must be proved, that the disease thus set up is incapable of contaminating the circulating fluids by absorption into the system, and by the multiplication, in the current of blood, of the poison-germ of the original and local malady. If the cutaneous manifestation be preceded or accompanied by febrile disorder induced by *zymosis*, as in varicella, or by disturbance of the digestive system, as in urticaria, or by nervous and vascular erethism from solar heat, as in lichen tropicus ; or if it be capable of contaminating the general system, as in the case of a syphilitic sore ; or be allied to a condition of dyscrasia, whether hereditarily obtained, as from a scrofulous father, or induced by deficient or improper nourishment, as purpura ; of course such manifestation is not a disease of the skin, but the sign of a disturbance far more deep-seated. That a certain amount and kind of constitutional derangement may accompany or follow a cutaneous eruption, and yet the latter be rightly denominated a disease of the skin, is true ; but this derangement holds quite a different relation to the cutaneous manifestation, from that which it bears in the other class of diseases. Here it is an *effect* of the skin-disease, an effect with no special peculiarity in its nature,—the intense irritation or itching, or the arrestation of the due performance of the functions of the skin, which the local disorder induces, being the cause of the general derangement alluded to. Yet examples of this character are rarer, we believe, than are generally supposed ; for it may be readily assumed, that where such irritation or arrestation, &c. exist, it is chiefly in severe, or chronic, or extensive forms of the maladies ; and this very fact renders it probable, that the cutaneous disturbance is based upon one more primitive. In such cases, then, after all, that which is really the source of trouble is a *disease affecting the skin*, such affection reflecting back much mischief on the system ; and such we know also to happen often enough in other than the class of cutaneous maladies which we have at the moment in view. Bearing in mind the relations of the affections before us with systemic disturbances, we ask the attention of our readers to a further exposition of them ; premising a reference to our article on *Poisons* in our second volume (p. 195 et seq.), as further illustrative of our meaning when employing the terms *poison* and *poison-germ* in the present discussion.

1. The system may be healthy in the first instance ; it is exposed to zymotic influences, by or without personal contact ; the blood becomes infected by a specific poison, the evidence of whose action is seen in the production of a febrile zymotic disorder, accompanied more or less constantly by a cutaneous eruption or exanthem.—Variola, varicella, fram-bæsia, morbilli, rubeola, scarlatina, erysipelas, typhus fever, typhoid fever, cholera.

2. The system may be healthy ; by inoculation a poison is introduced into the blood, which evinces its action by giving rise to constitutional disorder, accompanied with an eruption, &c.—Vaccinia, syphilis.

3. The system is originally tainted ; endowed *in utero* with the poison-germs of certain diseases, accompanied with eruptions, &c., or with some

morbid peculiarity of constitution, or with a diathesis frequently seen to be associated with, or constantly tending to produce, particular cutaneous manifestations.—Syphilis, lepra, psoriasis, ichthyosis, lupus, certain forms of porrigo, eczema, impetigo, and of ecthyma, elephantiasis (*græcorum*), pellagra.

4. The system may be originally healthy; but improper or deficient food, insufficient light and exercise, foul air, with noxious or excessive employment, produce a dyscrasic or other vitiated condition of the system, which is often accompanied with affection of the cutaneous surface.—Scorbutus, purpura, rupia, ecthyma, pellagra, acquired elephantiasis (*græcorum*), lupus, and forms of porrigo, eczema, impetigo, pemphigus, and of pompholyx.

5. The system may be healthy; but from repletion, or the use of an improper or particular article of diet, temporary derangement of the digestive apparatus ensues, along with a variable amount of febrile erethism (sometimes wholly wanting); and these are accompanied by an eruption on the skin.—Urticaria, roseola, forms of herpes, erythema, prurigo, and of acne, &c.

*a.* Or the cutaneous eruption appears to proceed from a general erethism of the system, produced by great solar heat;—as in lichen tropicus, erythema.

*b.* Or it originates in some temporary irritation of the nervous system;—as in strophulus, lichen urticatus, &c., during dentition; in pompholyx from mental emotion or the depressing passions; in certain forms of prurigo and of acne.

6. The system need not suffer either previously to, or synchronously with, the disease of the skin; there not existing, so far as is known, any poison in the blood, hereditary vice, acquired dyscrasia, derangement of the digestive or the nervous systems, of which the cutaneous manifestation can be fairly presumed to be an effect or symptom. Scabies, forms of psoriasis, impetigo, and eczema, of prurigo, pityriasis, and porrigo.

We shall not detain the reader with the non-eruptive morbid states of, and new formations on, the skin, or with the abnormal conditions of the hair and nails; but proceed to a few remarks in illustration of the preceding exposition.

That the poison-germs of variola, morbilli, and scarlatina, which are the types of our *first* class, can be thrown into a healthy system by inoculation, as well as by zymotic aerial infection, no one can doubt. With respect to variola, nothing further need be said; but in relation to morbilli, we would recall to mind the successful experiments relative to inoculation made by Home at Edinburgh in 1758, of Speranza at Mantua in 1822, and of Katona in Hungary in 1842. Home used the blood, Katona the tears, and Willan employed the fluid from the miliary vesicles of a patient with measles; the latter experimenter producing a disease which infected several other children. In most of the cases thus produced, the attack was mild. Dr. Thomson is of opinion that—

“The success of Dr. Home’s experiments should have induced the practice to be adopted; but it is to be regretted, that such a desirable event did not result from their publication. The inoculation ought still to be tried, as there is no doubt that the disease is increasing in severity.” (p. 33.)

Sir B. Harwood produced scarlatina by inoculating with the fluid of the

miliary vesicles; M. Miquel d'Amboise, with the blood; and Copland saw the affection produced in its most severe form from the contact of a small portion of the discharge from the throat of a patient labouring under the disorder. The existence of rubeola as a disease *sui generis* being yet *sub judice*, we refer for further information regarding it to the article on the "Diseases of Children" in our preceding number (p. 165); appending, however, the following extract from Dr. Parkes's short note in his appendix.

"Gustav. Simon believes, that in this term merely modifications of scarlatina and of measles have been included. In the last case the eruption resembles scarlatina; but the general symptoms may be those of measles. Simon considers this to be measles, and describes it under the name of morbilli confluentes." (p. 430.)

Though we have placed frambœsia in the first class, we must allow that the evidence in favour of its propagation by infection is very slight, whilst that which proves it transmissible by inoculation is amply sufficient. Dr. Thomson states, that it is both contagious and infectious; which assertion seems scarcely reconcilable with his after-statement, viz., that Dr. Bancroft informs us, that none ever receive the disease whose skins are entire, on which account the whites are rarely affected. On referring to a paper on the Yaws in the 'Edin. Med. and Surg. Journal' for 1819, by Dr. James Thomson of Jamaica, we find it stated:

"I have endeavoured by every means to ascertain whether the disease can be communicated by the mother to the child *in utero*, but without obtaining any satisfactory information. Many old negro women have told me, that a child born under these circumstances never has the yaws; but no reliance can be placed on their testimony. Equally suspicious is any information I have on the question whether the air can propagate the disease." (p. 325.)

The writer from whom we have just quoted, produced the disease by inoculation with "matter taken from ulcers where the scab had been removed;" but is of opinion, that the *blood* of a yaws patient cannot transmit the poison-germ. His experience on this point seems to us too limited to allow of a decision. Dr. Thomson states, that—

"It may be communicated by using the same spoon, by kissing, and by coition when the yaws appear on the genitals." (p. 78.)

Unlike frambœsia, there is full proof of varicella being communicable by infection, whilst it is doubtful if it can be given by inoculation.

"The experiments of Mr. Bryce, already referred to, seem to have settled the question. He inoculated children with the contents of the vesicles of varicella in every stage of the disease; and children also who had not had either smallpox or cowpox; but in none of them was either varicella or smallpox produced. Dr. George Gregory also lends his testimony to that of Mr. Bryce on this point." (p. 66.)

The position we have allotted to erysipelas may appear of doubtful propriety to some; but we have full warranty from our author for this view of it; indeed, nowhere does he speak with more emphasis than in his chapter on this disorder:

"The disease is a distinct febrile affection, of which the eruption is merely the external characteristic symptom, as much as the eruptions attending measles, scarlatina, or any of the other exanthemata. . . . Erysipelas may be defined—a febrile contagious disease, in which some part of the skin, and, although rarely, the



mucous membrane, are affected with heat, redness, swelling, and sometimes vesications." (p. 86.)

"Rayer, among many continental physicians, has combated the opinion of its contagious or infectious nature; and contends, that in those cases which have been regarded as resulting from communication with others labouring under the disease, it can be more rationally referred to the persons being exposed to the same influences. But the facts in support of its infectious nature are too numerous to admit of any reasonable doubt upon this point; and instances of erysipelas spreading in the wards of hospitals after the introduction of a solitary case into them, are almost of daily observation. In every hospital in London, whenever it occurs in any ward, it rarely happens that other patients, debilitated by previous disease, escape its attack. Phthisical patients, however, seem less susceptible to the infection than those labouring under other diseases. It has, however, rarely been observed to spread in private houses, although Dr. Wells has collected several examples which seem strongly to support the opinion of its general tendency to extend itself by the intercommunication of the healthy with the diseased. Whatever the nature of the infectious virus may be, it most probably, as in smallpox, propagates itself after it enters the blood. From five to seven days frequently intervene after the infection has been communicated, before the disease shows itself; but it has occasionally appeared on the second day." (p. 94.)

"The distance to which the influence of the infection extends, has not been determined. I have seen a patient confined to his bed, in a ward of University Hospital, at a distance of thirty feet from the person first displaying the disease, take the infection. The disease has been communicated by inoculation, and also by fomites." (p. 95.)

The position of typhus and typhoid fever amongst the febrile zymotics necessarily accompanied with an exanthem, cannot be said to be as yet *undoubtedly* determined; though the recent able investigations of Dr. Parkes's colleague, Dr. Jenner, have done very much in further support of this view of the question, which we hinted at in our account of the Irish Epidemic of 1847 (Vol. I, p. 308). Dr. Thomson remarks:

"There are two varieties of Continued Fever, which are attended with eruptions of the skin, and which have a close affinity to the Exanthemata, in being contagious and infectious, and the result of animal poisons; but the circumstance of their occurring only once during life being doubted by many, although I have no doubt upon the subject, induces me to arrange them rather as supplemental to the exanthemata, than as truly belonging to that class of diseases. There is some reason for supposing that they are the result of dissimilar poisons, for the infection of either always produces a disease similar to that whence it emanated; at least, the observations of those who have ample opportunities of seeing many cases of both varieties have led to this conclusion." (p. 80.)

Dr. Jenner is here particularly referred to by the author.

Few persons, we believe, doubt that the poison-germ of typhus can be given by infection; but there are many who deny that typhoid fever is so to be propagated. *We* should not particularly covet, except in the performance of our duty, to be long exposed to the atmosphere of a large ward of patients affected with typhoid fever.

Cholera is considered by some a "blood disease;" by others, a form of "congestive fever." It may be both. Some deny its communicability by infection; others believe in it, as we do ourselves. The general spread of an epidemic may not be due, in any considerable degree, to such a mode of extension; but that such a mode of extension *does* occur, with respect to cholera, is our firm conviction. (See our First Vol., p. 248.) In

relation to the skin-manifestations of this disease, Dr. Parkes thus writes in his appendix :

"During the stage of reaction in cholera, viz., in the period succeeding the cold stage, and generally in the advanced period of this reactionary stage, a peculiar eruption sometimes appears. An account of it has lately been given by Simon, and copied into the Prague '*Vierteljahrsschrift*.' According to Simon, the eruption may be said to have four stages, viz., the macular, the papular (a stage of exudation), the erythematose (or stage of absorption), and the desquamative. The course is as follows :—first appear little round red patches, which are isolated or confluent, and fade under the finger; they arise suddenly, without premonitory symptoms, and frequently appear during the night; they show themselves first on the backs of the hands; then on the fore-arms, the upper-arms, the neck, breast, back, and abdomen, in the order here given, and finally on the lower extremities. In from twelve to sixteen hours, this stage is completed. After two days, the larger and redder spots pass into wheals, something like urticaria, only there is no itching. After from twenty-four to thirty-six hours, the wheals or projections flatten, and pass into a kind of erythema, or take again the form of spots. In another twenty-four hours the red colour disappears, and the epidermis begins to separate, and continues to separate for three or five days. The exanthem appears, according to Simon, only in persons who have passed through true cholera and cholera typhoid. Treatment, age, sex, and constitution have no influence on this eruption, which Simon considers as arising, like the several stages and inflammations of internal organs, from the changes which have been impressed on the blood by the previous enormous discharge of watery fluid. An elaborate and excellent account of this eruption is given also by Virchow and Leusbuscher. In addition to the appearances noted by Simon, a diffuse general hyperæmia of the skin was observed as preceding the exanthema in certain cases, chiefly in young and athletic persons. The editor witnessed this eruption, in 1849, in two out of thirty-four cases of cholera. . . . . Contrary to Simon's and Virchow's observations, no desquamation, or but very little, followed it." (p. 435.)

Passing to our *second* class of constitutional affections, accompanied with an eruption, &c., we may state, that there exists no evidence for believing that Vaccinia can be propagated in the human race otherwise than by inoculation, by which we know it can be readily effected.

The experiments instituted by Mr. Ceeley, and some facts occurring naturally, have been generally supposed to prove, that Variola and Vaccinia are, at the bottom, identical diseases; and that the poison-germ, taken from a smallpox patient, can, by inoculation in the cow, give rise to vaccinia. Cowpox, in this view of the matter, is therefore only a variety or form of variola; the poison or virus of the latter being modified on its reception and multiplication in the inferior organism. Once so modified, however, its poison-germ preserves its speciality, and does not revert back, *in toto*, to its original character; though it retains a sufficiency of this to be able to free the human system, which has been subjected to its influence, from the *parent poison*—variola,—according to a well-known law, overruling febrile diseases having specific eruptions, viz., that they attack the body but once during life. Alluding to the experiments of Mr. Ceeley, Dr. Thomson writes :

"These cases and experiments are certainly very strong evidence in favour of the identity of the two diseases, and do not require the doubtful support of some experiments performed in India, in 1833, by Messrs. Furnel and Brown, which would tend to induce the belief, that matter, taken from the cow, labouring under what was termed variolous disease, produced, by inoculation, smallpox in man. They refute the opinion of Dr. Jenner, that smallpox originated from vaccine in the cow ;

and tend rather to the belief, that smallpox was the origin of the cowpox, and communicated by man to the cow; and, at the same time, strengthen the opinion of the identity of the two diseases. The strongest opponent of this identity is one whose authority should have considerable weight in such an argument, namely, Dr. George Gregory, who, in his valuable '*Lectures on Eruptive Fevers*,' says, 'I demur to the theory of identity, and hold that smallpox and cowpox are antagonist affections.' " (p. 69.)

The strongest argument in favour of the identity is, we conceive, the fact of vaccinia generally freeing the person from variola, according to the law we before mentioned. The strongest against it, is the fact of the two diseases having proceeded in the system at the same time, each preserving the *speciality* of its own virus. Thus, the following case occurred in the Smallpox Hospital, in October, 1800. A child, who had been exposed to the infection of smallpox, was vaccinated. Both diseases advanced. A lancet, charged with lymph from the vaccine vesicle, produced cowpox. Another lancet, charged with matter from a variolous pustule, formed within the vaccine areola, communicated smallpox. To such a case, however, it might be replied—that, as we are fully aware that two specific poisonous actions can go on in the system at one and the same time,—as variola and scarlatina, rubeola and vaccinia, &c. &c.—why should it not happen that variola may be present, and yet the system be affected by the superaddition, modification, or alteration, which this poison has undergone after having passed through the cow, and by which the human system is liable to be affected, so as to preserve and propagate it, when inoculated with it from the lower animal,—the presence of the variolous poison not freeing the former from such superaddition, when bestowed upon it? In vaccinia, there may exist two necessary elements in the poison-germ;—the original and parent one, the variolous;—a superadded one, the vaccinious. A person inoculated with the virus of vaccinia receives all that is necessary to free him from variola; but does he that is only inoculated with variola become impressed with all that may be required to defend him from the virus of vaccinia? Is a milkmaid, who has had smallpox, necessarily free from liability to vaccinia, though she be frequently engaged with diseased cows? The non-identity of the origin of the two affections, then, does not appear to us to be clearly determined, though there may be important differences in other respects. Few will be unable to see through the absurdity of the conclusions of Leichtenstein, who inoculated several persons with the fluid from the pustules produced by antimonial ointment, and obtained (as he states) vesicles, closely resembling those of vaccinia, from which he reinoculated others, who were found to be safe from the infection of variola, when brought within the sphere of its zymotic influence. The same persons, we would remark, might have been inoculated with anything, or not inoculated at all, and yet not have been affected with variola, though brought within the range of its poisonous action.

On the position we have allotted to Syphilis, it is unnecessary to make any remarks; but we shall refer presently to some interesting points connected with the transmission of the disorder. We may state, however, that, for the chapter on the Syphilides, in the work before us, we are indebted to Dr. Parkes; and an admirable chapter it is, both in its pathological and practical aspects. It contains nearly all the essence of a subject, which, in recent years, has been illustrated by no small amount of literary labour.

Referring to our *third* class, we may affirm, that the scrofulous, gouty, and rheumatic constitutions or diatheses may be hereditarily transmitted, and that such diatheses appear to be in some way connected with particular affections of the skin. In the scrofulous child, the offspring of scrofulous parentage, this connection is witnessed with certain forms of eczema, porrigo, impetigo, and herpes; which are always of more frequent occurrence in such children, and of a more obstinate and relapsing character; and which, moreover, are only satisfactorily treated by directing attention to the constitutional taint, of which they are exponents. That the system can be primitively endowed with a tendency to produce the leproid affections, is generally admitted; as also, we believe, that there is a connection between these and the gouty and rheumatic diatheses. It is the opinion, also, of most pathologists, that they are non-inoculable or non-contagious. This view is taken of them by our author. Bateman, in his edition of Willan, when speaking of psoriasis, remarks:

"It is not contagious, with the exception, perhaps, of the first species [psoriasis guttata], which Willan had observed to occur among children, in the same school or family, at the same time—a circumstance, however, which I never witnessed."

We believe that Mr. Wilson has made the same observation as Willan; and Dr. Copland, though he thinks that this *simulation of contagion* must arise from the constitutional predisposition to the disease existing in some families, combined with the simultaneous operation of the same exciting causes, admits that the subject deserves further investigation. Whilst noticing the leproid forms of cutaneous affections, it may not be out of place to remark, that Dr. Thomson is very decided in his opinion as to the propriety of separating lepra from psoriasis; thus differing from many dermatologists, who view them as mere varieties of the same disorder. Mr. Wilson, no mean authority, seems to be of the latter opinion; since he admits, that if any useful purpose were to be gained by their reunion, he would agree in its favour, seeing that the two affections are so similar in their essential nature. Still, he is of opinion, that the terms expressive of the two diseases are now so well understood, that no error can arise out of their separate existence; and, moreover, that they serve to express certain differences of moment between the two affections. We may also refer to the article, 'Psoriasis and Lepriasis,' in Dr. Copland's Dictionary; in which the two disorders are amalgamated, partly on the authority of Mr. Dendy, who has had considerable experience of the maladies in question. This writer, it will be seen, breaks up the various species of the two genera, and forms one genus out of them, *Lepriasis*, having seven species in it, amongst which are to be found, lichen agrius, pellagra, pityriasis, &c. This, we think, is carrying the point rather too far; though Dr. Thomson, when speaking of lichen agrius and psoriasis localis, admits, that if the case has not been seen from the commencement by the practitioner, it is often extremely difficult to distinguish the one disease from the other, and to form a correct diagnosis (p. 291). In allusion to the *Squamæ*, he remarks:

"The order is supposed to contain three distinct genera, namely, *Psoriasis*, *Pityriasis*, and *Lepra*; but modern writers on this class of diseases have endeavoured to class together the first and the last, and regard psoriasis as a variety of lepra. There is certainly much less reason for this amalgamation, than there would be for many that have not been attempted. Many of the physical appear-

ances of the two diseases differ materially; and if eruptions, not depending on specific fever, must be arranged according to their physical aspect, why depart from the rule in this instance?" (p. 285.)

Although it is not absolutely essential, for the production of Ichthyosis, that there be an hereditary vice leading to it in the system at birth, yet it seems to be the general rule that there must be; for local, accidental, or acquired ichthyosis occurring long after birth is stated to be rare. We once attended a patient dying of phthisis, between 50 and 60 years of age, whose arms gradually presented such an appearance, that we could liken it to nothing but to this disorder, to which no hereditary tendency could be traced. Lupus, we believe, can be transmitted from a parent; and when so, the constitution of the child is not the scrofulous alone, as has been sometimes imagined, but has this diathesis, with an important taint added to it. Pure scrofula, we believe, will not cause lupus; and even where the dyscrasia is produced artificially, if we may so term it, and this disease arises, which it often does, there is another element at work besides the vice of scrofula. At least such is our opinion; and we therefore fully agree with Dr. Parkes (p. 311) in refusing assent to Dr. Burgess's view of the essentially local nature of the disease, and consider with him, that lupus is but the local manifestation of a general disorder, and that local applications can never touch the real seat of the evil. We are more inclined, however, to agree with Dr. Burgess, than with Dr. Parkes, as to the value, nay, almost necessity, of combining such local treatment with the constitutional. Dr. Burgess states the latter to be perfectly useless when used *alone*; Dr. Parkes does not think it to be so. We are of opinion that often, without local treatment, we should not be able to get rid of the disorder, any more than we could cure scrofulous disease of the knee-joint, simply by giving iron or its iodide, &c., quinine or cod-liver oil. The constitutional vice, in both instances, must be attacked, whilst the local manifestation absolutely demands local interference.—According to the late researches of Danielssen and Boeck (see Vol. V, p. 180), hereditary predisposition is the chief cause of elephantiasis, it being not at all contagious, though it may be acquired by residing under unfavorable circumstances, in localities where the disease is endemic. Dr. Parkes says:

"The contagion of the disease has been asserted and denied. The question is still doubtful. The facts show, at any rate, that it is not virulently contagious." (p. 333.) "From 213 cases observed by Danielssen and Boeck, it appears that 189 were derived from families in which one or more persons had suffered from the disease. In 24 cases only did it appear to have arisen spontaneously. The hereditary influence was more marked on the maternal side (a fact observed also by Alibert), and more in a collateral than a direct line." (p. 332.)

We may say, with Dr. Parkes, that—

"The doctrine of a special cause, or of special causes, of venereal diseases, is so generally admitted, that it is not advisable to enter into any controversy on the point. Every syphilitic phenomenon, primary, secondary, or tertiary, may be, with certainty, assumed as the sign of the immediate or remote action of a definite virus. A different opinion has indeed been maintained, even in the present day, by some able writers, among whom Jourdan may be mentioned; but his arguments, ingenious as they are, have shown only the weakness of his cause." (p. 364.)

"Although the action of specific causes in the production of syphilitic eruptions cannot be doubted, it remains uncertain whether these causes have a single mode

of primary manifestation, or several. But whether they have or have not, it seems extremely probable, that the secondary eruptions follow, in the vast majority of cases, one particular manifestation, viz., the chancre. In a small number of instances they may follow another primary symptom, viz., urethral discharge; but it has not yet been accurately proved, that such discharge may not be attributed in these cases to hidden chancres. . . . . It has been asserted, that not only may the well-known primary symptoms—chancre and gonorrhœa—give rise to secondary eruptions, but that such secondary eruptions may themselves be communicated, *i. e.*, may arise in one person from contact or coitus with another suffering from secondary symptoms, without the first person being affected by any primary symptoms. . . . It has also been supposed, that an individual having connection with a person labouring under primary symptoms, may manifest the symptoms usually termed secondary without passing through the stage of primary and local manifestation.” (p. 369.)

Our readers will find, after perusal of Dr. Parkes's admirable exposition of the syphilides, that the above questions are so necessarily encompassed with such extreme difficulties, as to render it next to impossible to give definite judgments upon them at present. Some remarks made by Dr. Parkes, which we shall quote presently, and an interesting paper which appeared in one of last year's volumes of the ‘*Journal für Kinderkrankheiten*,’ suggests to us the propriety of a few observations relative to the infection of a child with the syphilitic poison by the parents.

It may be affirmed, that a woman having a primary syphilitic sore on the genitals, the secretion of which comes into contact with an absorbing surface of the child during parturition, may infect her offspring with the poison. It is very rare, however, for such a mode of inoculation to ensue; indeed, so rare is it, that some have denied that it ever takes place. The writer of the paper we have just alluded to, believes inoculation *intra partum* to belong to the most unfrequent of occurrences. Bosquillon, in his translation of ‘*Bell's Works*,’ avers that it is almost impossible to imagine how such a thing would occur during an ordinary labour. The rapid transit of the child, especially through the external genitals, the presence of the *vernix caseosa*, the previous washing out of the vagina, &c. by the amniotic fluids, the want of exposure of the mucous surfaces (the eyelids, mouth, and genitals being closed), and the soundness of the skin, are all obstacles to inoculation. If such were not the case, how could we explain the fact of so many children being born untainted, though their mothers have not unfrequently primary sores upon the genitals?—sores able to transmit the poison to the attendant on the labour, as we have ourselves known to happen. Still this mode of inoculation *intra partum* has been stated to occur; as also inoculation from the presence of a chancre on the nipple. When syphilis is produced in the child by these methods, the affection is of a primary character, and may run through all the phases of the secondary forms afterwards.

It may be asked, can a mother suffering under secondary symptoms transmit the poison to the child by the medium of her milk? There are considerable difficulties in the way of answering this question directly. First, it may be replied, that a mother who is capable (assuming that the milk can be the medium of conveying the poison) of infecting her child by this means, is also capable of previously infecting it *in utero*; and that, therefore, the cutaneous manifestations appearing during lactation should be regarded rather as proof of such intra-uterine infection having ensued, than

of the child having been born healthy, and having afterwards yielded to the influence of the virus of which the milk was the medium of transmission. It may be said, Secondly, that supposing a healthy child to be born of a syphilitic mother,—capable, as we should assume, *à priori*, of infecting it *in utero*,—there will be found in all probability such a peculiarity of its system, as to render it unsusceptible of the poison which may be contained in her milk, and which might readily affect the healthy system of another child born of non-syphilitic parents. It seems satisfactorily proved, that nurses, whose systems are saturated by the poison of this disorder, can bestow it by the medium of their milk upon the healthy children of non-syphilitic parents which are given them to nurse; although even this mode of communicating it has been denied, it being affirmed, that direct inoculation of the poison must have taken place from the presence of syphilitic sores upon the nurse's nipples, mammæ, &c. We think it not unlikely, however, that a woman in whose system the development of the poison has not advanced to the extent necessary to infect her offspring, and who produces a healthy one, may, in the course of its further development, secrete such milk as may infect the child who is fed upon it. But Thirdly, that a woman suffering under secondary manifestations during pregnancy can infect her offspring, is universally admitted. It must be remembered, however, that during the manifestation of well-developed *lues*, pregnancy is not very likely to occur; or if it does, abortion sooner or later will generally follow. The author of the paper we have before referred to, maintains that no well authenticated case of the former is on record; and that in his own extensive syphilitic practice, he has never met with one; on the contrary, he is inclined to believe, that severe syphilis is apt to render a woman barren, even after all the symptoms of dyscrasia have vanished from her system. There must be very great difficulty in arriving at a judgment as to this latter point; seeing how difficult, next to impossible, it is to say when the dyscrasia has departed from the organism; for we know it may exist for a very lengthened period, without making itself known by any decided cutaneous manifestations. For a woman to be able to transmit the poison to her child *in utero*, and to bring it forth alive, it seems to be requisite, at least as the rule, that she herself shall be inoculated either shortly before or after conception takes place, the primary syphilis passing into secondary during gestation. But this, again, is a circumstance which is considered by some as of unfrequent occurrence; it being believed, that during pregnancy the multiplication of the poison-germ, and its developmental progress, receive a check or arrestation. If the validity of the above objections can be proved, it must be seen, that the likelihood of a syphilitic child being tainted originally by the mother is not very great; and that therefore, since children are constantly born syphilitic, it is the father who is the real delinquent. Apart from all theoretical reasoning upon the subject, we are inclined to believe, that such is truly the case, judging from the histories of those examples, by no means rare, which come before our notice,—such, for example, as the following:

But a few months ago, a very respectable woman brought her child, a month old, to the writer. It had a decided syphilitic eruption over the body. The mother had no secondary symptoms at the time, and maintained that she had never had any, and was quite ignorant as to the cause and nature of the child's disorder. On cross-examination, however,

it appeared, that shortly before and after she believed conception had taken place, sores existed on the genitals; these had rapidly got well, and no importance had been attached to them. The child was under treatment about a month. A fortnight after the child had been dismissed cured, a secondary eruption appeared on the arms and face of the mother. The woman, it is believed, had never had any connection with any one but her husband. In this instance, was the poison-germ impressed upon the embryo, if we may so express it, directly by the father, shortly after he had inoculated his wife, the latter being unable to inoculate the child from a want of sufficient development of the poison in her system, as proved by the secondary eruption not ensuing until ten weeks after delivery? Or was the child affected by the virus in the mother, after its transit through, and a certain amount of development in, her system during the later months of utero-gestation? And could such a woman have tainted a child begotten by a non-syphilitic father, at the same time as that which was the result of connection with a diseased one? We believe that it is impossible to show, that this mother could not have vitiated her child in the *course of pregnancy*, although it is highly improbable that she could have tainted it in the earlier periods; yet we incline to the opinion, that the unfortunate heritage of the offspring was derived from the father at the time of impregnation. Before we can approach this question in the hope of attaining a satisfactory solution of it, the following inquiry must be answered, which we quote from Dr. Parkes.

"Can a father who is or has been infected, and is with or without constitutional symptoms, and who gives his wife no local or general disease, transmit to the offspring, by some wonderful alteration of the semen, a tendency or liability to the production of syphilis; or not merely this, but positively the disease itself? Several cases of this kind are recorded in the older writers, and a few in recent publications, but they do not seem to have been scrutinised with the exactitude which would be advisable. . . . Cazenave has the following case (p. 135). A lady who had never had any syphilitic complaint gave birth to a healthy child. Her husband then contracted syphilis, and was cured. His wife in no way suffered, but four successive children all died at the same age (18 months). Cazenave saw the two last: they had syphilitic roseola, and subsequently ulcerations at the nails, and caries of the nasal bones. The following statement is given by Cazenave (p. 136) on a friend's authority. A young man contracted a gonorrhoea, which he did not treat, and which disappeared without leaving apparent traces. He then married. His first child was born dead; the second, at the age of two months, presented all the symptoms of syphilis. Put out to nurse, the child infected the nurse, who infected her own child, which died. The mother remained perfectly well. As singular a story has been lately quoted in the English journals." (p. 370.)

A writer we have before alluded to, thus speaks:

"In what way a child is rendered syphilitic during intra-uterine life is not yet explained. Experience has shown, that quite a healthy woman, married to a man suffering under syphilitic dyscrasia, and becoming pregnant by him, brings forth syphilitic children, abortively, or alive at the full period, and exhibiting evident manifestations of syphilis immediately at birth or soon afterwards. At the same time the mother may appear perfectly free from the disorder; and if afterwards she have connection with a healthy man, and become pregnant by him, may bear healthy and strong children. We have known several cases undoubtedly proving the above." (*Journ. für Kinderkrankheiten*, vol. xii, p. 97.)

The following case happened to ourselves. The child of a respectable



of the child having been born healthy, and having afterwards yielded to the influence of the virus of which the milk was the medium of transmission. It may be said, Secondly, that supposing a healthy child to be born of a syphilitic mother,—capable, as we should assume, *à priori*, of infecting it *in utero*,—there will be found in all probability such a peculiarity of its system, as to render it unsusceptible of the poison which may be contained in her milk, and which might readily affect the healthy system of another child born of non-syphilitic parents. It seems satisfactorily proved, that nurses, whose systems are saturated by the poison of this disorder, can bestow it by the medium of their milk upon the healthy children of non-syphilitic parents which are given them to nurse; although even this mode of communicating it has been denied, it being affirmed, that direct inoculation of the poison must have taken place from the presence of syphilitic sores upon the nurse's nipples, mammæ, &c. We think it not unlikely, however, that a woman in whose system the development of the poison has not advanced to the extent necessary to infect her offspring, and who produces a healthy one, may, in the course of its further development, secrete such milk as may infect the child who is fed upon it. But Thirdly, that a woman suffering under secondary manifestations during pregnancy can infect her offspring, is universally admitted. It must be remembered, however, that during the manifestation of well-developed *lues*, pregnancy is not very likely to occur; or if it does, abortion sooner or later will generally follow. The author of the paper we have before referred to, maintains that no well authenticated case of the former is on record; and that in his own extensive syphilitic practice, he has never met with one; on the contrary, he is inclined to believe, that severe syphilis is apt to render a woman barren, even after all the symptoms of dyscrasia have vanished from her system. There must be very great difficulty in arriving at a judgment as to this latter point; seeing how difficult, next to impossible, it is to say when the dyscrasia has departed from the organism; for we know it may exist for a very lengthened period, without making itself known by any decided cutaneous manifestations. For a woman to be able to transmit the poison to her child *in utero*, and to bring it forth alive, it seems to be requisite, at least as the rule, that she herself shall be inoculated either shortly before or after conception takes place, the primary syphilis passing into secondary during gestation. But this, again, is a circumstance which is considered by some as of unfrequent occurrence; it is not generally believed, that during pregnancy the multiplication of the poison in its developmental progress, receive a check or arrestation. If any of the above objections can be proved, it must be seen, that the possibility of a syphilitic child being tainted originally by the mother is not so great; and that therefore, since children are commonly tainted, it is the father who is the real delinquent. In reasoning upon the subject, we are inclined to believe, that in the case, judging from the histories of the children, which come before our notice,—such, for example, as that of the child of a few months ago, a very recent case, which was brought to the writer, It had been born a month old, to the writer. It had been born with a very large body. The mother had no signs of syphilis. It was ascertained that she had never been infected. The cause and nature of the



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"Can a father who is or has been infected, and is with or without constitutional symptoms, and who gives his wife no local or general disease, transmit to the offspring, by some wonderful alteration of the semen, a tendency or liability to the production of syphilis; or not merely this, but positively the disease itself? Several cases of this kind are recorded in the older writers, and a few in recent publications, but they do not seem to have been scrutinised with the exactitude which would be advisable. . . . Cazenave has the following case (p. 135). A lady who had never had any syphilitic complaint gave birth to a healthy child. Her husband then contracted syphilis, and was cured. His wife in no way suffered, but four successive children all died at the same age (18 months). Cazenave saw the two last: they had syphilitic roseola, and subsequently ulcerations at the nails, and caries of the nasal bones. The following statement is given by Cazenave (p. 136) on a friend's authority. A young man contracted a gonorrhœa, which he did not treat, and which disappeared without leaving apparent traces. He then married. His first child was born dead; the second, at the age of two months, presented all the symptoms of syphilis. Put out to nurse, the child infected the nurse, who infected her own child, which died. The mother remained perfectly well. As singular a story has been lately quoted in the English journals." (p. 370.)

A writer we have before alluded to, thus speaks:

"In what way a child is rendered syphilitic during intra-uterine life is not yet explained. Experience has shown, that quite a healthy woman, married to a man suffering under syphilitic dyscrasia, and becoming pregnant by him, brings forth syphilitic children, abortively, or alive at the full period, and exhibiting evident manifestations of syphilis immediately at birth or soon afterwards. At the same time the mother may appear perfectly free from the disorder; and if afterwards she have connection with a healthy man, and become pregnant by him, may bear healthy and strong children. We have known several cases undoubtedly proving the above." (*Journ. für Kinderkrankheiten*, vol. xii, p. 97.)

The following case happened to ourselves. The child of a respectable

Scotchwoman was brought to us for an eruption on the skin. This was diagnosed as syphilitic. The mother was quite indignant, and maintained that she had never had anything the matter with her, either primary or secondary, and evinced pretty strong intentions of retorting upon her husband. Sometime afterwards, the father (a compositor) visited us, and stated that, on reading over the prescription-paper of the institution at which his child was a patient, he had noticed that we had written "Syphilis" down as the disease under which his child was suffering; and that he thought, or at least gave us to understand so, that he could not do better than come to one who appeared to be so acute as we were, and explain all about it, as he was fearful that still more untoward results might happen. He stoutly defended his wife, whom he affirmed never to have had anything the matter with her, but confessed that all the evil rested with himself.

As far back as the sixteenth century, Cataneus remarked: "*Videmus plures infantulos lactentes tali morbo infectos, plures nutrices infeciasse.*" Dr. Parkes has the following note:

"The observations of Calderini and Rizzi, at the Hospital at Milan, seem to put beyond all question, not only the possibility, but the frequency with which children give the disease to wet-nurses, and these to their husbands. Of 1050 syphilitic women, 266 gave birth to infected children. The disease most frequently communicated to the nurse was, 'tuberculous syphilide' of the mammæ. In 100 cases, this occurred alone in thirty-four, with angina in nineteen, and with other symptoms in forty-seven cases. Of these 100 women, nineteen infected their husbands, chiefly with tuberculous syphilide of the penis, scrotum, and perineum." (p. 369.)

With regard to the above 266 infected children, we should like to discover how many were begotten of non-syphilitic fathers; we suspect very few. Of course we do not imply that syphilis may not be bestowed by the mother upon a child, the result of impregnation by a healthy father; but we believe, that in by far the majority of cases, it is an endowment direct from the paternal side. And this may proceed either from an impression on the embryo at the period of impregnation, or from the mother being inoculated at the same time, the virus traversing her system and becoming located in the fœtus; it being unlikely that the mother herself, by virtue of a previous inoculation, should have generated the poison by which a healthy conception can be vitiated; for if she has much of this poison in her, which could so vitiate, she will probably not conceive; or, if she conceive, she will usually suffer abortion. In conclusion, we would refer our readers to a paper read by Mr. Acton before the Medico-Chirurgical Society, and reported in the '*Medical Times*,' vol. xii, p. 147.

We have now to pass on to our *fourth* class, including those constitutional derangements, accompanied with affections of the skin, which may owe their origin to the influences of improper or deficient food, &c. Though there is great amount of truth in the motto affixed by M. Lugol to his work, '*Sur les Causes des Maladies Scrofuleuses*,' viz., '*La santé des enfants tire son origine de la santé des parents*,' yet we cannot go to the extreme he proceeds, when he asserts, that scrofula cannot manifest itself in a constitution which has not derived the vice originally from hereditary descent. That it is continually so derived, no one doubts; but we regard it as equally well proved, that an improper or insufficient diet, the living in a dark, close, vitiated atmosphere, with deprivation of exercise, may

induce the development of scrofula, and as a consequence many cutaneous affections, identical with those met with in children who are scrofulous from birth. Without, however, the necessary induction of scrofula, the depressing circumstances we have just alluded to will bring about such a low, depraved, and cachectic condition of the frame, as seems to be especially connected with the appearance of the ecthymatous and rupoid eruptions.—Of Rupia Dr. Thomson remarks :

"It [the condition of habit inducing it] has been supposed, owing to its more frequent appearance in women than in men, to be that lax, leucophlegmatic diathesis, which is termed strumous: but thousands of strumous persons are exposed to the exciting causes of Rupia, without being attacked by the disease. Whatever the predisposing state may be, it seems to be accompanied with delicacy of frame, and a somewhat broken-down constitution. The exciting causes are obvious enough, namely, defect of good nourishing food and clothing; cold, damp, ill-ventilated apartments; indeed, whatever tends to weaken the habit. It will occur as a consequence of the poison of syphilis; but in this case, also, most commonly comes on in debilitated habits. The disease is confined almost to the lower classes, and is not an unfrequent sequence of other diseases, such as smallpox and measles; frequently it accompanies intemperance. I have seen it complicated with hæmorrhages of the subcutaneous tissue, and the mucous membrane; it always, indeed, indicates a reduced condition of habit, and consequently demands peculiar care not to lower the system in the treatment." (p. 281.)

"Chronic Ecthyma is excited by the same causes that develop the acute forms of the disease acting upon a debilitated frame, whether resulting from bad diet, imperfect clothing, and exposure to cold and moisture, the abuse of spirituous liquors, or from some previous disease. The system must, also, be in a highly excitable condition, an opinion verified by the fact, that pregnant women are more affected by the disease than those not in that condition. Mental affections of a depressing kind also render the habit more susceptible of chronic ecthyma." (p. 241.)

That a very severe constitutional derangement often precedes and accompanies the appearance of the hæmorrhagic spots of Purpura is well known; and though in some cases the general symptoms are but slight, yet there is, we think, much reason to believe, that some important though temporary alteration of the blood, and of the walls of the capillaries, is present. The exact nature of the conditions leading to purpura, however, is certainly not very clear. Dr. Parkes thus writes as to its causes :

"Little at present is known on this subject. Numerous statements have been made, but hardly any are based on good evidence. It appears, however, that the disease is not connected with want of fresh vegetables, as is the case in scurvy.

"Nothing certain is known respecting the cause of the hæmorrhage. As yet the analyses of the blood have thrown no light on the matter; if anything they tend to undermine the opinion of Purpura being simply, or only, a blood disease. The state of the vessels has not yet been made out. There must be capillary rupture, as blood particles escape from them; but whether this depends on mere congestion, or on disease of the coats, or on other causes independent of the vessel, is not known." (p. 341.)

Dr. Copland affirms, that it may attack those who live too exclusively on animal food, and deprive themselves of a sufficient quantity of fresh vegetables and fruits; and that it was remarkably prevalent during 1846 and 1847, when the crops of potatoes and other vegetables were generally blighted and scarce. We do not doubt but that occasionally a plethoric and congestive condition, with excess of fibrin in the blood, caused by repletion, &c., is at the base of the malady, which may in some cases be

relieved by bleeding and mercury. (See Watson's Lectures.) But on the other hand we are satisfied, that a dyscrasic condition, produced by deficient nourishment and other analogous depressing circumstances, is more often the cause of the affection. With regard to Scorbutus, it is quite unnecessary to say anything in allusion to the influence which a particular diet, aided by other circumstances, has in producing a severe general affection, of which the skin affection is, comparatively speaking, but a small part.

In relation to Pellagra, an endemic disease of Lombardy and Northern Italy, Cazenave notes the following, as the chief causes which have been assigned for its production. Insolation, the use of indigestible food, the exclusive use of aliments not sufficiently azotised, the habitual use of maize, an endemic and special influence of the soil.

"Some observations lately made by Girin seem to show, that it is not the mere use of maize which is hurtful, but its use under unfavorable conditions." (p. 346.)

For information connected with our present subject, relative to the Elephantiasis Græcorum and Spedalskhed, we refer to our Number for January last.

Of Pompholyx, Dr. Thomson remarks :

"Among the exciting causes may be mentioned bad or deficient food, causing more or less gastric irritation ; intemperance ; general debility, the imprudent use of cold water as beverage immediately after profuse sweating ; sudden suppression of hemorrhoids, or of leucorrhœa, of long standing, and whatever has a tendency to produce a cachectic condition of habit." (p. 174.)

Dr. Thomson thinks that pemphigus infantile, or rupia escharotica, ought to be placed under pompholyx. He states also that he has never seen the disease. The affection which Bateman describes under this title is perfectly familiar to us ; but we have never witnessed the extreme and serious variety of it, described by Dr. Whitley Stokes. Of Bateman's disease we have no hesitation in saying, that a cachectic condition of the system appears to be its usual precursor. The disease described by Stokes is said more frequently to come on whilst the child is in perfect health, attacking the "finest" children in preference, but the children of the poor more frequently than those of the affluent, those who live in damp situations seeming to be more peculiarly subject to it than others. It is difficult to us to believe in the existence of this "perfect health," and fineness of the children of the poor, living in damp localities.

In our *fifth* class, the Skin-manifestations seem to be constantly connected with a general disturbance of the frame, often very transient in its nature. This disturbance is frequently due to a peculiar impression made upon the digestive system by a particular article of diet ; the chylopoietic viscera are deranged, febrile erethism is set up, and an eruption, often attended with most intense irritation, breaks out upon the skin. The amount and duration of these constitutional symptoms vary greatly. Sometimes they last for several days, as often in the case of herpes zona, in which we have observed them rather severe ; at other times, as in urticaria, in less than twenty-four hours all disturbance has vanished. Three times we have been covered from head to foot with the eruption of the latter, and have been in torture from the irritation ; and yet in the time we have mentioned, we have always been relieved. In each case it was owing to an irregularity in diet ; once to lobster, once to carrot soup, and once to the almond paste on a wedding cake. There are certain articles which

some people can never touch without being affected by urticaria; other persons are only affected by them at particular times or seasons. As a rule it will be found, that these articles which produce the affection in different individuals are the same or nearly so. Thus shell-fish, almonds, caviare, and cucumbers, and certain tropical fish, are amongst the most frequent excitants of it. In a few instances, however, the most simple things, like rice milk and white of egg, appear to produce it. It is difficult to believe, with regard to some of these agents, that any special poison is swallowed; whilst in respect to others it seems to us probable that there is:

"M. Breumié asserts that it is not the mussel which proves poisonous, but an almost microscopic species of starfish, which invests the mussel at certain periods, and which develops the morbid symptoms that eating the mussel at these times induces." (p. 154.)

"With regard to some fish producing urticaria, a few of the tropical fish, namely, the yellow billed sprat, barracuda porga, Otaheite eel, and the kingfish, are so constantly causes of severe urticaria at certain seasons of the year, that they have been regarded as truly poisonous at such times, although the nature of the change in them which renders them poisonous is unknown." (p. 155.)

Where articles like sausages, bacon, damson cheese, caviare, salted fish, &c., produce it, it appears to us not unlikely, that a poison is generated during the partial decomposition of the material of which these things consist; such as we know often takes place in them before they are placed upon the table. The symptoms and effects which these last-mentioned agents produce, are sometimes, however, far more severe than those of urticaria; death even not unfrequently following. When this has happened, it has been supposed by one party, that during what has been termed by Dr. Christison a "peculiar" decomposition, held to be different from that form constituting the state of meat called "high," a particular poison is generated, thought by some to be hydrocyanic acid, or sebatic acid, botulinic acid, acrolein, &c.; and this is said to have been obtained in so highly a concentrated state, that by the administration of it to animals, in doses of the fraction of a grain, symptoms have been produced similar to those produced in man, and in so aggravated a form as to cause death. On the other hand, this generation of a special poison has been denied; it being affirmed, that the poisonous effects produced are sufficiently explainable by a reference to Liebig's theory, viz., that a molecule in motion is able to communicate an analogous motion to other molecules, with which it may come into contact. The deleterious article of diet is organic matter in a state of putrefactive decomposition; its particles are in a particular molecular arrangement and action; these particles resist the converting powers of the assimilative functions, the latter not being able to alter their arrangement and action, and they therefore enter the circulating fluids in that state of molecular motion which constitutes decomposition or putrescency; the result being that they act there as a septic poison, and develop a train of symptoms of great severity. Our readers will find this subject more fully developed in a paper read by Mr. Toynbee before the Medico-Chirurgical Society, and reported in the '*Medical Times*,' vol. xiii, p. 455.

"*Roseola aestiva* is usually referred to the increased heat of summer, acting on a sensitive irritable skin; but it is more likely to depend on some derangement of



the gastric and intestinal secretions, or an augmented, hurried secretion of bile, so frequently the result of the sudden setting in of hot weather. It may also arise from sudden alternations of temperature, or the indiscreet use of cold liquids after violent exercise, or exertion of any kind." (p. 162.)

In allusion to Prurigo generalis, Dr. Thomson remarks :

"It may be induced by certain kinds of food, 'especially the use of shell-fish, salted meat, and much stimulant animal food in hot weather, with a free potation of wine, spirits, and fermented liquors, and excess in the use of condiments, pickles, and vinegar' [Bateman], or whatever tends to produce an acrimonious state of the secretions. I have observed it follow the use of pickled salmon, herrings, and mackerel; and this is not a mere pruritus, such as may incidentally occur from various substances deranging the stomach, but the disease runs the usual course of true papular prurigo. Pork frequently eaten, and ham or bacon, also seem to produce the disease." (p. 210.)

Acne simplex is often caused by the same things which give rise to prurigo; but we have formed the opinion, that it is often due to the low and debilitated condition of the frame, accompanying the severe derangement of the assimilative and digestive functions, to which the practice of masturbation gives rise. We have before indicated, sufficiently for our purpose, the other more general causes of the cutaneous affections which may be arranged under our fifth division.

Passing to our *sixth*, we arrive at certain forms of cutaneous disorder, which seem truly to merit the designation of diseases of the skin; as neither for their origin nor for their continuance do they require any previous or accompanying constitutional derangement on the part of those in whom they first occur, or of those who may afterwards become the recipients of the poison, &c. of the varieties which are contagious or inoculable. In many cases it is fairly assumable, that local irritations, such as heat, dirt, and the action of caustic, acrid, or otherwise irritating agents, give rise to these diseases by producing vascular congestion or inflammation of the *dermis* and of its glands and follicles; this inflammation being effusive, suppurative, or depositive, as the case may be. In other cases the skin itself seems to originate the malady, from some morbid action taking place in its vessels and functions in particular spots, for which it is difficult to discover any external cause. And some appear partly to depend upon the contact of a special poison or other agent alone obtainable from a person suffering the same disease. In many instances it is observable, that a debilitated condition of body, uncleanly habits, &c., render the individual more liable to these affections, or favour their continuance. The following extract from Dr. Thomson will sufficiently indicate the causes of some of them. Eczema is, however, particularly alluded to; and of this he remarks that some varieties are due to the effect of irritation from long exposure of the skin to the direct rays of the sun at midsummer.

"Persons who travel on the tops of coaches, sportsmen, harvest labourers, and women whose necks are exposed, and who take long walks in the sun, are frequently affected by it. I have several times seen it occur in young men, who were for the first time engaged in chemical pursuits, and long exposed to the intense heat of a furnace. It is easy to conceive, from the well-known sympathy between the skin and mucous membrane, that the noxious influence of the causes just alluded to is sufficient to derange the latter; and this, in its turn, tends to maintain the abnormal action set up in the former. Many other external irritants, besides the heat of the

sun, excite eczema vulgare in those of irritable habits. It may appear on the hands and fingers from the irritation of sugar, constituting what is termed *grocer's itch*; from the irritation of *lime*, *arsenic*, *tartar emetic*, *the oil of cashew nut*, the application of *croton oil*, *Indian varnish*, blisters, and many stimulating plasters to the skin, such as the *emplast. calefaciens* P.D. Among other external irritants, I have seen eczema produced by the endermic application of the salts of morphia, although, in general they more frequently cause an eruption closely resembling impetigo. The eruption thus produced always extends to, and sometimes commences at, a considerable distance from the parts where the irritants are applied. I have seen the eruption excited by hydrochlorate of morphia applied to a blistered surface between the shoulders, appear first on the arms, then extend to the face, swelling the eyelids, and producing temporary blindness; and ultimately appear on the abdomen and the lower extremities." (p. 259.)

The theory of a particular poison or parasitic animal, as giving rise to a well-known disorder, may be illustrated by the following:

"The virus of scabies has been supposed to originate in 'crowded, close, and uncleanly houses,' [Bateman.] Plenck says, 'Victu acri, salsos, pingui nascitur. Hæc difficiliter quam acquisitæ curatur;' opinions with which the contagious nature of the disease seems at variance. That the disease is contagious there is no doubt; and, in my opinion, it is equally certain that it is the result of a morbid poison; and that the fluid of the vesicles or the pustules, is the agent transmitting the disease from one person to another. This opinion, however, is strongly opposed in the present day [see Wilson]. Among other opinions, Abinzoar, in the 12th century, suggested the idea that the disease originated from an insect which he first described. His opinion was revived by Gabucinus, Ingrassias, Laurence Joubert, and some other writers of the 15th and 16th centuries, all of whom saw and described the insect which they regarded as an *acarus*." (p. 134.)

Dr. Thomson, however, cannot accord with those who attribute the disease to that insect.

"Were scabies of insect origin, we must suppose that the ova of the acari, constantly floating in the atmosphere, would be deposited and find their nidus; and the insects generated from them, their proper food, in numberless individuals; hence we should have daily and numerous instances of spontaneous scabies, which, if it ever occurs, is very rare. I have presumed to say 'if it ever occurs;' for I have seen so many cases of impetigo mistaken for itch in cachectic children, and even in adults left in a greatly weakened condition after fevers and other acute diseases, that I am induced to believe that the idea of spontaneous itch has originated in such a mistake." (p. 136.)

"That the virus is capable of producing all the varieties of the disease, is evident from its occasionally producing each of them in different individuals of the same family at the same time. I have elsewhere stated [Bateman's Synopsis] that I attended a family, in which four of the children had the purulent form of the disease, whilst another, the eldest sister, and the parents, had the ordinary vesicular variety, or, as Willan terms it, *S. papuliformis*." (p. 137.)

Dr. Parkes adds a note in the appendix on the "*Sarcoptes Scabiei* (wrongly called *Acarus*)," relative to the late investigations of Eichstedt and Keyland on its development, &c. The following is also stated:

"Gustav Simon has adopted the opinion of those who consider the *sarcoptes* to be the cause of scabies, and the eruption to be consequent on the scratching. The first opinion is based on the facts, that inoculation of the fluid of the vesicles or pustules by Hebra, Kohler, and Eichstedt, has never produced scabies. Yet Hebra, Bourignon, and others have produced the disease by transferring the *sarcoptes*." (p. 432.)

In the body of the work Dr. Thomson remarks:

"The experiments of M. Albin Gras at the Hospital of St. Louis, in 1834, and even those of Professor Hebra of Vienna, have little weight with me, although these gentlemen procured the acari, placed them upon their skin, and produced the disease. In the five experiments which M. Gras made, although the insects burrowed in his skin and vesicles were formed, yet this is easily accounted for; when we consider that the bodies of the insects must necessarily be covered with the virus, their burrowing in the skin constituted a simple inoculation; and I arrive at the same conclusion respecting M. Hebra's numerous experiments." (p. 137.)

Dr. Parkes's observations (Append., p. 430) on Porrigo, strongly exhibit the very great confusion and discrepancy of opinion which exist among dermatologists, as to the nature and relationship of *P. favosa*, *lupinosa*, and *scutulata*. We shall not attempt here to unravel this subject, or to enter the maze of synonyms by which it is encompassed. Suffice it to say that—

"Dr. Thomson, after full consideration of these diseases, decided not only on retaining the distinction drawn by Willan and Bateman between porrigo favosa and lupinosa, but also concluded that the evidence in favour of the pustular origin of these diseases and of *P. scutulata* was sufficient. This opinion is not however shared by many observers." (p. 430.)

The terms retained by Dr. Thomson to designate the ordinary ringworms of popular phraseology, are Herpes circinatus, the vesicular and non-contagious malady, and Porrigo scutulata, the pustular and contagious affection. It is considered that, the diagnosis between them can only be obscure when *H. circinatus* attacks the scalp; but this obscurity is readily cleared up when the patches are closely examined. The pustular and contagious ringworm—

"Was little known in this country until the commencement of the present century, when some children infected with it came from India; after which it appeared in several boarding-schools, and was afterwards extensively and rapidly propagated by contagion." (p. 122.)

Our readers are fully aware of the modern views relative to the fungoid origin of this and some other cutaneous affections; according to which "the crust consists chiefly of vegetable fungi, mycodermata, accumulated in the greatest abundance at the periphery of the circles;" the disorder being propagated in others by the cells and sporules of these minute vegetable organisms, coming into contact with, or becoming located on, an apparently healthy though sometimes rather a dirty skin. Some have denied the true plant-nature of these assumed vegetable cells, &c., whilst others admitting it, refuse to acknowledge their growth and propagation to be the essential nature or cause of the disease. On this subject our readers may be referred to some lectures by Dr. Willshire in the 'Medical Times,' vol. xix, p. 316. Dr. Parkes alludes to the discovery by Eichstedt of a microscopic plant formed between the cells of the exfoliating epidermis in Pityriasis versicolor—Chloasma. (Append., p. 433.)

From so practical a man as we have described Dr. Thomson to be, and from one so well acquainted with the treatment of this class of diseases,—a treatment very varied, often troublesome, and of long duration,—any experience we can gain is of no slight moment. We shall, therefore, cursorily allude to the therapeutic procedures he advises, commencing at his third chapter on the 'Non-febrile Contagious Diseases.' Our space, however, we regret to say, will prevent us from doing it that justice which this por-

tion of Dr. Thomson's work demands. To be able to fully appreciate the "practice" inculcated, and the detailed and careful manner in which it is laid down and illustrated, the book itself must be perused.

*Non-febrile contagious diseases.*—*Porrigo.*—The treatment in the various forms must assume more or less of a constitutional character, according to the influence which a scrofulous or debilitated habit is presumed to have in their production or continuance. In some cases, it is requisite that great attention be paid to the general treatment; whilst in others, the treatment may be chiefly topical. In *P. favosa* the former requirement is generally most evident; and the use of hydr. c. cretâ, or in chronic cases that of the iodide of arsenic, will be found advantageous as alteratives, whilst the chloride of barium in the decoct. cinchon. or sarsap. will improve the general tone of the habit. The iodide of arsenic may be given in the dose of 1-20th of a grain to children under six years of age. In the chronic form of the disease, much benefit is occasionally seen from an issue in the arm; a practice adopted by Rayer. After removal of the crusts by poultices, a lotion of the carbonate of soda, followed by the application of an ointment of the carbonate of potassa, or of the iodide of lead, are to be recommended; but in long-standing chronic cases, the iodide of sulphur is most efficacious. No benefit has been found to follow the use of the pitch plaster, depilatories, and strong mineral acids; in some instances harm has resulted. *P. lupinosa* may be treated in much the same way; but—

"When the disease is extremely obstinate, and the achores continue to appear in successive crops, I have found nothing more beneficial than touching each pustule, before it perforates the skin, with a hair pencil, dipped in a solution of two drachms of nitrate of silver in half an ounce of nitric acid. This destroys the pustule, and it does not appear again." (p. 120.)

In *P. scutulata* the local treatment becomes most important; and this must vary according to the changes which occur in the progress of the disease. In the inflammatory stage, emollients, with an ointment of the powdered fruit of *cocculus suberosus*, prove useful. After this stage has passed, the object is to destroy the pustules, and with them the mycodermata, for which the solution of the nitrate of silver before mentioned is available. Iodine has been found efficacious, but is less permanent in its effects.

*Scabies.*—Dr. Thomson believes, that if there is any article of the *Materia Medica* that can be truly termed a specific, it is sulphur in the cure of itch; and when strong objections to the use of the ointment exists, the disease may be cured solely by the internal administration of the remedy.

"The question of the best mode of treating scabies may be answered in a few words. No remedy but sulphur can be depended upon; and, when it is properly employed, every day's experience proves that no case can long withstand its curative influence. When the cure is trusted to either its external application, or its internal administration, or to both combined, a warm bath should be taken every second day; and, during its use, the skin thoroughly cleansed with soap. In the treatment of scabies, no attention in general is paid to diet; but my experience warrants me in saying, that the use of stimulant food protracts the cure, and that the patient should be strictly kept upon a milk and farinaceous diet." (p. 140.)

We may observe, however, that many deny this specific (?) virtue to

sulphur; and lately Dr. Bennett has confirmed the assertion made by some Italian physicians, that sulphur ointment cures itch only in virtue of its oleaginous ingredient which asphyxiates the acarus by stopping up its spiracula. (Edin. Monthly Journal, Jan. 1850.)

*Non-contagious Febrile Diseases.*—In *febrile Erythema* the indications to be fulfilled are: 1st, to allay the febrile excitement; 2d, to diminish excitability by increasing the tone. In *E. locale*—

“When there is much irritation, I have seen great relief obtained from what is usually termed the black wash; or still more from a lotion, consisting of three grains of bichloride of mercury, a fluid drachm of dilute hydrocyanic acid, and six fluid ounces of emulsion of bitter almonds. I have occasionally known pencilling the parts with a solution of the nitrate of silver, in the proportion of a drachm to a fluid ounce of distilled water, as serviceable as in erysipelas; and when the erythema occurs from the person lying long upon the projecting points of the body, especially the hips, as in low fever, and in the bedridden, this is of all applications the best and the most likely to prevent subsequent ulceration. The factor of the discharge is readily corrected by bathing the parts with a lotion, composed of a fluid ounce of the solution of the chloride of soda and five fluid ounces of distilled water. . . . . The erythema caused by the sting of any insect is most effectually relieved by bathing the part with solution of the acetate of ammonia, containing an excess of ammonia.” (p. 148.)

*Urticaria febrilis* demands, in the greater number of cases, palliative rather than curative procedures; though in some instances even blood-letting is required, which in plethoric persons, with a sharp resisting pulse, is generally followed by beneficial results. Emetics, purgatives, and saline mixtures, with the topical application of vinegar and water, are chiefly available, great attention being paid at the same time to the diet. In chronic urticaria, the lotion of the bichloride of mercury, &c., in almond emulsion, is very serviceable.—Nothing is so likely to mislead a practitioner, as the opinion that *Pompholyx pyreticus* is an inflammatory disease. The indications are, to diminish plethora, if present, to allay fever, but especially to restore the lost tone of the capillaries and of the general system by alteratives and tonics. Dr. Thomson has had no experience of the effects of the iodide of potassium, stated by some to be a valuable remedy. Willan recommended the warm bath to palliate the general irritation; but our author has found it more frequently aggravate the symptoms, and occasionally bring out bullæ when employed in other skin affections. The best external treatment is puncturing the bullæ, gently pressing out their contents, and then pencilling the parts over with a solution of the nitrate of silver. If an ointment is required, “the best is composed of two drachms of the compound lead plaster liquefied by a gentle heat, and then mixed with six parts of lard and one part of oxide of zinc.” Under no circumstances ought the diet to be stimulant.

*Non-contagious eruptions, not depending on any symptomatic or specific fever, but chiefly connected with, and characteristic of, derangements of the digestive, assimilating, and secreting organs.*—*Lichen.* The treatment of all its forms is chiefly constitutional, topical applications not being generally required, and sometimes doing harm. Gentle aperients, salines, and a carefully regulated diet, prove most serviceable.—In *Prurigo*, we have to correct the deranged condition of the digestive and other organs, and to allay the irritability of the skin. The bowels should be kept loose by sulphur and nitre; and an acid drink, formed of one drachm of nitric or

sulphuric acid to a pint of *eau sucrée*, or a solution of the acidum nitrohydrochloricum may be employed. In many cases, in persons of advanced age, diuretics produce more benefit than any other internal remedies. They may be administered in the decoction of Pareira brava. Attention to diet is of the utmost importance. As external applications, the best are, tepid baths, either of water at 80°—87° Fahr., or of sea water at the same temperature, or alkaline baths. Temporary relief from the irritation is often to be obtained by the use of the lotion of the bichloride of mercury. In *P. podicis*, the itching is generally followed by amelioration under the use of alkaline lotions; the black wash is also useful, as are lotions of diluted acetic acid and the liq. ammon. acet. In *P. pudend. mulieb.*, equal parts of the chloro-sodaic solution of Labarraque and of water may be employed, after the application of leeches and emollient and narcotic lotions to the vulva. Dr. Thomson agrees with Rayer in condemning the use of "gelatinous-sulphurous douches." The treatment of *Impetigo vulgaris* varies according to the period of the disorder; if fever be present, the eruption very general, and the patient plethoric, ten or twelve ounces of blood may be taken; purgatives, salines, and effervescing draughts being made to follow. When fever has abated, sulphur and nitre may be given as alteratives, or antimonial and conium; the liquor potassæ, or the iodide of potassium in bitter infusions, or, in very obstinate cases, the arsenical solution in decoction of elm-bark, &c. The diet must be of milk and the farinacea. The most useful external remedy is the tepid water bath every morning, the lotion of the bichloride of mercury being used after coming out of it. In severe chronic cases, the waters of Harrowgate, &c., constitute the most effectual and permanent remedy. In *I. larvæalis*, the chief attention is to be paid to cleanliness, the regulation of the bowels, and the moderating of the diet. The ung. hyd. nitratis, diluted with one part of castor oil and six parts of cetaceous ointment, may be applied after removal of the crusts by emollients. In chronic cases, Biett's lotion of the sulphuret of potassium and sesquicarbonate of soda may be recommended. In *Ecthyma acutum*, mercurials are believed to be not only unnecessary but injurious. After maturation of the pustules, tonics prove useful. No topical treatment, except the warm bath, is required. In chronic forms of the malady, the syrup of the iodide of iron, with diluted nitric acid in the infusion of gentian, will be found the more preferable tonic. *Herpes* requires chiefly mild purgatives and salines, with occasional anodynes when the pain and tingling are severe. In *H. circinatus*, "the heat and tingling are allayed by spirituous and astringent lotions. It is an old popular custom to apply ink, and there is no doubt of its salutary influence, and that of many other metallic astringents." (p. 254.) *Eczema vulgare*, attended by exhaustion, will be benefited by a course of the mineral acids in the decoctions of yellow cinchona and serpentaria, the bowels being kept soluble, though active purgation is injurious. In other cases, in plethoric individuals, the use of the lancet tends greatly to shorten the attacks. The lotion of the bichloride of mercury may be employed locally. In *E. inveteratum*, whatever may be the exciting cause, the antiphlogistic and soothing plan of treatment at first has been the most successful. When the eruption becomes chronic, and frequent relapses occur, a course of mild mercurial alteratives is essential. The iodide of mercury, with the compound powder

of ipecac. and the decoct. sarsæ, answer all indications. The most decidedly beneficial topical application is cold water, applied by means of lint, the limb being afterwards enveloped in oiled silk. Except in cases where the scalp is the seat of the disease, not much advantage is derived from the use of ointments.

"The development of eczema on the genital organs forms the worst of the local varieties of the disease. The ichorous discharge is copious and extremely acrid, and causes the most distressing itching of the parts, especially when it extends from the perinæum to the vulva. The lead lotion, with diluted hydrocyanic acid, in the proportion of f. 3j to f. 3vj of the lotion, affords the most effectual relief." (p. 269.)

Case 55 is one of "Eczema inveteratum complicated with phlegmasia dolens," occurring in a policeman aged 34. The disease commenced six months before his admission into the hospital :

"This was the most severe case of eczema I had ever treated ; for although I met with one case, in private practice, in which the eruption was so generally diffused over the whole body, that the patient was obliged to be lifted upon a sheet, and immersed in a bath of linimentum aquæ calcis ; yet the eruption did not so frequently return, and it was uncomplicated with any other disease. The most remarkable feature in M——'s case was its complication with phlebitis, the improvement in the eruption whenever the phlebitis appeared, and its augmentation in severity on the decline of the pain and swelling of the limb. Reflecting upon these facts, I was inclined to imagine, that an artificial drain might supply, as it were, the beneficial influence on the eruption which the phlebitis apparently produced ; whilst, at the same time, it might arrest the inflammatory action in the veins. The result proved the accuracy of the reasoning, and to the influence of the issue the perfect recovery may, in some degree, be attributed." (p. 278.)

This patient was under treatment between seven and eight months. The chief indications in the management of *Rupia* are to remove every cause of irritation, and to bring up and maintain the tone of the patient's system.

"The first is fulfilled by the administration of mild aperients, such as small doses of Epsom salts, dissolved in as small a quantity of water as their solution requires, immediately followed by moderate exercise, and, an hour afterwards, by a large cup of warm tea, or any other warm fluid. . . . The best tonic is either the decoction of cinchona or that of sarsaparilla, with the nitro-hydrochloric acid, in doses of fifteen to twenty minims twice a day. In cases where the sufferer has been weakened by too low a diet, I prefer the sarsaparilla, on account of the slow manner in which its tonic influence is produced ; for I have observed, that mere stimulant tonics, such as cinchona or cusparia, are too excitant at first, and tend to exhaust rather than afford tone when the habit is much weakened. . . . The diet should be light but nutritious. . . . The topical treatment consists, after poulticing the affected parts to remove the scabs, in touching the ulcers with the following solution :—[Sol. Argenti Nitr. ʒj, ad ʒj.]" (p. 283.)

The following are Dr. Thomson's recommendations in regard to the treatment of *Psoriasis* :

"The same plan of treatment is applicable to all the modifications of general psoriasis. If the opinion that the eruption is most commonly symptomatic of derangement, either acute or chronic, of the mucous membrane of the stomach, be correct, the indications to be fulfilled are, first, to subdue that state ; secondly, to regulate the diet ; and thirdly, to bring up the tone of the habit." (p. 291.)

"When the disease assumes a chronic character, and has proved obstinate, the arsenical solution may be prescribed in doses not exceeding eight or ten minims at

first, and gradually increased until headache, nausea, or pain at the epigastrium, demonstrate that the medicine should either be left off, or the dose greatly diminished. There seems to be a conservative power in the system labouring under psoriasis, which enables the arsenical preparations to be carried to an extent that I hesitate to mention, but their influence should be closely watched." (p. 293.)

"In none of the varieties of the disease, with the exception of the local varieties, have I seen much advantage derived from topical applications; and in many instances, the least apparently stimulant substances aggravate the itching. . . . . The best topical remedy is the tepid bath." (p. 294.)

The diet should be completely free from stimulant properties, and consist chiefly of milk largely diluted with Seltzer water, and the farinacea. Dr. Thomson knows of no affection so difficult to cure, and so apt to return, if the remedies that have apparently cured it are not continued for a considerable time after the eruption disappears, as *Lepra*. Some practitioners treat the disease altogether with external remedies, but the author has never seen it yield to these means alone. In young and plethoric habits the use of the lancet is essential; and even in old and broken down constitutions, small or frequently repeated venesections aid very greatly the cure. Small doses of the bichloride of mercury, or of the biniodide, or the liq. potassæ in decoction of elm bark or sarsaparilla, appear to have considerable beneficial influence. The warm water bath, followed by the use of the common tar ointment, rubbed up with calomel and lard, are the most useful topical measures that can be employed. In the treatment of this troublesome affection, nothing is more essential than a mild milk and farinaceous diet.

"Patients have come to me who have been admirably managed as far as regarded medicine, but no instructions had been given with regard to diet. On continuing the use of the same medicine, and strictly enforcing the use of the milk, no recurrence of the disease has afterwards taken place." (p. 300.)

In *Pityriasis capitis* the hair should be freed of the scales by means of a soft brush, and the head washed with a solution of ʒij of liq. potassæ in ʒviij of rose water. Nothing is said to be more injurious than the use of the small tooth-comb, so commonly employed in the nursery. In *P. versicolor*, either the above lotion, or one composed of diluted hydrochloric acid in rose-water, may be used advantageously.

"Acne is always under the control of medicine in the young, unless the disease is of long standing. But in adult age, especially when there is hereditary predisposition, the disease sometimes resists every mode of treatment, and continues during life." (p. 406.)

In the slighter forms of the malady, light cooling diet, gentle aperients, and the use of a combination of liquor potassæ, hydrocyanic acid, and some unstimulating tonics, are all that is demanded. In the severer forms, the same measures, with mercurial alteratives at bedtime, should be persisted in for some time, and then superseded by the nitro-muriatic acid or a chalybeate, (Syrup. Ferri c. Iodid.) The iodide of arsenic, or the tincture of cantharides, combined with the alkaline solutions, are sometimes useful in obstinate cases. A combination of the former with Plummer's pill, has been productive of great benefit. The most useful local application has been found to be the lotion of the bichloride of mercury.

"When acne rosacea has attained its utmost height, it may be regarded as incurable, both on account of the change of structure of the parts affected, and the almost impossibility of restraining the habitual intemperance which is its chief



exciting cause. When in an earlier stage, the object is to allay the irritability of the stomach, and improve the hepatic secretion. For this purpose the hydrargyri iodidum in doses of a grain, in combination with half a grain of ipecacuanha, and three grains of extract of conium, administered every night at bedtime, and a draught consisting of thirty minims of liquor potassæ, four minims of hydrocyanic acid, and two ounces of decoction of taraxacum, are admirably adapted. As I have elsewhere stated, I am of opinion, that in acne rosacea the liquor potassæ has never been carried to the extent of the dose which is fitted to allay permanently the irritability of the stomach. I have frequently gradually carried it to sixty or eighty minims, in the bitter almond emulsion; or, in conjunction with the diluted hydrocyanic acid, in the decoction of elm bark, or of taraxacum." (p. 410.)

Mild astringent lotions may be employed. Attention to the diet, in fact to the whole hygiene, should be minutely carried out.

In *Sycosis*, the patient must be freed from the exciting causes of the disease, the antiphlogistic treatment adopted, and cataplasms of well-boiled conium and poppy beaten into a soft pulp, applied to the chin. Afterwards the decoction of cinchona, with tincture of serpentaria, and the liquor potassæ, may be administered. In long-standing cases, an ointment, composed of a scruple of the iodide of sulphur and an ounce of lard, may be used after the hairs have been removed from the inflamed pustules.

Excluding the "febrile diseases non-recurrent, except in exceptional cases," and the "febrile contagious diseases capable of recurrence many times during life," concerning whose treatment we have not thought it necessary to add to the length of our article by further illustrations from the valuable and extended experience of Dr. Thomson, the affections of the skin, whose therapeutics, &c. we have not noticed, have fallen to the duty of Dr. Parkes to describe. In justice to Dr. Parkes, we express our regret in not being able to find room for his concise, yet comprehensive account of the "treatment of the developed syphilides."

We recommend the combined labours of Dr. Thomson and of his nephew most cordially to our readers; feeling as we do, that for really useful and practical purposes, their work on "diseases affecting the skin," will be found one of the most valuable guides upon the subject in the English language. It makes no pretension to a history of these diseases in relation to their minute microscopic and pathologic anatomy; and in respect to these departments we have not canvassed its merits, being assured that in whatever respect it may thus be deficient, when compared with the treatises of Simon, Wilson, and of others, it is amply counterbalanced as to such deficiencies by its complete fulness of detail and copiousness of illustration in all that relates to direct clinical experience. It is truly a work for the practitioner.—Before parting from it, we cannot refrain from recommending to the student the perusal of the valuable lessons which are taught in the memoir of the author. In them he will find that persevering labour, study, uprightness, and moral energy of character, when combined with a fair endowment of natural abilities, can raise a man permanently, though it may often be but slowly, to a position, to which the brilliancy of an eccentric genius, depending for its advancement on its fits and starts, will often fail to attain; or from which, if it does attain it, wanting a well-formed and solid concrete as its base, it often falls even more rapidly than it rose.

## ART. V.

*Statistical Reports on the Health of the Navy, for the Years 1837, 1838, 1839, 1840, 1841, 1842, and 1843. Part I. South American Station; North American and West Indian Station; and Mediterranean Station. Parliamentary Paper.*—Folio, pp. 327. Ordered to be printed, 28th June, 1849.

TEN years ago, the first of this series of Statistical Reports was edited by Dr. Wilson, and comprised the details of the seven years ending with 1836; so that we shall have statistical tables extending over fourteen years, so soon as Part II of this second series is printed; giving condensed information as to the health and efficiency of a large body of naval forces, and spread over a wider extent of ocean, than has ever been known in the world's history. The naval flag of Great Britain floats, literally, from Indes to the Pole; and her ships of war perform every kind of duty. At one time they carry troops, at another specie and valuables; they cruise wherever merchant vessels penetrate, and keep up that superintendence which is indispensably necessary wherever industry and commerce have spread their ramifications across the ocean; maintaining that system of maritime police without which the high seas could not long be safely traversed by unarmed or weakly-manned vessels. They capture the slaver, and enfranchise the slave. They anchor in ports where the safety of British subjects, or of friendly powers, is endangered by political embroilments; or they carefully survey unknown, or imperfectly-known, coasts and harbours; and thus, while one portion of the naval service, as Dr. Bryson (the author of the present series of Reports) observes, is engaged in protecting the myriads of trading vessels that pass to and fro on the ocean, the other is silently, but not the less usefully, engaged in making known the dangers which beset them in their devious tracks across it.

Dr. Bryson has found it necessary to adhere to the tabular forms used by Dr. John Wilson, in drawing up the previous series of Reports: firstly, for the purpose of more accurate comparison of the two series, and secondly, because the returns being all made according to Cullen's nosological arrangement, any deviation from it would have involved an amount of labour which no single individual could attempt to perform. As to the formulæ adopted, it is to be observed, that they are of the simplest character. There is no attempt at classification; all ages and all ranks are comprehended in one table, in which the diseases affecting them are arranged. The simpler tabular forms show the number of cases and the numbers sent to hospital, invalided or deceased; the next form shows the ratio per 1000 of each of these numbers; and a third form exhibits the same ratios, but distinguishes the number invalided or deceased, accordingly as they were invalided or died on board ship, or in hospital. Tables are given for each year and each command, to which are appended general summaries and observations on the statistical results. When the first series of Reports was printed, the anomalies into which the reporter was led by the use of Cullen's nosology were pointed out in one of our predecessors; the lapse of years has not rendered these anomalies less striking; and the reader, to whom Cullen's

arrangement is little more than a literary curiosity, is startled to find diarrhoea arranged with apoplexy, epilepsy, and asthma; catarrh with dysentery; syphilis with scurvy and jaundice, and the like—being carried back, in fact, to the last century. It seems to be eminently desirable that all the statistical returns of the public service should be on one model; and we cannot understand why that of the Registrar General should not be introduced into both the army and navy; for, although necessarily imperfect, it is the clearest and most natural arrangement ever devised. Anyhow, we trust, we shall see no more statistical reports based on the hypothetical, formal, and antiquated nosology of Cullen; and we cannot but express our regret, that so much labour, paper, and print, have been utterly wasted; for many of the statistical details are altogether without value. Nor can we pass without remark another source of error noticed by Dr. Bryson, namely, the practice of wilfully making erroneous entries as to the cause of death, in tenderness for the character of the deceased, or in deference to the feelings of his friends. Thus, persons dying of delirium tremens, have been entered as dying of fever, dyspepsia, or melancholia. "Dyspepsia" is a heading well-known, indeed, to civil medical officers for all cases not understood, or to be concealed; we are not therefore, surprised to find, that in the statistical reports of the navy, the totals noted under that head can only be regarded, as Dr. Bryson candidly states, as "an aggregate of nondescript ailments."

But although the primary materials are so imperfect and untrustworthy, there is much information to be gleaned from the reports, and especially from the lucid and sensible summaries and remarks which Dr. Bryson subjoins to the tables. We shall, therefore, analyse these, and abstract such information as will elucidate general pathology or therapeutics, or in any way interest the medical reader.

*Febrile diseases.*—The eastern coasts of the American continent, the shores of the Canadian lakes, the ports of the West Indian islands, and the coasts of Syria and Asia Minor, are the principal localities in which malarious or miasmatic fevers have attacked the naval forces comprised in these tables. Dr. Bryson informs us, that the topography of the two sides of the American continent northward of the equator, is widely dissimilar throughout their whole extent. On the east, from the upper ridge of the Andes to the sea-shore, there is an immense extent of country of a more diversified character than is to be found in any other part of the world; vast tracts of sloping or tabular land are covered with primeval forests, and intersected with marshes. On the west, the precipitous descent to the shore prevents the formation of extensive marshes, and these are only found on diluvial formations at the foot of the Cordilleras, and on the banks and estuaries of the larger rivers. The terrestrial emanations thence arising excite fevers and agues; but with these exceptions, both sides of the continent south of the equator, are eminently salubrious, and present regions of surpassing beauty. They are not swept by the desolating hurricane, the typhoon, or the tornado; and except the pampero of the Plate, there are few violent commotions of the atmosphere.

The regions north of the equator are less favoured, and the general effect of the climate on the European constitution is highly destructive to health, although varying in degree according to the mode of life, local influence, and length of residence. It would appear, that from the equator to the

tropic of Cancer, the western coast is much more prejudicial to health than is generally supposed; not less so, perhaps, than the eastern coast, and the West Indian Islands; so that if the same number of unacclimated Europeans landed upon it, as on the latter, and were subjected to the same modes of living and employment, and similarly exposed to the ordinary vicissitudes of the weather, it is probable their health would suffer in nearly the same proportion, and from diseases of a similar nature. It is by no means unusual for the yellow fever to occur sporadically on the shores of the Pacific. North of the 35th degree of latitude, the climate is analogous to that of the greater part of Europe. The Society and Sandwich Islands are remarkably favorable to health.

*Epidemic influenza.*—During the last quarter of the year 1841, the ships' companies of the 'Rose,' 'Pearl,' and 'Southampton,' were attacked by catarrh to an unusual extent; in the latter, the disease broke out while the ship was making a passage from Rio to Monte Video, and assumed all the characteristics of influenza. A similar epidemic prevailed about the same time along the coast of Brazil; but as the winds, during the passage, blew strongly from the east, it could not be supposed, Dr. Bryson remarks, that the epidemic cause had been wafted from the sea-shores of America; adding, "it is not, generally speaking, an easy matter to account for these singular eruptions of disease in circumscribed spots on shore, or in ships at sea, otherwise than upon assumed grounds; and it is still less easy to explain why, at one time, they should take on the form of catarrh, at another of diarrhœa, and at a third of simple idiopathic fever, as in the 'Dublin,' on leaving Rio in 1838." As the influenza prevailed on the coast of Brazil at the time the ship sailed from Rio, the obvious conclusion is, that some of the crew became infected with this febrific poison; and the same may be inferred with regard to the cases of fever on board the 'Dublin.' On the 15th March, 1838, that ship left Rio, which was then healthy, for Bahia and England. On the 16th, the day after their departure, the first case of fever occurred; on the 13th there were two others; and on the 22d, when they anchored at Bahia, a fourth. On the 25th they sailed from Bahia, and the number of cases had increased to nine; and from this date to April 12, the attacks were daily more numerous. On the 24th of April the epidemic ceased, having attacked 150 persons in thirty-seven days, not one of whom, however, died. The surgeon could only account for this epidemic by the hypothesis, that it arose from the men sleeping on the damp decks in their night watches; an opinion, as Dr. Bryson observes, which can hardly be admitted. It seems to be more probable that a specific febrific poison was received at Rio into the blood of the party first attacked, and that the disease spread by infection or contagion.

The great *epidemic influenza* of 1837 spread through the ships of the Mediterranean station, while those of the North and South American were exempt. During the latter part of January, it was epidemic at Lisbon, and amongst the merchant shipping. In the early part of February, it appeared in the 'Russell,' 74, then at anchor in the Tagus, the first man attacked having been exposed during the greater part of the day in a boat and on shore: 81 cases were noted. In the early part of March, the 'Rodney,' then on the south coast of Spain, had 280 men attacked; and the suddenness of the attacks was very remarkable, the

seizures during the first two days being at the rate of three men per hour ! In May it reached Malta ; in a few days from the 20th, almost every person on board the 'Medea' was affected. On the 22d, 23d, and 24th, it put 150 on board the 'Bellerophon' on the sick list ; in the 'Caledonia,' 400 were attacked. It broke out in the 'Rapid' on the 21st of May, and declined on the 31st ; the admissions were the most numerous on the 27th, 28th, and 29th. The outbreak of the disease in the 'Volage' seems to fix the longest period of incubation of the poison of influenza at five days. This ship arrived at Malta, from the Bosphorus, on the 19th of May, after a voyage of sixteen days, and sailed again on the 23d. On the 28th two cases occurred, that is to say, five days after leaving the infected port. Two days afterwards, it broke out suddenly in the 'Vanguard,' which had sailed from the same harbour ; and in the course of forty-eight hours, 200 men were affected, or *at the rate of 8 per hour*. In July the ship's company of the 'Sapphire' was attacked at Corfu, the disease being prevalent in the town and garrison. Atmospheric causes were, as usual, assigned for the origin of the disease ; but Dr. Bryson justly observes, that, as the disease spread over the greater part of Europe, the local phenomena observed are of little etiological importance.

The methods of treating influenza, practised by the naval surgeons, appear in remarkable and instructive contrast. In the 'Russell,' 'Medea,' 'Caledonia,' 'Vanguard,' and 'Sapphire,' the treatment was simple,—mild cathartics, antimonials, &c. being ordered, and venesection avoided, except in very urgent cases. On the other hand, of the 150 attacked in the 'Bellerophon,' 102 were bled ; and in many the operation was repeated a second and third time, "with the best effect," as the surgeon of the ship remarks, meaning thereby, probably, that his patients got well ! A much larger experience, in a similar class of men, under exactly similar circumstances, sufficiently proves, that the bleedings were not only altogether unnecessary, but actually injurious, by retarding convalescence, so that they were performed with the *worst* effect. Dr. Bryson observes :

"That venesection, and what is called the depletory system, was carried rather too far in several instances, is to be inferred from the success attending the opposite and palliative plans of treatment ; the immediate results obtained under the latter were at least equally satisfactory. In the more remote there was this remarkable contrast, that those patients who came out of the disease without great physical prostration from the means used, recovered rapidly and satisfactorily ; while those who had been subjected to an opposite plan of treatment, and who had lost much blood, recovered slowly and unsatisfactorily."

We quite agree with Dr. Bryson in the remark, that these results ought to give a "useful lesson" to the advocates of heroic methods of treatment.

*Asiatic cholera* followed the influenza in the Mediterranean squadron, breaking out on the 20th of June, 1837, or one month later than the influenza ; it found some of the crew in a debilitated state. Its commencement and course afforded remarkable indications of its contagious nature ; the vessels of the squadron escaped so long as they remained absent from the infected localities, and, with few exceptions, contracted the disease when they anchored near them ; and this more particularly applies to Malta, around which they continued to hover while the disease was at its highest pitch of malignancy. Dr. Bryson observes :

"This, it would appear, in some instances, arose from a groundless supposition

that there was more danger in keeping on the open sea, from the risk of encountering a choleraic column or stratum of air, than there was in the vicinity of an island, where there was an hospital for the reception of the sick, should the disease make its appearance; but there was no instance of its occurring in any ship so long as she continued on the open sea, or even in the channel of Malta, unless she had previously communicated either with the latter island or Palermo while the disease was prevalent in these places." (p. 207.)

There are two interesting illustrations of the progress of the disease on board the ships of the squadron, which satisfactorily confute some of the mere mythological theories as to the source of the choleraic poison. No seaman appears to have smelt the "cholera taint" like the celebrated old soldier on the top of Salisbury steeple. The 'Rodney' was lying in Malta Harbour, when the disease broke out in the poor-house of the town, situate close to the quarantine harbour. The first case occurred in the poor-house on the 9th of June; on the 12th the 'Rodney' went to sea, and continued cruising about until the 29th; she then returned, and anchored in Bighi Bay for water, which was taken on board with all possible despatch, as the cholera was very prevalent in some vessels in the harbour; and early in the morning of the 1st of July she again left the island. On the following day at 7:30 a.m., the first case occurred, and proved fatal in about ten hours. This would make the period of incubation of the cholera poison, in this particular case at least, to have been not more than 48 hours. In eleven days (to July 23d), 94 individuals were attacked, and the disease continued until the 25th. Altogether she lost ten men by it.

*Diarrhœa* was prevalent in the 'Caledonia' during the end of June. In the beginning of July the ship was off the harbour of Valetta for several days, and then on the Sicilian coast, when (July 7th) the first case occurred, and was fatal. On the same day several men complained of diarrhœa, and others of cholera in a mild form; and this state of things continued for a week, the attacks averaging 8 or 10 daily; but on July 13th and 14th, they were double or treble that number; the last unequivocal case was on the 20th; diarrhœa, however, continued to prevail, although in a milder form. There were 62 cases of cholera, 5 of which were fatal, and "about" 100 of diarrhœa.

The mortality in the 'Russell' was 3 out of 12 cases; in the 'Bellerophon' (in which influenza was treated on the Sangrado system), the deaths were 10 in 18 cases. Whether this formidable mortality was determined by that method of treatment, seems doubtful, and Dr. Bryson evidently attempts to render such inference questionable. He observes, that it is probable that mild cases of cholera were placed under the head of diarrhœa, as the surgeon, when speaking of having used calomel in the treatment with "the best effect" (he bled in influenza with "the best effect" also), states, that "out of nearly 200 bowel complaints allied to cholera, only 10 proved fatal." It appears that this ship lost several of her men from cholera in the 'Hind Cutter.' If we take "several" to represent five, these data give a total of 25 deaths from cholera or choleraic diarrhœa in the 'Bellerophon;' but, in the tabular statement in the Appendix, we find 18 cases of cholera returned, and 14 deaths; and 167 of diarrhœa, and 1 death; in the 'Caledonia' there were 62 cases of cholera, and 5 deaths, and 172 cases of diarrhœa, but no death. The 'Hind Cutter' is not mentioned at

all; but taking these tabulated figures as representing the true mortality in the two ships from cholera and choleraic diarrhoea, we have a very extraordinary difference, even if the ships were of equal force; the difference is, however, more striking, when we observe that the 'Caledonia' has 120 guns, the 'Bellerophon' only 80. If the depleting system of treatment adopted by the surgeon did not cause this difference, what did?

These returns are deficient in an important element of numeratics; inasmuch as, although we are told the number of guns, we are not informed as to the numbers of the crew. Large ships may be very fairly looked upon as floating villages, in which the whole population is in the same state as regards sex, average age, diet, clothing, occupation, and all those circumstances which influence the progress and mortality of an epidemic. The number of population being given, we could have estimated the ratio attacked with cholera, choleraic diarrhoea, or simple diarrhoea, and the proportion of deaths; and as such population may be considered to be in an average state of health, (although, in this particular instance, the influenza immediately precedent to the cholera had probably reduced the health of the crew below *par*.) the data would have afforded an approximative estimate of the *absolute* mortality of cholera. To show the relative mortality, we abstract the following:

Name of Ship.	Force in Guns.	Cholera.		Cases of Diarrhoea.
		Cases.	Deaths.	
Caledonia . . .	120	62	5	172
Princess Charlotte . .	104	6	1	78
Rodney . . .	92	96	10	102
Russell . . .	72	12	3	81
Vanguard . . .	80	5	3	90
Totals . . .	468	181	22	523

Thus it would appear, that the proportion of attacks passing into the malignant stage to those not exceeding the stage of diarrhoea, was about as 1 to 3, and of the deaths in malignant cases about 1 to 8. This is a much lower proportion than occurs in the mixed populations of towns, the latter varying from one half to one fourth. Such a difference can only be attributed to the superior resisting powers of the ships' crews—of the populations of the floating villages.

Dr. Bryson has evidently a leaning to the doctrine, that the cholera is a contagious disease. Yet with perfect candour he quotes a fact which seems at first thought to militate against the doctrine; namely, that although upwards of 70 cases were treated in the Naval Hospital, not one individual resident therein contracted the disease, whether patients, nurses, servants, or medical officers. In a foot-note Dr. Bryson modifies this statement by information recently obtained from the principal medical officer, who informed him, that although none of the hospital residents were attacked by *cholera*, many suffered from diarrhoea, subsequently to the admission of the cholera cases. This is additional evidence, if any were required, as to the identity of the cause of the two forms of disease.

There is an important general fact to be noticed here, with reference to both influenza and cholera, namely, that, so soon as all the crew had been either actually attacked by the disease, or exposed for a certain period to the action of the febrific miasm, the epidemic terminated; that is to say, there were no more cases. This has also been the invariable rule on shore, when the two epidemics have prevailed in towns and villages; but, as the etiological question of a specific poison is complicated on shore with endemical or topographical considerations, collateral inferences could hardly be deduced from the general fact, without involving some degree of doubt as to their accuracy. These considerations are reduced, however, to a minimum, as regards the population of ships of war. The influenza, for example, appeared in the squadron during the period extending from February to July inclusive, or in winter, spring, and summer, distinctly showing, that atmospheric vicissitudes, so commonly and hastily assigned as the causes of influenza, have little connection therewith; while its independence of topographical influences is shown by the fact, that it prevailed equally in the ships of the station during the period stated, from the Tagus to the Ægean Sea. An interesting point in its spread is indicated in the case of the 'Sapphire,' of 28 guns. While cruising to the southward, amongst the islands contiguous to the coast of Greece, in the month of January, fever, apparently of a catarrhal nature, broke out amongst the crew; there were sixty-six cases, all cured on board. In July, the 'Sapphire' was at Corfu, when the influenza was prevalent amongst the troops in garrison and the inhabitants. It affected the ship's company of the 'Sapphire,' but its course was not so rapid as in the other vessels, the whole of the attacks, "upwards of fifty" in number, having occurred between the 10th and 30th of July. When we find that the epidemic ran its course in three or four days, in other ships, attacking the crews at the rate of from three to eight men *per hour*, for forty-eight hours, we cannot but note its exceptional progress in the 'Sapphire,' inasmuch as we find fifty cases occurring in twenty days; and the inference is almost unavoidable, that the previous epidemic of catarrhal fever in January had afforded an immunity, in some degree, at least, to the ship's company of that vessel in July.

*Catarrhal fever* occurs on board ship as an endemic, and seems to have a closer connection with the other forms of fever than is commonly suspected. We have already (*ante*) stated some facts bearing on this point; and we cannot avoid expressing a suspicion, that something similar, but on a smaller scale, takes place when catarrh is said to run through a family. The pathogeny of all febrific diseases is involved in obscurity, because we so seldom consider the possibility of diseases arising *de novo*, when beings, whether men or animals, are placed under a certain combination of agencies. Dr. Bryson leans to the opinion, that catarrhal fever might end in yellow fever. Commenting on the Reports from the North American and West Indian station, for the year 1843, he observes:

"Catarrhal complaints were extremely prevalent throughout the whole squadron; there is, consequently, a considerable increase in the ratio of the order to which they belong. These attacks, on the tropical division of the station, were not unfrequently accompanied with considerable fever; it is not improbable, therefore, that, had the causes from which they originated been in some degree more active or more abundant, or accompanied with other circumstances, whether of a meteoric or of a



telluric nature, adverse to general health, a great majority of these would have become the common endemic, or yellow fever of the country." (p. 126.)

Having so lately discussed the pathology of yellow fever, we will not occupy any space with the details which Dr. Bryson gives. *Diarrhœa* is endemic in the regions of La Plata, both amongst the inhabitants on shore, and the crews of vessels on the river. It has been known to attack a ship's company suddenly, and shortly after anchoring in the river. The disease has been attributed to the internal use of the river water; but a considerable number of the crew of the 'Stag' were affected shortly after their arrival off Buenos Ayres, on May 13th, 1838, notwithstanding that the whole of the water they were then using on board had been brought from Rio de Janeiro. It attacked the crews of the 'Clio,' 'Grecian,' 'Curaçoa,' 'Rose,' and 'Stag' in 1840, while stationary in the river Plate, and was prevalent at the same time amongst the inhabitants of Monte Video. Various opinions were expressed by the medical officers, as to the causes of the endemic. One unhesitatingly attributed it to the river water, which is certainly unfit for use when first taken up, as it contains a large proportion of sedimentary matter; but, after it has thrown down its impurities, it becomes perfectly wholesome. Another medical officer ascribed it to atmospheric changes, but more to the abuse of animal food; a third, to "crude ingesta," and changes of temperature, but more particularly to the river water; and two others, to meteoric influences only. Dr. Bryson remarks, that diarrhœa most unquestionably prevailed at times when it was not possible to attribute it to any of these causes or contingents; while, on the other hand, it has not occurred, although several of the latter were in full operation, and otherwise affecting the general health of the force; as, for instance, during the previous year, when the men were frequently on shore, living freely, and exposing themselves to the vicissitudes of the atmosphere. In the early part of the year, or after the summer solstice, the disease assumed a more severe form, several of the worst cases resembling dysentery in all essential features, except scybalous formations. We should be inclined to inquire into the condition of the sewerage and scavenging of the town of Monte Video, in investigating the cause or causes of the endemic. Fæcal or animal matter in abundance, becoming decomposed by the combined agency of a hot sun and excess of moisture, would sufficiently account for the endemic, especially if the prevailing winds blew from the town to the shipping. Endemic diarrhœa occurs rarely, if ever, independently of fæcal or putrid miasmata. In 1843, some of the cases of the Rio de la Plata diarrhœa were so severe, as to resemble Asiatic cholera; and the prevalence of the endemic in this year also seemed to be uninfluenced by any of its usually assigned causes.

*Dysentery* was prevalent in the Mediterranean squadron under varying circumstances. Early in 1838, the 'Wasp,' of 18 guns, was at anchor in the Bay of Naples, and that disease spread rapidly through the crew. The usual set of causes were assigned for the visit of the epidemic; and, firstly, the Neapolitan water was blamed, and its use discontinued, but the disease was not arrested. Then the Naples greengroceries got the blame, and rice was unavailingly substituted for fresh vegetables. Next, a barrel of rotten potatoes was discovered in the hold, and ejected, with no better result. Then atmospheric changes were arraigned by the doctor, and the whole ship's crew ordered to wear flannel next the skin. They

were also exercised in the cool of the day, morning and evening, and directed to avoid drinking largely of cold water when much heated. The lower deck was, at the same time, fumigated, and kept dry by swinging stoves. The disease, Dr. Bryson remarks, at last appears to have declined rapidly, in consequence of a change of locality, the ship having gone to sea. Here, as in the endemic of the Rio de la Plata, attention might have been directed with advantage to the *local* conditions under which the ship was placed—that is to say, were the crew exposed to fæcal emanations from the town or shore? Or, as it was the year succeeding the epidemical attack of Asiatic cholera, was the disease of a contagious character, and the farewell onset of the more malignant disease? Of 60 cases of dysentery on board the ‘Wasp,’ 2 terminated fatally.

Dysentery, diarrhœa, and *fever of a typhoid type*, were prevalent amongst the ships engaged on the coast of Syria in 1840, assuming the form observed in camps. In two of the cases of fever, which terminated fatally, in the camp at Djouni, the *skin became quite yellow*; and in one, the inguinal and axillary glands were enlarged, so that it was doubtful whether the case was not one of *plague*. These accidental facts are interesting, as indicating a closer connection, and a more common origin, in this large class of epidemical fevers, than is generally allowed. Of the men attacked, 18 per cent. died; but of 30 attacked on board the ‘Bellerophon,’ 7, or 26 per cent., died. With regard to dysentery, Dr. Bryson makes the following observations:

“It has not generally been deemed a difficult matter to account for the prevalence of fever and dysentery amongst seamen or soldiers who have been exposed to the influence of marsh exhalations, to insolation, and other meteoric agencies, while they have been engaged in laborious duties, and subjected to an irregular mode of living; these occurrences, generally, have been viewed in the light of cause and effect, to the exclusion of every other species of inquiry. When, however, one party or section of men, who have been engaged in the same duties, and exposed to the same causes, entirely escape, as was the case with the ship’s company of the ‘Benbow,’ the fallacy of trusting, on all occasions, to this mode of reasoning becomes evident. In this ship, which was actively engaged in the whole of the operations against the Egyptians, there did not occur a single case of dysentery, although the number of cases of diarrhœa was somewhat increased: moreover, the cases of fever were few and unimportant. The only thing in the shape of a reason that can be offered in explanation of this immunity of the crew of the ‘Benbow’ is, that she remained, up to the end of November, less at anchor than some of the before-mentioned vessels. Her marines, however, 120 in number, were disembarked on the coast, with others of the fleet, for about six weeks, [were] exposed to the same vicissitudes of weather, and [were] obliged to make use of the same bad water and unwholesome food; still, although they returned on board with their health greatly deteriorated in consequence of these privations, and from the effects of diarrhœa, none of them were attacked with dysentery: moreover, they were all cured of the former affection by the ordinary means, without the occurrence of any permanent lesion of the intestinal canal.” (p. 221.)

But this reason is rendered null and void by the facts reported with regard to the outbreaks of dysentery on board the ‘Rodney,’ which took place under the following circumstances:

“That vessel [the Rodney] arrived at Malta in October, where a considerable number of her crew were attacked with dysentery, without their having had any communication with the shore; and again, off Alexandria, in the same month, there

was another eruption of the disease. The number of cases altogether amounted to 62. They required early depletion, repeated two or three times in some cases, together with other active measures. From these data, therefore, one of two things must necessarily be assumed, namely, either that the disease was of a violent inflammatory character, or that the treatment was unusually "energetic." These cases occurring on board ship, seem to form a singular contrast with the mere diarrhoeal complaints which attacked the marines of the '*Benbow*' on shore; yet, notwithstanding the great difference in the method of treatment, and in the nosological arrangement, by a reference to the cases, it is not improbable that the diseases were identically the same. As the cases in the '*Rodney*' could not be referred to exposure, privations, over-fatigue, bad water, and marsh miasmata, it became necessary to attempt to trace their origin to some other source; consequently, the opportune prevalence of a high atmospherical temperature, with a sirocco wind, alternating with autumnal chills, was considered to have been mainly instrumental in their production." (p. 221.)

This reference to the "opportune" prevalence of the sirocco, sufficiently indicates Dr. Bryson's dissent from the etiological doctrines of the naval surgeons, and is a gentle satire on the routine system of reasoning adopted in cases of this kind. It would have been both interesting and instructive if Dr. Bryson had given his naval brethren the result of his thoughts on the matter; or, at least, had given some clue to a better method of investigating the etiology of this class of diseases. And we cannot but think, that the reports from the various ships would supply the crude materials of a more extended and philosophic inquiry; for the immunity from dysentery and fever enjoyed by the crew of the '*Benbow*' was shared by the crews of other vessels. While the ship's companies of the '*Powerful*,' '*Hastings*,' '*Ganges*,' '*Carysfort*,' '*Gorgon*,' '*Hydra*,' '*Implacable*,' '*Zebra*,' and '*Vesuvius*,' all suffered more or less from dysentery on the coast of Syria, those of the '*Asia*,' '*Benbow*,' '*Cambridge*,' '*Castor*,' '*Dido*,' '*Hazard*,' '*Magicienne*,' '*Stromboli*,' and '*Thunderer*,' entirely escaped, although they were employed in similar duties, and, generally speaking, exposed to the same causes of disease, whether existing on board or on shore. On referring to the tables in the Appendix, we find that the ships whose names we have italicized, and the crews of which escaped the dysentery, were exposed to the choleraic poison in 1837; while not one of the ships whose crews suffered from it, were so exposed, at least in the Mediterranean station. Were the ship's companies of the '*Benbow*,' and of the '*Cambridge*,' '*Magicienne*,' '*Stromboli*,' and '*Thunderer*,' also already protected by having been exposed to the choleraic poison, either on the Mediterranean station or elsewhere? The data of the reports do not enable us to trace these ships or their crews; but if the affirmative were established, we should still have the '*Rodney*' as an exceptional case, for we have already noticed the outbreak of epidemic cholera in that vessel, in 1837. If, however, the ship had been paid off and recommissioned in the interval, the outbreak of dysentery on board would cease to be exceptional. We have remarked on the epidemic dysentery in the Mediterranean squadron at length, with the hope that naval surgeons may be induced to direct their attention to the prophylactic efficacy of previous attacks, not merely of diseases identical in their nature, but of those different, although allied. An attack of choleraic dysentery or diarrhoea, for example, may not only protect the individual for a limited period from an attack of cholera, but may also render him less susceptible, or even insusceptible, of

the dysenteric poison ; or an attack of true exanthematous typhus, or even of Asiatic cholera, may be prophylactic against an attack of the plague or bubo fever of the Levant, and *vice versâ*. This seems to have been the reason why, in the camp at Djouni, which suffered severely in the autumn of 1840, from diarrhœa, dysentery, and typhus, no case of dysentery occurred from the 1st of January, 1841, to June 18th, when the troops re-embarked, and only 8 cases of diarrhœa. On this point of exemption Dr. Bryson observes, "Whether it was that the men, from having become accustomed to the use of bad water, and the change of diet, were less obnoxious to these complaints, or that there was a change in the local influences affecting health, there are not any means of satisfactorily determining."

The action of malaria is well illustrated by the returns from the 'Monarch' and 'Medea,' which had the duty assigned to them of removing the Zanthian marbles. The 'Monarch' anchored off the mouth of the Zanthus on the evening of the 18th May, and landed 120 officers and men, who immediately ascended the river for about five miles, and in conjunction with a party from the 'Medea,' commenced removing the antiquities to the banks of the river, and on board pontoons. In spite of great labour, and much exposure to the sun, rain, and malaria of the locality, the men continued in perfect health until the 4th or 5th of June, when remittent fever made its appearance. On the 7th the men re-embarked, and in the evening the 'Monarch' sailed for Rhodes. New cases, however, occurred from time to time, until the 24th of June, or seventeen days after their departure, but exclusively amongst those who had been on shore, of whom 84 were attacked, and 9 died. From this statement we gather, that the period of incubation of the malaria was not less than 17 days in any case, and was probably much longer in many. The 'Medea' landed 33 men, and 15 were attacked, no case terminating fatally. The first cases in the 'Monarch' were the worst and the most intractable; those which occurred after the 20th of June were comparatively mild. The course of the fever varied considerably in its intensity in different individuals. In some the stage of invasion was marked by syncope, and although little excitement followed, there was great confusion of intellect, bilious vomiting, and yellowness of the eyes. In some there was continuous fever for 30, 40, or even 60 hours before the remission occurred; in others a constant succession of remissions and exacerbations took place from the beginning, with a gradual aggravation of all the symptoms, to which excruciating pain in the stomach was added, and constant vomiting of all ingesta. The treatment of the cases on board the 'Monarch' was of the heroic character, bleeding being had recourse to; on board the 'Medea' a cure was sought by an early exhibition of an emetic followed by calomel, so as slightly to affect the system, together with simple means calculated to moderate febrile action. Unless there was a marked difference in the hygienic condition of the two ships, in favour of the 'Medea,' the expectant and simpler method of treatment practised on board the latter must be considered as having been the cause of the difference in the mortality and invaliding that resulted from the fever. We have already seen, that 1 in 9 or 10 died on board the 'Monarch,' none on board the 'Medea;' and while, in the following year, 20 were invalided from the 'Monarch,' in consequence of the effects of the primary fever, or its sequel, ague, only 1 was invalided from the 'Medea.'

The nation can hardly be aware that the marbles from the valley of the Zanthus cost so much in the health and lives of its seamen. Of the 153 who landed to secure the antiquities, 104 contracted fever; 9 or 10 of these died, and 21 were invalided. This loss is the more lamentable, because it was unnecessary; for the greater part of the men of the 'Beacon' were employed in the same valley during the winter months, without any deterioration of their health at the time or subsequently. And even if the summer months were those in which an undertaking of this kind could alone be successfully completed, still we think much good might have been done in the way of prophylaxis by a daily administration of quinine.

There are two sources of disease, and causes of death, to which seamen are particularly exposed; namely, *accidents* and *intemperance*. With regard to the latter, we extract the following statistical data, premising, however, that they do not by any means represent the real amount of injury inflicted by intemperance. On the South American station, during the seven years 1837-1843, there were 23 cases of delirium tremens, 1 invalided, 3 deaths; on the North American and West Indian station, 58 cases, 1 invalided, and 17 deaths; on the Mediterranean station, 136 cases of delirium tremens, 5 invalided, and 15 deaths; making a total of 217 cases, and 35 deaths.

With regard to the increased dangers to life which are incidental to the seaman's occupation, Dr. Bryson observes, in his general summary of the Reports on the South American station:

"The number of deaths from drowning is, for two reasons, especially deserving the attention of all who may take an interest in these matters; in the first place, because some who thus perish are generally the most exemplary and willing of the ship's company, who fall overboard sometimes from the body of the ship, but more frequently from aloft, while actively engaged in the discharge of their duty; and in the second, because it is a question whether these deaths are not increased, in consequence of an over-zeal on the part of both officers and men in the performance of certain manœuvres within the least possible space of time. There is, in nearly all operations requiring manual labour, a certain degree of tact or proficiency and celerity attainable, beyond which it is not safe to calculate on any further advance or improvement; and this, perhaps, is nowhere more observable than in the hazardous and not unfrequently embarrassing duties of a seaman, in the performance of which a degree of rapidity may be aimed at, which can only be accomplished at the risk of inefficiency, and, from over-exertion, loss of life. These observations, it is deemed, will not be considered irrelevant, when it is stated, that the mortality from drowning, during these [seven] years, greatly exceeded that from the most fatal disease; it, in fact, amounted to nearly, if it did not exceed, one third the total mortality from all classes of disease." (p. 42.)

On the North American and West Indian station, of 617 deaths, in seven years, 127 were from injuries and accidents; 21 of these were from causes not stated, but probably from drowning, of which the cases actually reported amount to 66. On the Mediterranean station, the ships were engaged in warfare for a certain period, and the proportion of violent deaths is greater, being 260 in a total of 996. Of these 72 were from drowning, 141 from wounds and other injuries, and 54 from causes not known, but probably drowning. The average proportion, for the three stations, of violent deaths, is 25 per cent., or one in four, of the deaths from all causes. During the five years 1838-1842, the violent deaths from poisoning, drowning, injuries, and other external causes, in England and

Wales, averaged 11,737 per annum, and were in the proportion of 1 to 29 of the deaths from all causes. We must, however, take a more definite and nearer point of comparison; the total violent deaths, in the year 1840, among the navy and merchant seamen at home, including the navy half-pay, fishermen, watermen, boat and bargemen, pilots, &c., amounted to 4·133 per 1000; the proportion on the Mediterranean station, for the seven years 1837-1843, was 3·73; on the North American and West Indian station, 4·97; and on the South American, 7·48 per 1000 of the mean force. It is deducible, consequently, from these statements, that of the three stations, the South American service is by far the most dangerous in regard to the casualties to which the seaman is particularly liable, while the Mediterranean is the least dangerous. This comparison is the more striking, when we remember, that the number of casualties on the latter station includes those of active warfare on the coast of Syria, during several months; so that, in fact, the seamen of the Mediterranean squadron are much less exposed to the ordinary dangers of maritime life than our seafaring population at home. The great excess of violent deaths on the South American station may, we think, be fairly attributed to the more dangerous character of the seas traversed by the vessels engaged in the service of that region of the globe, which constitute some of the most tempestuous portions of both the Atlantic and Pacific oceans.

But, although the population of her Majesty's navy may not be exposed in the Mediterranean and West Indian seas to greater dangers than the maritime population at home, still the mortality from violent deaths is much greater amongst seamen than in the average of civil life, and even exceeds that of any other occupation whatever. Thus, in 1840, the violent deaths among miners were at the rate of 3·99 per 1000; amongst engineers, engine-workers, stokers, &c., 3·50; amongst coachmen, post-boys, and all occupations having reference to horses, 2·57; amongst labourers of all kinds, 1·33; and amongst the whole male population, aged 20 or upwards, of England and Wales, 1·08 per 1000. Thus, while at home a maritime life is above 4 times more dangerous than the average, on the West Indian station it is nearly 5 times, and on the South American station nearly 7½ times more dangerous,—a fearful increase of hazard. We cannot but think, therefore, that Dr. Bryson's remarks are well founded, and merit the careful consideration of those to whom the lives of ships' companies are intrusted, especially on the stormy coasts of the Atlantic and Pacific oceans.

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## ART. VI.

*The Principles of Surgery*. By JOHN A. ORR, A.B. F.R.C.S.I., One of the Surgeons of the City of Dublin Hospital.—*Dublin*, 1850. 8vo, pp. 496.

THE 1st of May has long been famous for the agreeable associations it brings with it. In former times the mail-coachmen used to don their new liveries, and to decorate their harness with leaves and flowers;—perhaps the custom is still kept up among the railway guards who have taken their place; but, at any rate, the postmen may still be encountered on the smiling May morn, as gay as butterflies that have just ceased to be chrysalises. Everybody used to have something pleasant to tell upon the 1st of May; but as far as our profession is concerned, we fear the charm is broken. Be it our painful task to tell the reading world, that the 1st of May, 1850, has witnessed the birth of another work on the principles of surgery, which is considerably worse than any similar book that we have for a long time past encountered. Had it appeared exactly one month earlier, that is to say on the 1st of April, instead of the 1st of May, we might have been inclined to treat it a little more tenderly; it would certainly have had a more appropriate birth-day; and regard being had to the character of the performances of that day, it might not then have been considered, as the author fears it now will, “*de trop*.”

At the present day, when there is so much to be learned, and so short a time to learn it in, manuals and compilations are not only useful, but quite necessary to the student. That they are appreciated, the successive volumes, which under the title of Manuals have been given to the profession, afford a sufficient proof: but in proportion as knowledge in this concentrated form is required and made use of, is it of primary importance that such knowledge be correct, and, so far as it goes, complete. There can be no medium in a work of this class; it must be either good, or absolutely bad; and unless it be good, the injury it may inflict upon those who are but just entering the profession, and are most likely to resort to it for information, is extreme. A *superficial Manual* is a great evil, and deserves a severe judgment. The first book that is put into the student's hand is certain to influence him for good or for evil, more than many others which he may subsequently possess; and it is, therefore, very important that its manner and its matter be both first-rate.

With this conviction upon our mind, we shall apply ourselves at no great length to the consideration of this new Epitome from Dublin. In the first place, the Preface tells us, there are two *intentional* omissions in it; to wit, there is no Operative Surgery, and there are no Illustrations. To treat of the one without being *meagre*, would be impossible; and to be *meagre* would be a “*disadvantage rather than an improvement*.” In this there may be some truth; but we still think the principles of surgery without some of the practice, is something like the salt without the egg, in the old story of the Yorkshire boy, who wished for some salt, and then petitioned for the egg, because the salt was no use without it. In point of fact, too, our author himself finds this so; and thus, when speaking of the treatment of inflammation, he very properly gives a description of the mode of performing venesection. We wish he had carried out this prin-

ciple yet further, and appended a more minute description of the mode of applying bandages and of performing those more usual operations of "minor surgery," which the student is continually called on to execute.

Mr. Orr's apology for the omission of all illustrations in his book is as follows :

"I have not introduced any illustrations into this work. This may appear at first sight a considerable drawback to its utility ; but I trust that a little consideration will alter such an impression. In a work on operative surgery, illustrations are, no doubt, highly desirable ; as the direction of an incision, the form and length of a flap, or the arrangement of a bandage, can be more readily represented in this manner, than by verbal description. Many also would be inclined to say, that they might be advantageously employed to depict the changes in form produced by certain fractures and luxations, but to this I cannot assent. They are, I conceive, needless where the changes are considerable ; and in cases where they are slight, I believe that the young practitioner will make a more correct diagnosis by the assistance of an accurate verbal description of the alteration in the relative position of the structures, (keeping, at the same time, the anatomy of the part in his mind,) than by comparing the case before him, irrespective of the age of the patient, his muscular or adipose development, the swelling which follows the injury, &c., with a single figure ; which, however accurately it may represent the case from which it is taken, can scarcely be considered indiscriminately applicable. Still less can I approve of plates copied from book to book, by which the well-being of the patient and the reputation of the surgeon are made dependent on the fidelity of the artist." (Preface, pp. v-vi.)

Satisfactory as these reasons may be to the author himself, they will scarcely be equally so to students, who are wont to learn nearly as much from what they see as from what they read, and who would probably have been thankful for a few good plates, even if they had been copied from works of authority. In the last objection, the author can scarcely be serious, because no one is compelled to insert a drawing which is inaccurate, or does not convey an accurate idea of the thing signified ; and as for copying drawings which already exist, that can be done with as much ease and certainty, as the same book can be printed in different types.

The omission of any table of contents, except such as is embodied in the preface, is another "want," which has also, we presume, been intentionally created.

The "first Part" of the book is devoted to the Inflammatory Process and its results. It occupies exactly 80 pages ; within which the author disposes of the whole theory of inflammation and its treatment, abscess, ulceration, mortification, erysipelas, irritation, congestion, &c. &c. The length at which it treats of these various subjects, and of those topics which arise out of them, is of course not very great. Take for an example of this, and of the truthfulness of its statements, these fifteen lines upon the question of amputation in gangrene.

"There are three periods at which the surgeon may be called on to operate, with reference to the occurrence of mortification. First : in cases of severe injury, when a limb has been crushed, or when the principal vessels and nerves have been divided, we do not wait for the appearance of gangrene, which is inevitable, but operate as soon as the patient has recovered from the shock of the injury. Secondly : mortification having set in, being the result of any external agent, we do not wait for the line of demarcation to form, but operate at such a distance above the dead parts as the case seems to require. Thirdly : when the exciting cause is an internal



agent, we invariably wait for the line of demarcation to form; knowing that the stump will be inevitably attacked by gangrene, if the operation is performed before; nay, more, we often allow the separation of the soft parts to be completed by the process of nature, lest the irritation of the operation should cause the mortification to spread." (pp. 64-5.)

It may, perhaps, be well to state, that the same subjects occupy in 'Miller's Principles of Surgery,' a book of the same size, and averaging nearly the same number of words in a page, about 235 pages. To avoid all suspicion of unfairness in this comparison, we may observe, that eight of these pages in Mr. Miller's work are devoted to scrofula, which is not considered till later by Mr. Orr; but as the latter includes erysipelas, hospital sore, and diffuse inflammation of the cellular tissue, which the former does not, any advantage arising in this way is more than compensated for.—Surely the work of condensation could no further go.

As we have just referred to Miller's 'Principles of Surgery,' this would seem to be the appropriate place for noticing a circumstance that has painfully struck us in our perusal of Mr. Orr's work. It is quite evident to our mind, that Mr. Orr has been writing the whole of the chapter on Inflammation with Mr. Miller's book lying open before him; and that he has made a much freer use of it than he was justified in doing, especially considering that the form, title, and character of his book place it in some degree of rivalry with Mr. Miller's. This chapter may indeed be fitly described as an epitome of Miller, badly executed. Not only is the arrangement nearly identical,—this, in the treatment of such a subject, might be excused, if it were all;—but the sentiments, the examples, the expressions, and, in some instances, the very form of words throughout a whole sentence, are copied from Miller; and all is done without the slightest acknowledgment, and without the least indication, in any part of the volume, that Mr. Orr had ever even heard of Miller's 'Principles of Surgery.' When, indeed, he has altered Miller's arrangement, or substituted words of his own to express the same meaning, he has generally only interfered to spoil; and has not at all succeeded in his apparent object of disguising the source whence he derived his information.

If Mr. Orr possesses any sense of shame, we think that even he must have confessed a blush upon his cheek, when he remembered, that the very passages cited by one of the weekly journals, as worthy of commendation, were among those taken without acknowledgment (to use the mildest phrase possible) from Mr. Miller.

In proof of the correctness of our opinion, that Mr. Orr's library contains a well-thumbed copy of Miller, of which large use has been made in the treatise before us, we shall place in juxta-position one or two passages from each author, leaving our readers to form their own conclusions as to their identity; and we shall also point out a few examples, on the same system, not less reprehensible, though less apparent. To do this, it does not require that we should dip far into the book. Let us contrast the very first passage in each work:

MR. ORR.

"Inflammation may be defined to be an increased and altered condition of the blood and blood-vessels of a part, by which its functions are impaired, its

MR. MILLER.

"*Inflammation*, the source of much evil, medical as well as surgical, may be defined: A perverted condition of the blood and blood-vessels of a part, inter-

secretions perverted or suspended, and its normal structure altered. Its ordinary symptoms are *heat, redness, pain, and swelling*; and it produces more or less constitutional disturbance.

"A correct knowledge of the process will be much facilitated by considering inflammation as a condition not arrived at at once, the first steps towards it being only an exaltation of the healthy action of the part. It is impossible always to draw the line where health ends and disease begins; but the principal stages in the transition are sufficiently well-marked to allow of their separate consideration. These may accordingly be divided into the periods of *incubation, vascular excitement, active congestion, and inflammation*." (p. 1.)

In this definition, what Mr. Orr has altered, he has only altered to make it nonsense. What is meant by "an *increased condition* of the blood and blood-vessels of a part?"

Next we show our readers a little of that juggling which makes the detection of the imposture only the more difficult, not the less complete. In this example, the arrangement has been altered, as follows:

MR. ORR (pp. 4-10).—Section II. Phenomena of the Inflammatory Process (including *local and constitutional symptoms*).—Section III. Extension of the Inflammatory process.—Section IV. Varieties of the Inflammatory Process.

MR. MILLER (pp. 50-70, 105-108).—Local Symptoms of the Inflammatory process—Extension of the Inflammatory Process—Constitutional Symptoms—(and, passing over thirty-five pages, we come to) Varieties of the Inflammatory Process.

Let any one carefully read through these sections, and then say what Mr. Orr has done, but abridge and adapt Mr. Miller's matter.

Still following Mr. Orr, section after section, we come to Section v, on the Results of the Inflammatory Process. Here, again, we trace a much greater similarity in the sequence of the ideas, and in the diction employed to convey them, than can be reasonably accounted for by the identity of the topic discussed in the two works; the following passages are, to our minds, conclusive. It must be remembered that Mr. Orr is compressing and abridging throughout:

MR. ORR.

"It [the serous effusion] may take place on the surface of a part; and if this be a free surface, like mucous membrane, it flows harmlessly away, like the healthy secretions; if it be a closed sac, like the serous membranes, it gives rise to acute or chronic dropsy, according to the degree of the inflammation.

MR. MILLER.

"The serous effusion may be from the *surface* of the part; whence it flows harmlessly away, as does the ordinary secretion in health,—as in the case of inflaming mucous membrane. Or it accumulates within an internal cavity, as those of the serous membranes, then constituting *acute dropsy* of the part; the bulk, uneasiness, and disturbance to healthy function, by pressure, varying according to the extent and rapidity of effusion.

rupting its healthful function, and changing its normal structure; ordinarily attended with redness, pain, heat, and swelling; and inducing more or less disturbance of the general system. . . . . From health to true inflammation is not one step, at once attained, but a transition, gradually effected,—the time occupied varying according to circumstances. In some cases, a very few hours suffice; in others, days shall have elapsed, and yet the process is incomplete. The transition may be conveniently subdivided into three stages:—1, Simple vascular excitement; 2, Active congestion; 3, True inflammation." (Miller's 'Principles of Surgery,' 1st ed., 1844, pp. 1-2.)

"Acute effusion of serum, whether it forms œdema or dropsy, is usually speedily removed, on the decline of the inflammatory action, by the increased activity of the absorbents; the contrary is the case when the action has been chronic." (p. 11.)

"Acute effusion of serum, whether in the form of œdema or dropsy, usually disappears soon after decline of the action which produced it, by the resumed and increased play of the absorbents. Herein, again, practically most different from the result of chronic congestion." (Op. cit., pp. 79-80.)

The same likeness exists between Mr. Orr's paragraph on *Fibrinous effusion*, in continuation of the last quotation, and Mr. Miller's *Effusion of Plastic Fibrin*; only that the order of the sentences is changed by way of disguise.

MR. ORR.

(2.) "The fibrin may be effused either by itself or mixed with serum; and according to the proportion which it bears to the serum, forms an homogeneous mass, or separates from it in the form of flakes. In either case it is termed coagulable lymph.

(1.) "Fibrinous effusion is the result of a higher degree of action than the last; being, as before explained, characteristic of the second stage of the inflammatory process." (p. 11.)

Passing over several other pages, all open to the same charge of appropriation, we arrive at one of those passages, which, as we have already remarked, ought to make Mr. Orr blush for the approbation it has gained from one of his critics.

Look on this picture, and on that:

MR. ORR.

"The steps of the ulcerative process may be briefly described as follows: true inflammation with suppuration; softening of the part; its reduction to a fluid form; actual death of the part in minute portions, which are mixed with and carried away by the pus." (p. 18.)

MR. MILLER.

"The steps of the process (*ulceration*) are—1. True inflammation, with suppuration. 2. Softening of the truly inflamed parts. 3. Its reduction towards a fluid form—a vital act—more or less complete. 4. Disintegration—or death and detachment—in minute portions, or molecules. 5. Mixture with the pus, and removal in one common discharge." (Op. cit., p. 94.)

If further proof still is wanting, we may cite the following passages from the commencement of Mr. Orr's seventh section, entitled "*Management of the Inflammatory Process*," and from Mr. Miller's section, having the same rather uncommon heading.

MR. ORR.

"It is of the greatest importance to take advantage of the interval generally afforded by the period of incubation. If that is properly employed, it will usually check the progress, if it does not prevent the accession of the approaching inflammation." (p. 23.)

MR. MILLER.

"*Prevention*. Therapeutic means, applied immediately after removal of the exciting cause, may have the effect of entirely frustrating its ordinary operation, and preventing perverted vascular action. For this purpose the period of incubation must be diligently improved." (p. 108.)

Another passage for which Mr. Orr has received critical commendation, is that enunciating a useful precept as to the mode of substituting warm for cold applications in the treatment of inflammation. The following citations will show to whom the merit is really due, and will give to our readers another opportunity of judging how far the coincidence in ideas and words is accidental :

MR. ORR.

"When abandoning the use of cold after our object has been gained, it is well to discontinue it gradually, first substituting for it a cool, then a tepid, and finally, warm irrigation or applications, otherwise inflammatory reaction may be set up in the part." (p. 24.)

MR. MILLER.

"Another precaution is necessary. Let not the cessation be abrupt, but gradual; from cold to cool, from tepid to warm, from warm to hot; otherwise the second or reactive effect of cold, intensely favorable to vascular action, is inevitably produced." (p. 137.)

Passing on now to the next great division of Mr. Orr's work—*Injuries*—we shall cite, as a sample of the information it contains, the following ten and a half lines, which inform the "*young practitioner*" of all that is necessary to be known about fractures into the knee-joint :

"*Fractures into the knee joint* are dangerous; *first*, because they are usually caused by great direct violence; and *secondly*, because they are almost certainly followed by severe inflammation of a large synovial surface. There is usually a more or less transverse fracture below the joint, from which a longitudinal one extends into its cavity; or the bone may be comminuted so as to open the joint in several places. Mr. Adams has detailed a case, in which, from a violent sprain, the anterior crucial ligament tore off the portion of the head of the tibia into which it was inserted. The constitutional disturbance which followed, was so severe, as to carry off the patient. Amputation will often be necessary in these fractures." (p. 195.)

This facility of condensation is not, however, always conducive to the clearness and correctness of the information sought to be conveyed. Turn we to page 203—*Luxations of the Shoulder-Joint*. Here are Mr. Vincent's remarks upon the reduction of this class of luxations, so felicitously condensed, that the very point essential to be known is left out altogether: "In some cases, Mr. Vincent has easily effected the reduction by bringing the arm across the chest after extension has been made." Now the chief point in the success which attends this proceeding, is the *suddenness* with which the arm is brought across the chest, when the head of the bone has been brought down to the edge of the glenoid cavity, and the muscles, all just upon the stretch, are allowed suddenly to contract, and thus pull the bone into its place again. Everything here depends upon a little circumstance which has entirely escaped Mr. Orr.

Part III speaks of Surgical Diseases. Here we meet with the Venereal Disease, treated of in a really very good section, by far the best in the book, and from which a good deal may be learned.

Under the head *Bubo* we find the following table, the composition of which shows a considerable power of analysis, and so much observation, as to make us regret the more that Mr. Orr should have contented himself with staking his reputation on such a work as the one under notice :

*A Parallel between the Characters of different Species of Bubo.*

	SYPHILITIC.	CONSTITUTIONAL.	SCROFULOUS.	GONORRHOEAL.	FROM MERCURIAL FRICTIONS.
GLANDS.	Superficial.	Deep.	Both deep and superficial.	Superficial.	Superficial.
NO. OF GLANDS.	Solitary, usually on side of chancre.	Generally several.	Several, often in both groins, and elsewhere.	One or two glands.	One or two glands on the side rubbed.
SHAPE OF BUBO.	Circumscribed and prominent.	Broad base, and flat.	Round, soon fixed.	Circumscribed and moveable.	Circumscribed.
PROGRESS.	Rapid.	Slow, sometimes stationary.	Very slow, but softens gradually.	Subsides soon by rest, &c.	Subsides readily.
SENSATION.	Very painful.	Little pain.	Little pain.	Tender to touch.	Tender to touch.
SIZE.	Size of a small orange.	Large.	Large, often fills the groin.	Small.	Small.
COURSE.	Almost always suppurates.	Sometimes no suppuration; usually several points.	Suppurates in several places.	Never suppurates, if attended to.	Not prone to suppuration.
NATURE OF TUMOUR.	Matter forms its great bulk.	Little matter in proportion to tumour.	Matter small in proportion.	No pus.	Little pus.
COLOR.	Skin red, and largely thinned before bursting.	Skin purplish at more points than one.	Skin dusky red, traversed by dilated veins.	No redness.	Seldom red.
PUS.	Thick, yellow, often mixed with blood.	Thin and white.	Flaky and curdy.	—	Pus laudable.
INCISION.	Tumour much diminished by opening.	Tumour little diminished by opening.	Tumour little diminished by opening.	—	Not so much diminished by opening as syphilitic bubo.
FEVER.	Follows local disease.	Feverishness precedes the bubo; headache, lassitude, night-sweats.	No fever, health weak.	Rather smart fever.	Little fever.

With a few other exceptions like this, the same characteristic of *thorough superficiality* runs through the work from beginning to end; and as we have a shrewd suspicion that Mr. Orr is capable of much better things, we shall conclude by expressing the hope, that he will profit by the hints we have given him, and speedily redeem his character by producing something more worthy the pen of an observant surgeon.

We have thus endeavoured to do substantial justice to Mr. Orr. While obliged to censure, we have been desirous to find opportunity for praise; and can only regret, that the occasions for the former procedure have been so much more numerous, than those which have presented themselves for the latter, which would have been far more agreeable to our feelings as well as to Mr. Orr's. With regard to the appropriation-system, on which Mr. Orr appears to us to have proceeded to a considerable extent, we must remark, that whilst the author of *any* systematic treatise must profit, to a considerable extent, by the labours of others, and must be guided, in great degree, by that current of opinion which cannot be said to be the exclusive right of any individual, still the writer of a "Manual," whose aim should be to embody what is fixed and definite, may be reasonably expected to have made such knowledge his own, and to be able to give it forth in his own language, and after his own method. And if he cannot trust himself to this, a work of similar character to his own seems to us the last from which he should draw his materials.

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#### ART. VII.

1. *Rapport de la Commission créée par S. M. le Roi de Sardaigne pour Etudier le Crétinisme.*—Turin, 1848. 4to, pp. 220.

*Report of the Commission appointed by the King of Sardinia for the study of Cretinism.*

2. *Du Crétinisme, de son Histoire et de son Traitement.*—Genève, 1850. 8vo, pp. 35.

*On Cretinism, its History, and its Treatment.*

3. *Second Report on Idiocy, presented to the State Legislature of Massachusetts.* By S. G. HOWE, M.D.—Boston (N. E.), 1850. 8vo, pp. 72.

THE first of these publications is a valuable contribution to our knowledge of Cretinism, and to some extent of idiocy generally, based upon a large number of facts, observed over a wide district by persons entertaining great differences of opinion, and submitted to others well qualified to estimate their value and develop their bearings. It is only by analogous investigations, that legislative or other measures of preventive character can be instituted for the removal of great social evils; for although very much may be done in drawing public attention to misery in its lowest and most degraded forms, by the enthusiasm and philanthropy of a Howard, a Pinel, a Conolly, a Voisin, or a Guggenbühl, yet will the results be transient and incommensurate, unless the remedies devised have been based upon statistical data largely accumulated and closely scrutinised. The institution of Committees and Commissions to this end is a most

valuable feature of modern times ; and had the writers of the public press, in place of ridiculing the unwieldiness of the resulting "blue books," occupied themselves in mastering and diffusing a knowledge of their important contents, we should not be, at this period of time, only just entering upon the rational mode of managing the insane,—not having arrived even at that point in respect to the "dangerous classes." How profitless even the most sensible, but mere individual suggestions often are, is shown in the case before us. Dr. Reeve, of Norwich, visited the cretin districts of the Valais in 1805, and published the result of his observations in a very short but excellent paper, in the Fifth vol. of the *Ed. Med. and Surg. Journal*, which then enjoyed a large circulation, both at home and abroad. In this paper, the conclusions of the present report, which we are about to lay before our readers, may be said to be completely epitomised ; and the same distinction between essential and non-essential causes, is as strongly insisted upon in the one as the other. We are not aware that Dr. Reeve's views were ever sought to be utilised ; and the present Report, though professing to give an account of all that has been written on the subject, does not even mention his name.

This Commission was instituted by the late unfortunate King Charles Albert, at the suggestion, we believe, of an enlightened prelate, M. Billiet, Archbishop of Chambéry, himself a man of science, and warmly interested in the welfare of the poor cretins, who abound in his diocese. It consisted of nine resident, and nine corresponding members. The former included five medical professors, Dr. Cantù professor of medical chemistry, and professors of geology, mineralogy, and zoology. The corresponding members were doctors of medicine, and ecclesiastics, in the cretin districts. From the commission, schedules of queries were issued to every medical, ecclesiastical, and governmental authority throughout the continental portion of the Sardinian dominions ; answers to which, as well as specimens of the water from every cretin district, were required to be returned by a certain date, so as to exhibit the amount of cretinism in the kingdom at the beginning of 1846. The returns so procured were digested by an active and able physician, Dr. Trombotto, who was also despatched by the Commission to visit the respective localities, for the purpose of verifying the statements received. All persons co-operated zealously in the task, and the report was published in 1849 (though dated 1848), in both the Italian and French languages, the latter being spoken in most of the Sardinian provinces where cretinism prevails endemically.

To render the account more complete, the Commission has consulted the various works which have been written upon the disease, so that the report takes the form of a condensed treatise upon the subject. We may briefly notice the various divisions into which it has been distributed.

*Etymology and History of Cretinism.*—Among the great number of etymologies that have been proposed, the commission thinks those suggested by Fodéré and Iphofen as among the most probable ; viz., *chretien*, from the cretin being incapable of faults, and therefore a Christian *par excellence* ; or the *cretira* of the Grison dialect, which signifies a poor or stupid creature. In the different localities wherein they abound, a great variety of appellations are bestowed upon them ; and as many of these imply contempt (as *cagot*, *garachos*, &c.), Dr. Millingen draws an argument thence in favour of the view popularly entertained, that they are the

remnants of persecuted races of people, holding very much the position of the Indian pariah.

The first regular mention of these poor objects occurs in the sixteenth century, when, on ecclesiastical registrations of births and deaths being instituted in Aosta, they are mentioned as innocents or sanctified persons (*beats*). Two Swiss physicians, Plater (1500) and Simler (1574), first described cretinism as it prevailed in the Alps; but although various travellers, as Coxe and Saussure, contributed important particulars concerning them, the first complete monograph upon the subject was published by Fodéré at Turin, in 1792, and again at Paris in 1802. This work excited great attention; and since its appearance, a large number of treatises have appeared, among which, as most deserving of notice, may be mentioned those by Michaelis, Hacquet, Autenrieth, Wenzel, Wunderlich, Ackerman, Iphofen, Zschokke, Troxler, Maffei, Gugger, and Rösch. More recently the subject has been discussed at the meetings of the Swiss and German naturalists, and commissions have been appointed by these for its investigation. In 1841 the active humanity of Guggenbühl led to the formation of his establishment on the Abendberg.

*Description of Cretinism.*—This is given, by the Commission, with a minuteness that renders it impossible for us to follow it in the space we have at command; and, indeed, from the accounts which have now been so often given in books of travels of the general appearance of cretins, it can only be necessary to indicate some of the particulars stated. It is a very general belief in the cretin districts, that the infant exhibits appearances *at birth* which indicate its future fate. These statements, however, are very vague and contradictory; and certainly the signs are never pathognomic, save when they exist in such number or intensity as really to constitute the first stage of the disease. After *five or six months*, however, the ill-developed frame, large head, with wide fontanelles, the absence of all mental or bodily vivacity, the disposition to sleep, the tumid belly and large neck, even when not goitrous, indicate what is imminent, if the child be not removed from the locality. He is unable to walk until the sixth or seventh year, and can only utter a few harsh sounds. It is at about the seventh year, that the disease becomes really developed; and all agree, that after this period a child never becomes a cretin under the sole influence of local circumstances. There seems in the cretin to be no intermediate age between childhood and puberty (usually about 20) or between puberty and old age. Infancy is prolonged to puberty, and old age at once succeeds. Some writers state that puberty exerts a favorable change, and others that the imbecility then increases; but this discrepancy probably arises from different degrees of cretinism being referred to, since local observers state as a rule, that the true cretin usually degenerates, while the semi-cretin, by the improvement he then makes, seems to repay the care bestowed upon him.

In the *adult* state the stature of the cretin rarely exceeds  $1\frac{1}{2}$  metre; and a great number examined by the Commission did not reach three French feet (0.975 met.). There is a disproportion between all parts. The feet are too large for the body, the head large and pendant, the chest too small, and a protuberant belly is supported by wasted legs. The form of the head is almost always misshapen, though differently so in different localities. It is usually flattened in front and behind, protruding laterally, and rising conically to the point of juncture of the lambdoid and sagittal



sutures. The face in all bears the same impress of stupidity. The eyes are distorted by convergent strabismus, frequently half closed; and when open, they have not the slightest expression, while the eyelids are often in a diseased state. The enormous zygomatic protuberance adds much to the general ugliness of the face. The mouth, too, is of extraordinary size, with swollen lips, the lower one being pendant, so as to allow a constant flow of saliva. The tongue, of large size, is seen between the teeth. The second set of teeth are sometimes never developed, and at others become diseased. The lower jaw is strong, and usually pendant. The sexual organs are, in some cretins, of enormous size, and in others atrophied; but in both cases few hairs are present. In true cretins the penis is an unformed mass, incapable of erection.

In respect to the performance of the various *functions*, it may be observed, that in proportion to the degree of imbecility, the respiration and circulation are slower, and the temperature reduced. Digestion, judging from the enormous quantity of food consumed, would seem little affected, but complete cretins are very liable to dysentery. True cretins, whether female or male, do not possess the power of reproduction, but semi-cretins occasionally do. The lasciviousness of the latter has been much exaggerated by authors, and masturbation is not of frequent occurrence.

When the affection does not exist in its most intense form, the cretin is capable of affection or aversion towards those who feed or torment him; and save in the completely imbecile, is actuated by fear from past suffering. The deficiency of the moral sentiments is one of the most marked characteristics. Notwithstanding the inexpressiveness of the eye, the sight is excellent, and blind cretins are rare. Hearing, on the other hand, is very generally defective; and the true cretins seem to be destitute of smell or taste, swallowing whatever is given them. Semicretins can distinguish objects by the taste. Touch seems little developed, the hand being used merely for prehension, though some cretins acquire great dexterity in twisting their sticks. There is little transpiration; and they are very indifferent to even great extremes of heat and cold. Even when possessing the power, cretins are averse to muscular exertion; their walk is like that of a drunkard, and they fall before the least obstacle. Every opportunity is sought for indulgence in sleep. The voice of the cretins resembles the cry or howling of an animal, and is only employed to express their wants, although they sometimes imitate the sounds of man and animals. In the semicretins it is harsh and childish; and the least affected of these, though capable of pronouncing a sentence, are yet unable to maintain any part in the simplest discourse, and facilitate comprehension by gestures. This want of power of expression may serve as a measure of the degree of the affection. More or less imbecility is a constant characteristic, and exists in proportion to the intensity of the disease. When this is slight, the individual can beg or perform various actions for his subsistence. A singularity, pointed out by Maffei, is very common, namely, the suspension, during several times a day, of whatever degree of mental activity these cretins may possess: the most utter immovability both of body and mind then prevailing. There are few so utterly destitute of memory, as not to remember those who feed or annoy them; and of all the faculties, it is that which is least damaged, and upon the existence of which prospects of

amelioration must be based. Some disposition to sociability is felt only by the semicretin; the true cretin coveting solitude, and exchanging his ordinary apathy for hatred when in the presence of other cretins, with whom he will contend and fight. Even the semicretin only seeks the society of the opposite sex during the prevalence of the instinct of reproduction. Some of the semicretins can be induced to perform slight services for those who feed them, but this only with excessive slowness and when urged by hunger or fear of suffering; always on the approach of fatigue desisting for repose in spite of all opposition.

After detailing these various symptoms at length, the Commission thus indicates those which it considers as essentially constituting cretinism.

"It is needless to observe, that the number and intensity of these symptoms are by no means the same in all cretins. Their differences and gradations are as varied as there are varieties of cretins, from the simple idiot from birth, to one endowed with mere vegetative functions. But some of these symptoms are proper to all cretins, and really constitute cretinism; whilst others are only the effects of the first, to which they form a secondary dependence, so that they may, or may not, be met with without changing the essential nature of cretinism.

"Recapitulating the essential signs and symptoms of cretinism, we may assert, that all cretins present, in a greater or less degree,—1. A badly formed head, generally flattened before and behind, and exuberant laterally. 2. A disproportion of the various portions of the entire body. 3. Imperfect nutrition. 4. In general absolute impotence; and, at all events, great torpidity of the reproductive powers. 5. Little muscular energy, indecision in voluntary movements, and an impossibility of continuing these for long. 6. A total absence or notable imperfection of articulate language. 7. An expression of imbecility in the countenance. 8. Intellectual powers below those possessed by persons of the most ordinary capacity.

"The coexistence of all the preceding conditions constitutes absolute cretinism. The absence of some and a diminution of their intensity constitutes semi-cretinism. Between these two extremes there are infinite gradations, as each cretin may want what abounds in others, and possess in abundance that in which they are defective." (p. 48.)

It seems to us, that, if these are to be considered the essential signs of cretinism, a large number of beings, usually termed idiots, in this and other countries, will be embraced within their scope. The definition offered by Dr. Guggenbühl is scarcely more distinctive. He states cretinism to be "a defective development of body and mind, or a degraded state of our nature, which gets worse year by year, until the dignity of human nature entirely disappears." In fact, we find no mention anywhere in this Report, of idiots properly so called; and we have no doubt, that those persons who are said to be suffering from the lesser degrees of cretinism, are identical with the beings designated as idiots in other countries. Dr. Forbes, on his recent visit to the Abendberg, of which we gave an account twelve months since, observed several inmates in no-wise distinguishable from ordinary idiots.

*Diseases of Cretins.*—The mortality of infants menaced by cretinism is relatively very high, the majority dying soon after weaning, from convulsions, diarrhoea, or hydrocephalus. Cretins who escape death in their infancy, rarely live beyond forty years; and then only in districts where cretinism is rare, or when their families are in easy circumstances. Almost all the documents fix the duration of life at from twenty to forty years, the maximum relating to incomplete, and the minimum to complete cretinism. It is rare to find a family in which it prevails, reaching the fifth generation;

and but for constant immigration, whole districts would long ago have been depopulated.

Still the health of the cretin is frequently good; and, owing to his inertia and insensibility, he is not easily affected by slight ailments or changes of temperature. When really ill, he flies society, and can be got to submit to treatment only with difficulty. Asthma, induced by goitre, or by malformation of the neck, is of frequent occurrence. Convulsive affections, too, are common; and, occasionally, attacks of furious mania are observed, though very rarely.

*Connection with Goitre.*—A full third of the cretins were returned as goitrous. Goitre is, however, found almost generally prevailing at the entrance of certain valleys where cretinism is unknown. It is sometimes congenital; but it is usually at puberty, and among women during their first pregnancy, that it becomes most developed. It is found most frequently in females, and a hereditary disposition to it is especially conferred by the mother. After puberty it does not usually increase in size, but then may assume a cartilaginous or even osseous density. Generally it is very moveable and pendant, but at other times lies flattened and compressed between the neck and sternum, giving rise to attacks of asthma, to cardiac lesions, change of voice, cerebral congestion, &c. Goitre spares neither sex nor age, social nor intellectual condition, and attacks the more recently arrived inhabitants as well as the indigenous ones. The following are the conclusions the Commission draw from a consideration of the numerous replies they have received:

"1. The goitre observed among cretins, and in the inhabitants of mountainous countries, is of a peculiar character, and should not be confounded with scrofulous goitre. 2. Contrary to what occurs in this last, it rarely spontaneously suppurates. 3. The goitre of the inhabitants of valleys, is quite consistent with a state of perfect general health, which is not the case in the scrofulous form. 4. A greater number of goitres in a country does not give rise there to a greater number of cretins. 5. If a greater number of goitres is found coexisting with a greater number of cretins, that does not arise from any influence of the one upon the other; but merely from the fact, that among the numerous causes which may concur in the development of cretinism, some of them may also contribute to the production of goitre. 6. Among the causes which may engender goitre, we almost constantly meet with a bad condition of the potable water, and bad living, and often hereditaryness, especially on the mother's side. 7. The frequency of cretinism bears no direct relation to goitre, for goitrous persons are not always cretins, nor are cretins always goitrous." (p. 45.)

There are certain affections which are supposed by some authors to have an affinity with cretinism. Thus *deaf-dumbness* is of frequent occurrence in countries wherein cretinism prevails; and as cretins are very commonly deaf, some authors regard congenital deafness and difficulty of speech as a variety of cretinism. But the deaf-dumb person does not speak, because he is deaf; while however good the hearing of the cretin may be, he yet never speaks properly. The one too has his language of signs, and thinks, which is not the case with the other; while the susceptibility of education, and the imputability of actions, still further point out the chasm which separates the two.

Ackermann believed cretinism to be only a special form of *rachitis*; but the cretins are few who manifest this peculiar condition of the osseous tissue. Moreover, in even the minor degree of cretinism, there is a total

want of that intellectual power and facility of expression, which are not infrequent in the rickety.

Many writers have confounded *scrofula* with cretinism; and as the distinction is by no means clearly made out by even the most recent of these, we shall give the views of the Commission in the words of the Report:

"The manner in which the symptoms and precursory signs of cretinism manifest themselves in infancy, bears some analogy to the symptomatology of scrofulous affections. Several authors have, in consequence, confounded the two affections; and in order to explain away the difficulty which the intellectual activity observed in *scrofula* presents, they have imagined a new variety of the disease, which they have termed *passive scrofula*, principally characterised by defective energy in the acts of the life of relation. And, in fact, among the characteristics assigned to the scrofulous constitution in children, authors enumerate the short thick neck, the large jaws, the puffed face, the relaxed state of the tissues, the tardy progress of dentition and ossification, the weakness of muscular power, and the late period at which the power of walking is acquired; all which symptoms are frequently found in young cretins.

"In spite, however, of these features of resemblance, any one who will carefully examine the phenomena presented by the two diseases, will easily, even in infancy, recognise notable differences between them. The scrofulous subjects have generally a very large head, which projects, especially at its posterior part; while the cretins generally have no occipital protuberance. In the one we find a white skin and fresh complexion, and a degree of roundness of the features which presents an agreeable aspect; while the others are pale, of an olive-yellow, and always misshapen. The former have the upper lip large and projecting, the nose red and shining at its point, and the nostrils swollen: the latter have a pendant lower lip and a flattened nose. The differences become more obvious as both advance in age. Glandular tumours, particularly in the neck, and eruptions of the face, with consequent ulcerations, so constantly seen in scrofulous children, are very rarely met with in cretins. The reproductive powers are precocious in the former, and retarded, imperfect, or quite absent in the latter. Moreover, if scrofulous disease or cretinism once become completely confirmed, the latter may be consistent with a perfect physiological condition; while a scrofulous affection is never found in connexion with a state of complete health. In one word, cretinism cannot be considered as a disease, in the proper acceptance of this word; while a scrofulous affection is so always." (p. 49.)

Although not exactly understanding how cretinism can be said to be consistent with a perfect physiological condition, the distinctions drawn in the above extract are probably well founded. Still several good observers maintain the connection of the affection with *scrofula*. Thus Dr. Hensinger, in allusion to Guggenbühl's division of cretinism into four forms, the rachitic, the atrophic, the hydrocephalic, and the innate, remarks, that the rickety form may be regarded as a scrofulous state of all the bones, the atrophic as a general *scrofula*, and the hydrocephalic as a cerebral *scrofula*. In a report inserted in the '*Nouvelliste Vaudois*,' No. 58, 1849, it is stated, that it had resulted from an official inquiry, that most of the children visited in the institution in the Abendberg were suffering from curable *scrofula*, and not from cretinism; and the present commission expresses a somewhat similar opinion. It would seem, therefore, that either the diseases are associated together more frequently than supposed by some, or have become mistaken for each other. Examples of what is here termed passive *scrofula* are by no means rare in this country; the children manifesting almost as slow a physical and mental development, as the young cretins are represented as doing; and being alike apathetic to all that

surrounds them. But such children are either carried off by some acute disease, or at the epoch of puberty a revolution takes place in their system, which is followed by an amount of additional development the unfortunate cretin is not susceptible of. Contrasting it with what he had observed in Switzerland, Dr. Guggenbühl was much surprised at the very numerous cases of virulent scrofula which he met with during his journey in Holland, a country he pronounces the freest from cretinism of any in Europe.

*Classification of Cretins.*—After adverting to the classification of cretins proposed by Fodéré, Rösch, Maffei, and others, the Commission thus indicates its own views :

"The offering a more exact classification, fitted to facilitate the scientific and practical study of cretinism, is, if not impossible, at least a difficult matter ; because cretinism is not a simple pathological fact, which affects a single function, apparatus, or organ ; and the groups of symptoms which constitute its different degrees, are never manifested in an isolated manner in the same individual.

"Nevertheless, the careful study of the symptoms presented by cretins, and the frequency of some of them compared to others, have determined the commission to establish three classes conformably to the order already observed in the description of the symptoms : 1st. Cretins endowed only with vegetative faculties, entirely destitute of reproductive and intellectual powers, and not capable of employing articulate language,—*cretins*, simply so called. 2d. Cretins endowed with vegetative and reproductive faculties, and some of the rudiments of language. The intellectual faculties are strictly limited to expressing the bodily wants, and are due solely to the impressions of the senses—*semicretins*. 3d. Cretins endowed with vegetative and reproductive faculties, and possessed of a less imperfect language carried on by words and gestures. Their intellectual faculties are also less limited, but yet always below the ordinary level : and they possess some power of acquiring an occupation, and of undergoing labour—*cretinous patients*." (p. 55.)

*Geographical Distribution of Cretinism, especially in the Sardinian States.*—Cretinism, the authors of the Report say, may be found existing *sporadically* in every country ; but *endemic* cretinism is especially seen in the vallies of mountainous districts. It is not, however, exclusively confined to these, for Piedmont presents examples of its existence in plains. The *sporadic* form is especially found in narrow, ill-built, overcrowded towns ; while well-built towns, peopled by an active and industrious population, seldom offer examples of it. Turin offers a good example of what may be done by sanitary measures ; for since its perimeter has been doubled, and the exertions of its municipal authorities have rendered it one of the cleanest and most beautiful cities of Italy, the number of deformed, rickety, and scrofulous subjects has prodigiously decreased. Sporadic cases of cretinism are also met with in villages which are remote from the provincial capitals, having few roads near them, and placed in damp localities ; but even here these cases are only found among the very poor, the parents themselves often being half idiotic. As a general rule, cretinism is not met with *endemically*, higher than from 2 to 3000 feet above the level of the sea ; but exceptions occur to this, especially in the secondary vallies of the province Aosta. Professor Forbes found it very prevalent in one of these, that of Ollomont, at a height of 4000 feet. In Switzerland it principally prevails in the Valais, the Grisons, Argovia, Friburg, and Coire. In the Sardinian dominions it abounds in the Pennine and Graian Alps ; and next in Salzburg, the Tyrol, Styria, Carinthia, and Carpathia. "It is moreover frequently met with in the Pyrenees, Brittany, and the Jura in France,

Wurtemberg, Saxony, the Hartz mountains, Bavaria, Thuringia, Hesse, and England." We are not aware upon what authority the Commission places England among the countries in which cretinism (if anything beyond idiocy is intended to be expressed) abounds. In the pamphlet, the title of which is placed at the head of this article, it is also stated, that "A little village in Yorkshire has 20 cretins among 200 inhabitants;" i. e., a far higher proportion than the most cretinous districts of Alpine countries!

Entering into the consideration of endemic cretinism as it prevails in the Sardinian states themselves, the Commission states it is found thus prevailing in Aosta, in several provinces of Savoy, in the Canavese, in some of the plains and vallies of the provinces of Coni and Saluzzo, and in different parts of Pinerolo. The principal centres of development are the vallies of the Graian and Pennine Alps which surround Mont Blanc, the valley of the Dora Baltea, the vallies of the Arc and the Arve in Savoy, and of the Orca in the Canavese. The topographical details of each province in its relation to cretinism are set forth at great length; but we must content ourselves with transcribing the conclusions:

"1. Endemic cretinism is limited to the vallies and plains which belong to these grand Alpine elevations, having for their centres the three summits of Mont Viso, Mont Blanc, and Mont Rosa. The disease commencing in the primary ramifications of the Maritime Alps, increases in the Cottian Alps, and acquires its highest degree in the Graian and Pennine Alps. 2. The condition of these different vallies is so similar, whatever may be their direction, that a person traversing them in succession might believe he had never left the same valley. 3. The vallies most infected are the deepest and narrowest, the most damp, and the most destitute of air and light. 4. Cretins are met with especially in abodes situated at a distance from large towns, having the worst exposure and most wretchedly built, being frequently hidden by trees, or in the vicinity of marshes. 5. In towns and villages much frequented by strangers, it is only in the streets farthest removed from the consequent activity and progress of civilization, that cretins are found. 6. To these conditions, however, there are found so many exceptions, that it becomes impossible to determine anything absolute as to the relations subsisting between local circumstances and goitre and cretinism." (p. 83.)

*Statistics of Cretinism in the Sardinian States.*—This chapter contains the detailed tabular views of the answers received by the Commission to their various queries; and we may state some of the general results. The value of such tables is much impaired by the fact, that answers to some of the queries were only returned by a portion of the correspondents. The first set of tables is intended to exhibit the relative amount and intensity of cretinism in the various geographical divisions of the continental portion of the kingdom. From these it appears, that from the seven divisions, Savoy, Aosta, Turin, Cuneo or Coni, Alessandria, Novara, and Nizza or Nice, having a population (in 1838) of 2,651,106, there were returned 21,841 persons as suffering from *goitre*, of whom 4323 were men, and 5236 women, while in 12,282 the sex was neglected to be specified. The numbers suffering from this disease are, in fact, much greater than those indicated; as sporadic cases were not returned, nor those prevailing in places where cretinism was not endemic. The total number of *cretins* returned amounted to 7084. Of these, 2011 (1120 men and 891 women) were destitute of goitre, and in 3912 (1953 men and 1959 women) this existed; the fact whether or not it was present,

not being stated in respect to the remaining 1161. As to the intensity of the cretinism of the 7084 cases, 2165 are returned as confirmed cretins, 3518 as semi-cretins, and 434 as cretinous, 967 remaining unspecified. The per centage of the inhabitants affected by cretinism varied in the seven divisions of the kingdom from 0·01 to 2·79, this last ratio only being found in the province of Aosta. In Savoy, the proportion varies greatly, being 0·20 in Savoy Proper, 2·72 in High Savoy, 1·45 in Tarentaise, 2·27 in Maurienne, and only 0·01 in Geneva. In no other parts of the kingdom, save Savoy and Aosta, did it reach 1 per cent.

The second table professes to give an account of the months of the year in which the cretins were born, the periods at which cretinism or goitre manifested itself, and the ages of the cretins at the period of the investigation; but the large proportion of the returns, in which these particulars are not specified, much interferes with the conclusions to be drawn from them. The births of 4681 of the cretins, concerning whom the information was returned, seem to have been too equally distributed over the different months of the year, to justify the opinion which attributes influence to season. The age at which cretinism was supposed to have commenced, was returned in 4888 cases; but these data are of a very uncertain character, derived, as they are, from non-medical observation, and obscured by the wish which the mothers generally have to conceal the congenital character of the disease. Of these 4888 cases, it is returned as occurring before the second year in 4440, between the second and fifth in 187, between the fifth and twelfth in 202, between the twelfth and twentieth in 31, and after the twentieth in 28. The goitre (of 3201 in which this point is indicated) occurred before the second year in 2333, between the second and fifth in 199, between the fifth and twelfth in 449, between the twelfth and twentieth in 157, and after the twentieth in 63. The ages of the cretins, at the time of the inquiry, are returned in 4955 cases. Of this number, 331 were less than ten, 1332 between ten and twenty, 1339 between twenty and thirty, 1021 between thirty and forty, 442 between forty and fifty, 322 between fifty and sixty, and 168 sixty and upwards.

*The Etiology of Cretinism.*—The authors of the Report believe, that the hitherto unsatisfactory results which have attended the investigations of others on this point have not only arisen from the defective manner in which these have been conducted, and the difficulty of the subject, but also to the fact of their having been made only in special localities, the result of which has been to attribute the production of the disease to causes that are not of general application. They hope to obviate this objection by having obtained returns from so many and such various localities.

The *predisposing causes* are distributed into three categories. 1st. Those general causes inherent to the localities, which are little susceptible of amendment. 2d. Those derived from the mode of life followed by the inhabitants of infected localities, and which are, to a certain extent, remediable. 3d. Those which more especially and nearly relate to the families in which cretins are found.—Each of these is dwelt upon at great length in the Report.

1st. *Permanent Local Causes.*—Among these are—(a.) The *situation and frequency of the country*. Although there are exceptions to this, the

disease is generally found in deep, narrow, tortuous valleys; the secondary valleys, in which from their pent-up condition the wind can only blow in one direction, being the worst. (b.) *Elevation*. Although many exceptions exist, it is true, as Saussure represented it, that cretinism is seldom found endemic at a height of 500 toises (about 1000 metres); but this may arise, in some degree, from the fact, that habitations and cultivated lands are chiefly found below such elevations. Archbishop Billiet has indicated many places wherein cretins are found at a great height, as Mont Cenis, 1382 metres; Aussois, 1498; and Albiez le Vieux, at 1566 metres (near 5000 feet). (c.) *Humidity*. The air in the infected localities is always overcharged with moisture, and the unhealthy appearance of the mass of the inhabitants betokens the deleterious influence of miasmata. That humidity alone will not produce the disease, is shown by its rarity in Holland, and in grounds on which rice is cultivated. This cause has only an indirect action, by deteriorating the general health. (d.) *Absence of wind*. In these close valleys, ventilation by means of currents of air never occurs, and the access of air is still farther prevented by the bad construction of the houses, and by the trees surrounding them. (e.) *Variations of temperature*. At midday, the temperature of the valley may be so high as almost to impede respiration, but at night and morning it falls greatly. Sudden changes are common, the thermometer falling perhaps from 60° or 70° to 32°, to rise shortly again to 75° or 85°. In winter, too, on leaving their heated huts, the inhabitants are exposed to a very sudden change. (g.) The *absence of direct solar light* has been frequently signalled by authors as a chief cause of cretinism. Some villages are entirely deprived of it during several months of the cold season, and enjoy only two or three hours' light daily in the summer. On the other hand, there are villages in which cretins abound, fully exposed to the sun's rays. (h.) *Defective electrical conditions*. As electrical tension is diminished in the valleys, Iphofen and others have referred the defective physical and mental activity to this, the absence of electrical meteors and hail-storms adding force to the opinion; but valleys adjoining to the infected ones, and in the same electrical conditions, are found free of cretinism. (i.) *Miasmata*. Great importance has been justly attached to the fact of the mountain streams being allowed to spread out into stagnant marshes, generating poisonous miasmata; for, where these have been embanked, a diminution of cretinism has always resulted. (j.) *Bad water*. Although almost all travellers, from Dr. Reeve to Dr. Forbes, testify to the excellent character of some of the Alpine waters, the inhabitants of the infected localities attribute much influence to them, especially in the production of goitre. The snow-water theory seems to be generally abandoned; but many of the streams are very turbid, and loaded with sulphates and calcareous carbonates. Still, cretinism abounds in places wherein the waters are excellent; while in some of those watered by the turbid Dorea, neither it nor goitre are met with. Professor Cantù, to whom the specimens of the waters from the various districts were submitted, has long been engaged in such investigations, one result of which is, that the waters in healthy localities contained portions of *iodine* and *bromine*, in which those from the infected districts are deficient. M. Grange has made several communications to the Académie des Sciences,\*

\* Annales de Chimie, 3d ser., vols. xxiv and xxvi.



detailing the analyses he has made of the waters, as they streamed down over the various soils from the glaciers to the Isère; and he states, as the result of close inquiry, that wherever goitre or cretinism prevailed endemically, the waters in use always contain notable quantities of magnesian salts. Archbishop Billiet, in a note addressed to M. Grange, states that his observations apply exactly to Savoy. (*k.*) *Peculiarities of soil* are believed by many writers to be active causes of the disease; but this is usually the case with those who are only familiar with the disease in some one locality. Bishop Rendu, of Annecy, states that the disease is never found endemic, except where the rains and torrents carry down a schistous detritus, and deposit it in the hollows below; and this observation receives weight from the fact, that cretinism ceases in Savoy exactly where the large central schistous elevations give place to the calcareous elevations of the Jurassic system. Archbishop Billiet, too, who has evidently taken much pains in investigating the subject, in his communication to M. Grange, just alluded to, maintains that the geological constitution of a country infested with cretinism should be especially regarded. He indicates localities in which geographical circumstances, humidity, absence of light, and extreme poverty, all coexist, and yet neither cretinism nor goitre are found in them; while certain other parishes, excellently circumstanced as far as hygienic conditions are concerned, are yet infested with it. He attributes, with M. Grange, great influence to the operation of magnesia and magnesian salts, which are inherent to the talcose and anthraciferous soils on which cretinism appears; while, when the compact calcareous soil, without argillaceous admixture, is reached, it no longer prevails. Other correspondents, however, inform the Commission, that the disease is most abundant where the calcareous strata abound, and where the waters are overcharged with sulphates and carbonates of lime, as were some of those submitted to M. Cantù. Moreover, in some of the secondary valleys in Aosta, adjoining those producing cretinism, and of the same geological conditions, the disease is unknown. The Commission believes that the only influence of the soil is indirect, and dependent upon its want of fertility, rendering subsistence more difficult. (*l.*) *Condition of vegetation and the domestic animals.* The statements made by Ferraris and others, that animals and plants degenerate in the cretin districts, are much exaggerated in respect to those indicated by them, while they are totally untrue as a general rule, both animals and plants in some districts attaining a remarkable perfection.

2. *Removable Causes.*—Among these may be mentioned—(*a.*) *The bad position of the villages.* Those in which cretins abound are usually built under the shadow of some mountain top, or within its angles, frequently near stagnant water, and overshadowed by trees, so that the naturally unhealthy condition of the valley becomes aggravated. Villages having a northern exposure, contrary to Saussure's observation in Switzerland, are especially liable to the disease. (*b.*) *The wretched condition of the dwellings.*—The houses in these villages seem constructed solely for guarding against the cold of long winters. They are so huddled together and surrounded by trees, that the highest winds can hardly reach them. For the sake of the warmth, too, the inhabitants usually inhabit the same apartment with their cattle, remaining thus in a heated atmosphere, amidst almost utter darkness, small holes supplying the place of windows, and surrounded

by ordure and horrible filth of every description, producing a stench absolutely intolerable to the uninitiated. The room is so low, that a person can hardly stand upright, and so narrow, that man and beast can hardly be crammed into it; while the breath of the inmates, joined to the evaporation from the excrementitious substances on the soil, keeps it in a constant state of heated humidity. It is in such abodes that the women and children crouch day and night, without ever undressing for repose, during seven months of the year; the men only leaving occasionally for some short occupation out of doors, when they are exposed to a variation of temperature of 30 degrees or more. (c.) *Food*.—Rye, barley, or maize, is the ordinary food, wheaten bread being a luxury; while many are obliged to content themselves with chesnuts, potatoes, peas, cabbages, &c. made into soups with some skimmed milk. The uniformity of this diet is as injurious as its innutritious character. Even the wealthiest inhabitants only taste animal food at festivals, and then it is usually smoked or salted. Some authors attribute cretinism to the general use of the potato, and the neglect of the employment of salt; but these conditions are neither constant nor general, and when present, act only indirectly. Food of this slight nutritive power is taken in enormous quantities, to which perhaps the enlarged bellies and torpidity of the inhabitants of these districts may be attributed. (d.) *Clothing*.—This is insufficient and filthy, never being changed for washing, but worn till it drops off, the receptacle of dirt and insects. (e.) *Social position and habits*.—The pooriness of the soil, and the absence of industrial occupations and commercial intercourse, reduce the inhabitants for the most part to extreme poverty—cretinism being generally found intense in proportion to the indigence of the locality. Even those of the inhabitants who become wealthy, still lead the same wretched lives as the others, employing their money in merely increasing their possessions, without seeking to improve their physical or intellectual condition. There are schools for boys in several of the districts; but they are opened only during winter, and the children are cramped into them in a state of compelled inactivity. The superstition and ignorance of the inhabitants of some of the remote regions, cannot be surpassed even by races of savages. After spending the winter months huddled together with their cattle in the stables, both men and women work laboriously when the disappearance of the snow allows them; but this is only to accumulate enough to live upon in the same wretched manner during the ensuing winter. The absence of all commercial activity and intercourse, impresses upon the inhabitants of these districts an appearance of the most excessive indolence. The children are not active like other children; but, serious beyond their years, sit vacantly gazing. The adults are slow in walking and in speech, and always ready for sleep. Cretins are always found to abound where this brutalization prevails; but when by opening up new routes, or other circumstances, an active intercourse with strangers has infused new life into these districts, then cretinism notably diminishes. So where lines of intercourse have been diverted from places they once traversed, these become plunged into this forlorn condition. Inactivity is the grand characteristic of the infected districts. Eating and drinking constitute their only recreation, and no amusement is followed that requires exertion of body and mind. On festival days a large portion of their savings is spent in brandy. The morning is far advanced before they commence

their occupations; and Dr. Trombotto remarks, that while he traversed the Val d'Aosta, he never heard a village song. (f.) *Diseases*.—Typhoid fevers, as might be supposed, commit great ravages among such populations; and the intermittent fevers, generated by the stagnant waters, are scarcely less destructive. Articular inflammations, pneumonia, and phthisis are common results of the extreme changes of temperature. In the worst districts the inhabitants generally are cachectic, of short stature, with the bony portions of the frame disproportionately developed. Scrofula and rachitis are rather common, and goitre very common; while, when it is not present, there is still a thick doughy-looking neck.

3. *Individual Causes*.—(a.) *Marriage*.—There does not seem to be any foundation in the opinion which refers the production of cretinism to late marriages, young married people frequently producing cretins. The prevalence of the disease, however, seems to bear a proportion to the custom of intermarrying within the cretin districts; especially disappearing when the wives come from non-infected ones. The intermarriage of healthy persons with cretinous subjects is not of frequent enough occurrence to act as a general cause. If the woman is not a cretin, the offspring too may be healthy; while if she be, she is generally sterile. The statistical returns show how rare are marriages contracted beyond the limits of the infected districts. Thus, of 4009 fathers, 3915 were born in the infected places, 62 in non-infected, and in 32 the particular is not stated. Of 4015 mothers, 3881 were born in the infected districts, 70 in non-infected, and in 64 this is not specified. (b.) *Sanitary condition of the parents*.—According to the imperfect returns made, the proportion of goitrous fathers was 1 to 3·75, of mothers 1 to 3. The number of semicretin fathers was 1 to 25·63, and of mothers 1 to 36·83. The fathers exhibiting something abnormal in their health or general appearance were as 1 to 2·87, and the mothers as 1 to 2·50,—a large proportion of parents thus being returned as healthy. Dr. Trombotto, personally inspecting the localities, declares these returns to be completely wrong, based, as they probably were, upon the fact of the persons not being absolutely laid by. He states, that in localities wherein the disease is endemic, cretins have generally a father, oftener a mother, and sometimes both, scrofulous or rickety, or suffering from goitre or other ailment; and where this has not been the case, some near relative has presented these diseased conditions. Examining into certain isolated facts of parents healthy in appearance arriving from a distance, and generating cretins in an infected locality, he has constantly found either in them or their immediate relatives the above dispositions. In respect to the *hereditary character* of cretinism, the Commission enters at some length into the distinctions which prevail between this and the hereditary character of phthisis; inasmuch as, in this last, a person must be himself phthisical to convey the disease, while in cretinism any organic vice of the constitution, aided by concurrent external circumstances, may suffice to propagate it. An acquaintance, however, with the writings of Sir James Clark and the late Dr. Todd upon Dyspeptic Phthisis, would have shown that the difference here set up is an imaginary one.

*Appreciation of the Predisposing Causes*.—In reviewing these, the Commission finds the most constant to be the humidity of the air or its vitiation by any of the numerous causes mentioned, the faulty composition of the waters, and the deficient nutritive power of the alimentary sub-

stances employed. The other causes it regards as secondary, partially prevalent, and of only concurrent operation. The action of the three first-mentioned causes is to deprive the organism of the elements most essential for the maintenance of life; but they cannot be regarded as the direct cause of the production of cretinism. They are the same causes of insalubrity, which, when less numerous or less permanent in other countries, produce scrofula, rickets, and other organic vitiations. To no one of them can the genesis of cretinism be exclusively attributed, without numerous exceptional circumstances rising in contradiction. For the development of the disease, the concurrence of several causes is requisite. Fortunately those of a local and irremovable character are insufficient, and require the co-operation of others dependent upon the conditions of social life, and therefore more tangible.

The proofs of the truth of this statement are derived not only from the flourishing condition of vegetation and domestic animals in many of the infected vallies, but also from the comparison of the conditions of adjoining places. For this purpose the Commission has selected the valleys of Challant and Gressoney, two secondary valleys of the Val d'Aosta. In these the topographical conditions, supply and composition of waters, depth and width of the vallies, nature of the soil, direction of the winds, character of the seasons, supply of light, the condition of vegetation,—in a word, all immovable circumstances,—are absolutely identical. Yet in the Challant, cretinism and goitre abound, while in the Gressoney they are absolutely unknown! The latter valley is inhabited by an industrious population, living in well-built villages, the houses of which are spacious, unsurrounded by trees, amply provided with windows and doors, and of a remarkable cleanliness. Work is abundant, and a feeling of kindness prevalent. The physical and intellectual education of the children is well cared for. Dress is simple and appropriate, and frequently changed. Without being intemperate, the inhabitants have an abundance of food. The Commission may well pronounce this a model valley; and if some of our own sanitary inspectors are induced by Dr. Forbes's enthusiastic recommendations to visit the the alpine regions to recover strength after ungrateful and ill-requited labours, they cannot do better than repair to the happy Valley of Gressoney to receive renewed health and inspirations. To describe the condition of the other valley, would be only to repeat all the revolting particulars with which we fear we have already fatigued the reader. Prior to the opening the routes to France through Tarentaise and Maurienne, cretinism prevailed as intensely in those provinces as it now does in the Val d'Aosta; but since that period, great have been the improvements. Numbers emigrate during the long winters; good roads have been constructed, torrents embanked, marshes drained, an active commerce established, and cretinism banished, so to speak, to unimproved localities. The Commission states that it may be laid down as an absolute rule, that wherever cretinism has diminished, it has done so in consequence of the adoption of sanitary measures alone.

It is by bringing this important fact so prominently forward, that the Commission is likely to do most good. Much zeal and industry have been wasted in searching for the specific cause of the disease; but it now bids fair to be brought within the same category as typhus, ague, plague, and the like. We do not know the nature of the poisons upon which these

diseases depend, but we have acquired a knowledge of the avoidable circumstances, upon the removal of which their virulence becomes abated, or even annihilated. Cretinism is only another example of how large a portion of the disease with which mankind is afflicted, is preventible.

*Proximate Causes.*—Owing to the prejudices of the relatives, only five opportunities of examining the brain in cretins have occurred to the correspondents of the Commission; but one of their body, Professor Bellingieri, unfortunately since dead, has collated the various observations upon this point, that have hitherto been published. Many discrepancies prevail among these, but the following considerations may reconcile some of them, and serve to generalize the subject sufficiently:

“When describing the cretins, in the former part of this work, and considering the classification under which they might be best ranged, we remarked, there was no true type to which all these degenerate creatures might be referred; and that there was as much difference, perhaps, in the intensity and manner of manifestation of cretinism, as there was in the individuals affected. It is precisely to these differences that we must attribute the want of agreement among authors in regard to the lesions observed by them in the bodies of cretins. The characteristics which each individual cretin presents, and the conditions which accidentally distinguish cretins, are so variable, that it is not surprising that the pathological lesions are not identical,—the cerebral substance being too firm in one, too soft in another, one having the hemispheres too small, another without symmetry, &c.

“Still the differences observed in these various relations may be reduced to diversities of form, rather than of essence; for all authors agree in stating, that cretins, in a greater or less degree, present some anomaly in the cranium and encephalon. All have observed a defective symmetry and proportion, and an altered structure in the bones of the cranium; sometimes an excess, but more frequently a defect in the development of the encephalon or some of its parts, as well as a departure from its normal consistence, and various lesions, according to the degree of cretinism, and the peculiar circumstances of the cretin, in respect to diseases, &c.” (p. 206.)

The various opinions which have been offered as to the proximate cause of cretinism, may be reduced, the Commission believe, to four principal ones, none of which rest on a secure basis. Malacarne and Fodéré endeavour to explain it by the abnormal condition of the brain and cranium; but although these exist in almost all cretins, they are of a very various character, and at most will only explain the idiocy. Ackerman attributed the disease to intense rachitis, while Zachokke and Rösch regarded it as intense scrofula,—opinions which we have already shown to be erroneous; Gugger and Savoyen lastly consider the essence of the disease to consist in a preponderance of the venous condition of the blood, and in the absence of nutritive material. This, however, will not explain why, at great elevations, and in rich families, cretins are met with; and why, where cretinism is endemic, so many individuals, submitted to the influence of the same causes, escape.

*Prophylaxis and Treatment.*—Upon the first of these heads we need add no more. We have seen that many of the causes are removable; and the recommendations of the Commissioners enumerate such as are so, bringing the disease, in fact, within the scope of those sanitary regulations which of late have excited so much attention among ourselves. They also recommend the appointment of a permanent commission of scientific men, having in charge the superintendence of the execution of any measures of

amelioration that may be adopted, the suggesting any additional improvements which the teaching of experience and the progress of science may supply, and the accumulation of accurate statistical data. Much, as we have already seen, has been done in some localities for the diminution of the disease; but there is reason to fear that others have been invaded by it: Dr. Reid, visiting the Valois in 1805, states that the disease was then diminishing, and every subsequent traveller has repeated the same statement. Dr. Forbes, the most recent, says that wherever he inquired, he was assured this was the case; and Dr. Grillet, of Sion, once the head quarters of cretinism, entertains no doubt on the subject. Still, from other statements, the diminution would seem, in some places, more apparent than real; and probably Dr. James Johnson's suggestion, that the progress of civilization has rendered the parents of these poor creatures desirous of concealing them, in place of exposing them as heretofore, has some truth in it. While traversing the back streets of Sion, he saw them driven out of sight as he approached. Dr. Guggenbühl tells an amusing anecdote in point. While travelling in the Valois, he stopped at the village of Bremoi, which once abounded in cretins. The curé declared to him, that not a cretin was to be found there since he had taught the inhabitants the advantage of opening their windows and letting in the fresh air. The cook, who stood by, listening to the conversation, could contain herself no longer, but cried out, "Oh, your reverence, what are you talking about, there is not a house without one!" Archbishop Billiet states, that cretinism prevails in his diocese of Chambéry, very much as it did near a century ago. In a valley of the canton of Argovia, recent investigations have discovered no less than twenty-eight villages infected, some of them having become so quite recently. Still, however this may be with some localities, we think there can be no doubt, upon the whole, that the disease is diminishing, though, perhaps, not so rapidly as some suppose.

But not only has the diminution, and the probable eventual eradication of the disease, occupied attention, but the amelioration of the condition of the unfortunate beings who have already become cretins. The successful endeavour to raise them from their revolting condition to one compatible with some of the pleasures and privileges of rational existence, has been entirely the work of Dr. Guggenbühl's active humanity. A few particulars of the career of this benevolent man may be gleaned from the anonymous pamphlet, the title of which stands at the head of this article. He was born at Mailen, on the borders of the Lake of Zurich, in 1816, and at an early period exhibited a liking for a medical career. Studying at Zurich under Schönlein, Oken, and Troxler, he was in the habit of spending his vacations in the mountainous regions for which he had a special predilection. On one occasion he was struck with the sight of an unfortunate cretin mumbling at the foot of a cross a prayer that had been taught him; and the idea of endeavouring to develop the latent spark of intellect in so unfortunate a being, at once occurred to him. Repeated journeys in the alpine regions brought him in contact with numbers of these unfortunates; and deeply affected by their lot, he resolved to consecrate his future life to their amelioration. He established himself amongst them as a practitioner, at Cleinthal in Glaris, and during two years occupied himself with their treatment. He soon, however, became convinced, that to treat the disease effectually, he must have charge of the children when young; and that

for the proper conjunction of medical treatment and intellectual training, a special establishment was requisite. He published a proposal in 1839, which attracted the attention of a congenial spirit, Emanuel de Fellenberg, who invited him to Hofwyl, in order that they might concert together the means of carrying out the project. In putting it into execution, he had to contend with the opinions of persons who regarded it as visionary, just as the views of Pestalozzi had been before stigmatised in the same canton of Bern. Eventually, however, the Swiss and German Natural History Associations investigated the subject, and reported favorably of his views.

Some difficulty at first arose in the selection of a site; but at last the heights of the Abendberg, near Interlachen, 3000 feet above the level of the sea, was chosen. It had already been the seat of an interesting experiment; for Kastofer, the celebrated Swiss forester, had demonstrated, that even in these regions a variety of plants might be successfully cultivated. On hearing the nature of the experiment, he readily yielded possession of a spot, now universally acknowledged as the best that could have been chosen for the object Dr. Guggenbühl had in view.

All who have witnessed the results hitherto obtained, speak of them as highly gratifying, though many believe the whole of the inmates cannot properly be regarded as cretins. Professor Demme, of Bern, reported, that of eighty children treated, the intellects of a third had become sufficiently developed to allow them to attend the public schools, or follow some simple occupation. More than 300 have now been treated there, a very large proportion of whom have been materially benefited. It is, however, melancholy to think, that an experiment, fraught with so much good to the human race, is carried on, from want of sufficient funds, on so limited a scale. Dr. Guggenbühl's first object is to fortify the system by an appropriate regimen and medicinal treatment; and it is not until the health is ameliorated, the functions regulated, and the bodily strength thus increased, that intellectual and moral culture can be safely commenced. The extent to which this may then be carried, by appealing to the dormant senses and instincts of the child, is truly remarkable: but as the principles upon which the management is based do not materially differ from those guiding the education of idiots generally, we need not dwell upon them now, merely referring to an excellent article upon the subject in one of our predecessors.\* Dr. Fauconneau Dufresne, in an interesting account of cretinism (*Rev. Med.*, 1846), while doing justice to Dr. Guggenbühl's philanthropic exertions, expresses a hope, that the prior labours of the French physicians, in the instruction of idiots, will not be forgotten. We believe that Dr. Guggenbühl's endeavours were educed in the manner we have narrated, and were in no wise imitative; but we also hope, that the labours of Ferrus, Voisin, and Seguin, in the cause of the poor idiot, will always command a grateful remembrance. All who have perused Dr. Conolly's graphic statement† of the results he witnessed, will be aware that ample justice to these labours has been done on this side the channel.

Dr. Guggenbühl believes, that for the effectual treatment of the cretin, from three to six years is required; although occasionally, if the management be commenced within the first two years, two may suffice. Success

\* *British and Foreign Medical Review*, Vol. XXIV., p. 1.

† *Ibid.*, Vol. XIX., p. 292.

is, indeed, usually proportionate to the youthfulness of the subject, when the education is commenced. Until the seventh year there is risk of relapse, if the child be not carefully watched; but after that time the children may return to the infected vallies without this occurring. There are children now in the Valois who have left the Abendberg three years, and yet have continued constantly to improve. M. Cerise remarks (Ann. Med. Pscy., May, 1846), that cretins who have been perseveringly taught to speak, never afterwards fall into that state of idiocy observed in those in whom this had been neglected. It seems to exert a therapeutical effect upon them, contributing not only to the development of the encephalon and intellect, but also acting usefully upon the rest of the economy.

We may be thought to have dwelt somewhat long upon a disease, which, in spite of the Commission, we persist in believing is not of frequent occurrence in England. But although true cretinism does not prevail, idiocy abounds to an extent, little thought of prior to attempts for its treatment being set on foot; and there is strong reason to regard it as endemic in particular localities, and therefore as dependent upon the same class of causes as that which induces cretinism. We hope to be able to lay some information on this subject before our readers at an early opportunity, and shall then notice Dr. Howe's Report, which contains some valuable contributions to our knowledge of the physical conditions of idiocy.

#### ART. VIII.

1. *Fruits and Farinacea the Proper Food of Man; being an Attempt to prove from History, Anatomy, Physiology, and Chemistry, that the Original, Natural, and Best Diet of Man is derived from the Vegetable Kingdom.* By JOHN SMITH. Second Edition.—London, 1849. Fcap. 8vo, pp. 342.
  2. *Lectures on the Science of Human Life.* By SYLVESTER GRAHAM. (Reprinted from the American Edition.)—London, 1849. Royal 8vo, pp. 289.
  3. *De l'Influence comparative du Régime Végétal, et du Régime Animal, sur le Physique et le Moral de l'Homme.* Par EMILE MARCHAND, D.M.P., &c.—Paris, 1849. 8vo, pp. 266.
- On the Comparative Influence of Animal and Vegetable Diet on the Physical and on the Moral Condition of Man.* By Dr. E. MARCHAND.

IN our former article on this subject, we discussed the validity of the objections raised by the advocates of Vegetarianism against the use of animal flesh as an article of diet. We showed, in the first place, that a large number of those who call themselves Vegetarians are so in nothing else but in name; since they, like the world in general, consume as an essential part of their diet, substances (milk and eggs) which have entered into the composition of animal bodies, although these substances are not obtained by the destruction of conscious life. Such Vegetarians we can put on no other footing than that of moderate flesh-eaters; and all the favorable experiences which they so loudly and confidently proclaim as due to their adoption of Vegetarian habits, we felt justified in carrying over to our own side of the question; since they demonstrate that a diet into which a moderate portion of animal food enters, is at least equally



conducive to health with one that is exclusively derived from the vegetable kingdom. We then exposed the inconsistency of these amiable enthusiasts, who would not hesitate to make an unrelenting crusade against fleas and bugs, cockroaches and black-beetles, cock-chaffer-grubs and wire-worms; but who think it a sin to rear animals in comfort and enjoyment, that their lives may be terminated by a procedure far less painful than the death to which the same animals in a state of nature would be subjected. And we pointed out some of the absurd fallacies into which those fall, who are so ready to lay a large proportion of the diseases of our time to the account of bad *meat*, whilst they entirely ignore the large admixture of bad *wheat*, which exists in most, if not all, of the bread supplied by the cheap baker to the working-classes.

We then applied ourselves to the fundamental question, which is not now raised for the first time, but which has been brought again and again under consideration during every period of the world's history; and which, by the zeal and assiduity of its present advocates, has been lately made to assume an importance, among certain sections of our community, which it never before presented. It was this which at first induced us to bring the matter under the consideration of our readers; and we now request their attention to a continuation of the inquiry, because we hope to show, that some beneficial results may accrue from the direction of their thoughts to the subject. The point to which we brought it in our former article was this:—After an examination of the evidence supplied by the physical and mental constitution of man, and his peculiar adaptability to varying conditions in regard to diet, as well as to climate, mode of life, &c., we came to the conclusion, that the Creator, by conferring upon him such an adaptiveness, had intended to qualify him for subsisting upon those articles of diet, whether animal or vegetable, which are most readily attainable in different regions of the globe; and thus to remove the obstacle which a necessary restriction to any one kind of food would have otherwise opposed to his universal diffusion. If we were to bring together the habitual diet-scales of the several races of men which people the surface of our globe, we apprehend, that the diversities which they would present, would be scarcely less strange than those which exist among the regimens of the most dissimilar species of Mammalia. We should find the purely animal-feeding on the one hand, the pure vegetarians on the other. Among the former we should find some who devour animal flesh, others fish, and others fowl, while others are even insectivorous; then, again, we should encounter some who devour their food raw, others who cook it; some preferring it immediately that it has ceased to live, while others do not relish it until it has become almost putrescent. So among the vegetable-feeders, we should find some subsisting upon soft fruits, others upon hard grains, others again chiefly upon succulent herbage, and others upon roots so tough as to require artificial means for their reduction. In the various devices by which man has succeeded in availing himself of these, and in the various tastes which have led some to avail themselves of articles of food which others would loathe, we see the evidence of the same wise Design, as that which has given to different tribes of animals their respective preferences; and we deduce from the whole the conclusion, that man is left by his Creator at perfect liberty to select that kind of nutriment which he finds most suitable to his tastes and to his wants; the former,

when not absolutely vicious, being (there is strong reason to believe) an exponent of the latter, just as the simple desire for food is the exponent of the need for it in the system.

We then entered upon the practical question, how far the teachings of experience indicate, that a true vegetarian diet is suited to the wants of man, and is preferable to the mixed regimen employed by all who can afford it in this country, save by those who abstain from animal food on principle. In considering this question, we cited a number of well-established facts, which seem to place beyond all doubt the capability of mankind (our examples being drawn from all the principal races), to attain a high grade of physical power and endurance, upon a purely vegetable diet. Whether the *greatest* amount of muscular strength which man is capable of putting forth, can be developed upon a vegetable regimen, has not yet been carefully determined; and it is certain, that our most powerful "navies" have performed their greatest feats, when supported by a diet including a very large amount of animal flesh, from 2½lbs. to 5lbs. of butcher's meat a day. But such an exclusive development of this one kind of power does not indicate, that a highly animalised diet is the best for the "average man;" for with him we have to consider the development of mental as well as of muscular energy; of moral as well as of brute force. And it cannot be urged, that the railway "navy" is remarkable either for the vigour and penetration of his intellect, or for the clearness or strength of his moral perceptions.

It has been affirmed by some, that although a vegetable regimen may serve to maintain the body in a vigorous condition, yet that it is not favorable to mental development; and they ground this inference upon the fact, that although the example of individual vegetarians may be quoted, who have been distinguished by their intellectual ability, or by the endowments of genius, yet that no nation or race has ever made itself remarkable for mental vigour, into whose habitual diet animal food has not entered. We cannot admit this to be an altogether valid argument (supposing it true in itself to the extent assumed) in favour of the necessity of a partly animal diet for the development of high mental power; since it would not be difficult to point out many other circumstances of difference, which have a manifest bearing on the result. One of the most important of these is undoubtedly *race*; for it has been from the great Indo-European family of nations, that nearly all the progress of civilization has taken place; and if we compare the several members of this family with each other, we certainly can find nothing like a constant proportion between their relative progress and the quantity of meat they respectively consume. Mr. Smith does not take notice of this discussion; but contents himself with citing proofs of the intellectual quickness and apprehension frequently observable among young persons of various nations, who have subsisted from their childhood upwards upon vegetable food. As an example of these, we may give Judge Woodruffe's account of the Greek school, containing from 200 to 300 pupils, which he several times visited at Syra:

"I very soon began to feel and express astonishment at the remarkable vivacity, sprightliness, and mental activity and power of these children. Their memory was truly surprising. Dr. Korke assured me, that he had never, in any country, found children equal to these for clearness, sprightliness, and power of intellect,—for aptitude to learn and ability to retain. And I can truly say, that these Greek

children manifested a capacity for learning, which exceeded anything I had ever before or have since witnessed. Dr. Korke attributed this extraordinary ability in his pupils, mainly to their habits of living, which were extremely simple. Coarse unbolted wheatmeal bread, with figs, raisins, pomegranates, olives, and other fruit, with water, constituted their diet. Figs and other fruit composed a large proportion of their food; but I am confident they did not consume an ounce of flesh-meat in a month." (pp. 266-7.)

In like manner he quotes the testimony of a gentleman who had interested himself in the instruction of young negro children in the West Indies, in favour of the remarkable aptitude for instruction of the little field-negro children, and the comparative slowness of those who attend upon their masters and are fed from his table. Now we can fully confirm, from our own personal experience, what is said of the aptitude for instruction exhibited by negro children; and having had similar experience of the capacity of the children of the working population in this country, we do not hesitate to say that the former are at least equal to the latter *up to a certain point*. We did not observe, however, any such difference as that affirmed to exist between field-negroes and house-negroes; on the contrary, in our own case, the house-negroes had decidedly the advantage; but the circumstances of the two are altogether so different, that we should not think of founding any inference as to the influence of diet, upon such diversities as might present themselves between them. The only fair test is where the trial is made upon two sets of individuals, placed under circumstances as nearly as possible identical in every respect but diet. Such a case seems to be presented by the experience of the Orphan Asylum at Albany (New York), containing 130 children, whose dietary was changed in 1833 from one which contained flesh or flesh-soup once a day, to a purely vegetable regimen. Three years after this change was made, the principal teacher in the Institution thus speaks of it:

"The effect of the new regimen on the intellectual powers of the children, has been too obvious and too striking to be doubted. There has been a great increase in their mental activity and power. The quickness and acumen of their perception, the vigour of their apprehension, and the power of their retention, daily astonish me. Indeed, they seem eager to grasp, with understanding minds, almost any subject that I am capable of presenting to them in language adapted to their years." (p. 266.)

It is a pity that no later information is given as to the continued working of this plan, both as regards the welfare of the institution, and the character of the young people who have successively quitted it. Supposing, however, that it were fully proved (and we must say that a great deal more evidence than that collected by Mr. Smith would be requisite to convince us), that a purely vegetable diet is more favorable than a mixed regimen to the development of the faculties of observation and apprehension, this does not give the least foundation for the assumption that it is equally favorable to the development of those higher intellectual faculties, which are only unfolded as adult age comes on. Many boys who are "quick" at school turn out very "slow" men; and some of our most distinguished men have been dunces at school.

Now, if we were inclined to generalize as hastily as we consider Mr. Smith and his vegetarian friends to have done in this matter, we might quote not only our own experience, but that of many who have had more extensive opportunities of observing the negro character than we have

enjoyed,—to the effect that the evolution of the faculties in most negro children seems to be prematurely arrested, so that the adults are nothing else than “children of a larger growth;” and ~~as~~ might hence argue, that a vegetable regimen is decidedly unfavorable to the development of the higher intellectual powers. Such a conclusion would obviously be utterly untenable, since no fair inference could be drawn from any comparison between beings so differently conditioned in every respect, as are Negroes and Europeans or Anglo-Americans. The only fair test,—and no *single* experiment even of this kind would have much value,—would be to bring up two sets of negro children, the one on a purely vegetable diet, the other on the ordinary mixed regimen, to educate them in precisely the same manner one with another, and to compare the result. And even then, should it be proved that the constitution of negroes, descended from a race that manifests a special adaptation to a warm climate, is more favorably developed on a purely vegetable than on a mixed regimen, it does not follow that the same rule holds good with regard to the Anglo-Saxon or any other of the European races, which have a constitutional adaptation to a temperate climate.

The Vegetarian advocates, in all ages, have laid stress on the tendency of a flesh diet to excite the animal passions and propensities, and on the contrary tendency of a purely vegetable regimen. We cannot help thinking that a great deal which has been said and written upon this subject has had its origin in the absurd notion, that the carnivorous animal is ferocious *because* it lives upon animal flesh, and that the comparative mildness and docility of the herbivorous quadruped are dependent upon its vegetable diet. Let us put the case the other way, and say that the carnivorous animal eats animal flesh because it is ferocious, i. e., because the Creator has implanted in it the instincts which are necessary to its acquirement of the food for which its whole organization is destined; and that the herbivorous quadruped was rendered destitute of the like savage nature, because it was intended to derive its food from the produce of the soil; and we shall perhaps come nearer the truth. But even if we were to admit that the habitual diet reacts upon and tends to keep up the respective propensities of these two classes, there is ample evidence that there is nothing in a vegetable diet which necessarily induces mildness and docility. The Vegetarian's ideas of herbivorous animals appear to be formed upon our domesticated races of sheep and oxen, in which, be it remembered, the castration of the males most effectually destroys one of the propensities which is elsewhere productive of the greatest excitement. But did Mr. Smith and his friends ever chance to come in the way of a pugnacious bull? or did they ever hear of the ferocity of wild cattle? or of bisons or buffaloes? or of wild boars? or of rhinoceroses? or of enraged elephants? or of rutting deer? Did it ever occur to them that the duck, on their theory, must necessarily be a fiercer bird than the fowl; and that sporting gentlemen are quite wrong in their preference for fighting-cocks, and should turn their attention to *drakes* as the better suited to their purpose? When arguments of so unphilosophical a character are employed, as those to which we have adverted, it is fair to meet them in their own kind. We are far from questioning the influence of diet on character, but we affirm that it is not what it is asserted to be; and that the effects ascribed to “high feeding,” are really referable, not so much to the source of the

food, as to its quality, and to the stimulating liquors which commonly form a part of the regimen. Those who are actually suffering from its injurious influence, may very probably (as we shall presently show) be decidedly benefited by the adoption of a purely vegetable regimen; but we cannot see the least ground for the conviction that a man in good average health, with no plethoric excitability, will be in the least changed for the better by relinquishing his slice of mutton and potatoes for its equivalent in wheat flour, still less, by substituting for it an omelet and a custard pudding. And if the effect of universal Vegetarianism were to be to reduce the characters of all mankind to the insipidity of the said omelet and the blandness of custard pudding, we for our own parts should not like the world half so well as we do now. A very excellent lady, who had kept a school for nearly half a century, was accustomed to say in our hearing:—"I never liked the girls who were brought to me with 'very good' characters from their parents; they had either no energy, or were very sly. Give me the 'naughty' children; there is something in them to work upon; and a promise of future activity." All metaphysicians agree in representing the emotions and propensities as the sources of all *action*; and if these be tamed down to the Vegetarian standard, we apprehend that neither will the better parts of human nature be called forth into energetic operation by their own activity, nor will the worse call forth that energy for their repression, which is often the foundation of what is noblest in the human character.

The next question which we shall consider is, whether there be any differences of climate and temperature, which render one regimen preferable to another. This question is very coolly disposed of by Mr. Smith:

"The numerous references already made to men living on vegetable productions, in all climates, whether hot or cold, and engaged in all kinds of occupations, and yet enjoying health and strength, may be regarded as a sufficient refutation of the opinion that human diet should vary with the climate in which a man resides. It is true that a diet of animal food will agree much better with a person living in a cold climate, and taking a considerable amount of muscular exercise, than with one residing in a hot climate, and leading an inactive life; but it is also equally true, that a diet of fruit and farinacea is conducive to the highest and most complete development of man physically, mentally, and morally, in cold countries as well as in hot." (pp. 183-84.)

The only evidence which he has adduced in support of this last assertion, is that which we have already quoted in regard to the Russians, a similar statement in regard to the Norwegians (who, however, eat milk and cheese with their rye-bread), and the assertion (of which the precise authority is not given), that "gentlemen who have spent many months in Siberia affirm, that no exiles to that wintry region endure the severities of the climate better than those who have been all their lives accustomed to a simple vegetable diet." Now, on the other hand, we have the well known fact, that the nations who inhabit the *most* northerly regions, and who are therefore exposed to the *greatest* severity of cold, are almost exclusively supported upon an animal diet, and that they do not believe themselves able to resist the extreme of cold upon any other. This is the case, not only with all the nations of Mongolian descent, which inhabit the shores of the Arctic Sea, but also with the Indian races that roam over the coldest portions of the North American Continent, with the Canadians and half-breeds who adopt the same life, and with the European

traders whose stations are in the same latitudes. It is well known, also, that European travellers who have ventured into these inhospitable regions, have experienced the insufficiency of farinaceous food, and a craving for animal flesh and fat, which removed all sense of wonder at the enormous amounts consumed by the residents in these countries. Thus Mr. George Combe mentions (in his work 'On the Constitution of Man'), that he was informed by Dr. Richardson, after his return from his expedition along the Polar Sea with Capt. Franklin, that the effect of the extreme *dry* cold to which he and his companions were constantly exposed, was to render bread not only uncared for, but comparatively impotent as an article of diet; that pure animal food, the fatter the better, was the only sustenance that maintained the tone of the corporeal system; and that when this was abundant, a delightful vigour and buoyancy of mind and body were enjoyed.

How, then, are we to reconcile these conflicting assertions? We believe that the means of doing so are very easy, and that from a discriminating examination into the facts, some very important dietetic principles may be deduced. In the first place, we can see no reason for the supposition that the demand for azotised nutriment is increased, *ceteris paribus*, by exposure to a low temperature; or that if vegetable food be adequate to supply it in temperate or tropical regions, it should fail to do so in arctic. What is essentially needed is combustive material, of a kind to generate the largest possible quantity of heat; and a comparison of the chemical constitution of farinaceous and oleaginous substances will at once show why the latter should be much more efficacious as calorifying agents than the former. In the farinaceous and saccharine compounds, the oxygen and hydrogen already stand to each other in the relation of the atoms of water, so that the oxygen required to convert starch or sugar into carbonic acid and water will be only that which is needed to burn its carbon. On the other hand, the fatty matters contain very little oxygen, and have a large proportion of uncombined hydrogen; in *their* combustion, therefore, a large quantity of oxygen is consumed, in addition to that which is required to convert the carbon into carbonic acid; and it is in the combustion of *hydrogen* especially, that *heat* is given off. We can understand, therefore, why bread and other simply farinaceous substances should be found inadequate to sustain the extremity of cold; and why recourse should be had, in such large quantity, by a sort of natural instinct, to oleaginous meats. The Russian soldier has oil allowed as part of his ration with his black bread; and if our notion be correct, a sufficient supply of oleaginous substances, in a state fit for being digested, would have a similar value on the shores of the Icy Sea, whether the oleaginous matter proceed from the animal or the vegetable kingdom. The craving for animal food, mentioned by Dr. (now Sir John) Richardson, would seem, on this view, rather to result from the feeling of necessity for oleaginous matter, than from any preference for it *as* animal; and if a sufficient quantity could be supplied in vegetable food, in some *natural combination* (for we do not think that drinking a tumblerful of vegetable oil per day would answer the purpose dietetically), the same support would probably be given. Now we are enabled to cite the same high authority in support of the fact, that the servants of the Hudson's Bay Company are now finding out by experience, that although *wheaten* bread does not give them adequate support, bread composed of *maize* flour (which contains a large

quantity of oily matter) answers every purpose; and that two pounds and a half of this flour is fully equal, or even superior, in sustaining the capacity both for muscular exertion and for bearing cold, to the eight pounds of fat meat, which constitutes the usual allowance. We are inclined, then, to believe, that a purely vegetable diet, if it contain a due proportion of oleaginous matter, is capable of maintaining the physical powers of the body at their highest natural elevation, even under exposure to the extreme of cold; and that there is no proof that an exclusively animal or even a mixed diet possesses any advantage in this respect, that is to say, when we are concerning ourselves with bodies of men, and not with individuals. It seems to us probable, however, for reasons which will hereafter appear, that there are cases in which cold and muscular exertion may be best sustained upon a partly animal regimen; and that these are more numerous than those in which a purely vegetable regimen has any positive advantage.

For the very same reason that oleaginous food is needed, and farinaceous aliment is insufficient for the support of life, within the Arctic Circle, farinaceous matter is preferable to oleaginous, as the accompaniment to albuminous compounds, in the diet of those who live under a tropical sun. We do not see that any difference arises out of variety in climatic conditions, in regard to the demand for azotised aliment, or the source from which it may be best obtained; this demand being entirely dependent, so far as we at present know, upon the amount of exertion put forth; and the supply being as readily obtainable from one kind of food as from another. But farinaceous substances have this great advantage, as the staple article of diet, in tropical regions; that while they afford an adequate supply of azotised aliment, they give to it that *bulk* which the digestive apparatus seems to need, by means of the admixture of a substance whose heat-producing power is low. It is not a little surprising, however, to find the inhabitants of many tropical regions mainly supported upon articles, such as rice, in which the proportion of albuminous matter to starchy compounds is very small; so that, in order to obtain a sufficient amount of the positive nutriment, an enormous amount of the heat-producing material is taken into the system. Their case is not altogether the same as that of the Irish with regard to their potatoes; for although they too are forced to eat a very large bulk of that vegetable, if it serve as their sole aliment, to derive from it the material requisite for the support of their bodily frame, yet a large part of the starchy constituent is disposed of in maintaining the heat of the system, which cannot be the case with the Hindoo, who lives in an atmosphere not far below the temperature of his own body. We suspect the fact to be, that a large proportion of the starchy constituent of the food passes out of the body again in an unchanged state; and that only so much is digested by the secretions of the alimentary canal, as is required by the wants of the body. There would not seem to be that danger from excess in the use of farinaceous substances in warm climates, that attends an oleaginous and highly azotised regimen; for if this latter regimen be too freely used, the quantity of aliment appropriated by the system is too great, and the respiratory process carries off but a small proportion of the superfluity, instead of being (as in cold climates) its great emunctory. Hence it appears to us that the superiority which there is some reason to attribute to a farinaceous regimen over the

ordinary mixed diet of Europeans, in tropical climates, is not due to the circumstance of its being vegetable rather than animal, but to its containing the requisite amount of azotised matter, largely diluted with a substance in itself innocuous, from which the system takes up just that which it requires to sustain its heat, and no more. The great evil of the ordinary diet of Europeans seems to be, not in its animal nature,—for if it were so, the Guachos of South America, who are exclusively kreatophagous, must be rapidly killing themselves,—but in the too large proportion of azotised and oleaginous materials which it contains. And that this is the case, we think, appears from the experience of those who, having been very moderate in their diet, and abstinent in regard to fermented liquors, have preserved their health in India, and have arrived at a good old age notwithstanding that they did not adopt the vegetable regimen. Further, it is found that vegetable food of a very oleaginous character is too heating for use in warm climates; thus even maize-flour could not be employed as the staple article of diet, unless with the accompaniment of fruits or farinacea of a non-oleaginous character, even in the warmer parts of the temperate zone, still less between the tropics. We come, then, to the conclusion, that in hot latitudes, as in cold, the question of superiority does not lie between the vegetable and the mixed regimens *per se*, but has reference entirely to the demand for heat-producing material, and to the advantages of dilution of the proper aliment by farinaceous matter. A man who eats six or eight ounces of meat and a pound of rice daily, is in very much the same position as a man who eats a pound and a half of bread; and we do not yet see any positive reason to believe that the one diet is less beneficial or more dangerous than the other, even in hot climates.

We have carefully considered the charges which are brought by Messrs. Smith and Graham against the use of animal flesh as an article of diet; and without going into details, we may say of them all, that they are true only of *excess*, and do not affect the moderate use of it. The case is altogether different from that of alcoholic liquors; for in regard to these, we hold it to be clearly proved that they are not nutritious (except as contributing to the maintenance of animal heat), and that whatever effect they produce on the healthy system, is a disturbance of its natural course of action, whether a small or a large quantity be ingested. The question of animal flesh, *versus* fruits and farinacea, on the other hand, is simply which of the two is *best* adapted to support the human frame; *both* being found by the experience, not only of the lower animals but also of man, to supply the alimentary material required. Now we freely admit that among the middle and higher classes, there is usually a certain degree of over-feeding. There are very few who restrict themselves to the quantity which nature absolutely requires; and we believe a considerable proportion of their diseases to arise from habitual excess, though this may be to a very small amount. Such excess is most common in summer, when the system naturally requires the least amount of food, but when few persons make a proportionate diminution in their usual allowance, and when many, who feel the external warmth invigorating to their systems, positively increase it. We have a strong suspicion that the “bilious attacks,” so common in the early autumn, and usually laid to the account of fruit, are really the result of the charging of the system with a surplus of hydro-carbonaceous matter, and are an effort of nature to get rid of it. The public needs much to be enlightened upon the operation of this cause of



disease; and we are not by any means sure that it is duly appreciated by the profession. Many persons are instinctively led to make that variation in their diet, at different periods of the year, which brings it into correspondence with the habitual diet of the dwellers in arctic and equatorial regions respectively. Thus we know many who, in the winter, have an amazing relish for a slice of fat bacon at breakfast, and for a cup of cocoa at night, yet can touch neither the one nor the other when the weather becomes warm; and some of these very individuals experience a craving for acid fruits in the summer, and an entire disrelish for animal food, which makes them practically vegetarians during that season. Now we believe that this is the true theory of diet in climates subject to great diversities of temperature; in winter to make use of oleaginous articles,—whether derived from the animal or vegetable kingdom, we do not think is of any great consequence; and in summer to make “fruits and farinacea” the staple article of diet, adding thereto a quantity of animal food if the appetite prompt to it, and much active exertion be used. We know those who adhered to this diet, in spite of the denunciations of the Board of Health, during the last cholera epidemic; and who were perfectly free from ailment throughout the whole period of its prevalence.

The *stimulating* properties of animal flesh are continually dwelt upon by the vegetarian advocates, as if they were fully demonstrated; and all the arguments against the habitual use of stimulants, are directed against the practice of flesh-eating even in moderation. Now we cannot see the least proof of this. The effect of habitual “high feeding” is universally admitted to be a state of plethora; in which the solid constituents of the blood, and especially the red corpuscles, are above their normal average. The usual diet of the “high feeder” contains by far too large a proportion of the oleaginous and nitrogenous constituents; and alcoholic stimulants also generally form part of it. It seems to us quite unfair, therefore, to saddle upon animal flesh that which is really the fault of excess in the use of compounds, which *might* be derived from the vegetable kingdom. We admit that such excess is far less likely to be practised by the vegetarian; and we look upon it as one of the most valuable properties of a well-selected vegetable diet, that, on the one hand, it affords less temptation to general excess, whilst, on the other, the azotised and oleaginous materials are so *diluted* with the farinaceous, that even if the stomach be filled, and the feeling of repletion arrived at, there is comparatively little surplus except of starchy matter; and of this no more seems to be appropriated, than the system can make real use of. Such, it appears to us, is the real source of the advantage which a well-selected vegetable diet possesses, for such individuals as are disposed to excess in eating, and who are liable to suffer from its results. To those who have self-control enough to confine themselves to the allowance of food which the natural appetite dictates, we cannot see that there is any sufficient cause for restricting themselves in regard to the source of that food, provided that a due proportion is maintained between its principal components. Be it observed that all the praises of the Vegetarian system, which are at present being sounded through the country by its disciples, apply to a regimen of which animal albumen derived from eggs and milk forms as large a part (as we have already shown) as it does in the diet of an ordinary flesh eater; and it necessarily follows, therefore, that the asserted benefits of their regimen do not really depend upon the avoidance of animal nutriment, but upon mode-

ration in the use of it; so that it has no real advantage over an equally moderate regimen, of which flesh meat should form a similar proportion.

Great stress is laid, also, upon the comparative freedom of vegetarians from epidemic diseases; but here, too, the proof entirely fails that it is the abstinence from animal food which is the source of the immunity. In most of the cases cited, the epidemics occurred in hot climates, or during the summer in the warmer temperate latitudes, such as New York, and Philadelphia, where the temperature is then nearly tropical. Under such circumstances, we think that there may be a suitability in a vegetable diet to the human constitution, which places it in a condition favorable to its power of resisting the epidemic poison; but no proof of this is supplied by the instances quoted; since it is to be recollected, that the *voluntary* disciples of Vegetarianism are for the most part individuals in easy circumstances, living in well-ventilated and well-drained habitations, abstainers (commonly, if not invariably,) from fermented liquors, moderate in their use of food, and attentive to the conditions of health in every way. Now we have not the least question that any like number of individuals, similarly circumstanced in all other particulars, but using animal flesh in moderation as an article of diet, would enjoy a similar immunity; and until it has been proved that such is not the case, the advocates of Vegetarianism have no right whatever to take credit to their system, for their asserted freedom from the attacks of epidemic diseases. How these assertions are to be reconciled with the experience of the potato-eating peasantry of Ireland, they do not attempt to explain; certainly the people of that unfortunate country would seem to have a special liability to epidemic fevers, altogether irrespective of the influence of occasional famine. This fact, on the other hand, has been raised into what seems to us undue importance, by the opponents of Vegetarianism, who have argued, from the prevalence of epidemics in Ireland, that a vegetable diet does not afford the means of resisting zymotic poisons, in an equal degree with an animal regimen. We do not for a moment doubt, that the diet of the Irish peasantry is one main source of their peculiar susceptibility; but it is manifestly unfair to charge the whole of this upon diet alone, when the filthy, ill-ventilated condition of their habitations unquestionably contributes largely to the result; whilst it is alike unfair to set down as chargeable against a vegetable regimen, that which may be fairly attributed to the poverty of a potato-diet in regard to the essential elements of nutrition. Everyone knows that the Scottish peasantry, whose chief support has been oatmeal, for a longer time than that during which the Irish have mainly depended on potatoes, are not more liable to epidemics than can be fully accounted for by the conditions of their dwellings, their want of cleanliness, and other bad habits. And we regard it, therefore, as yet to be shown, that there is any difference in such liability, between those who support themselves upon a *nutritious* vegetable diet, and those who make a moderate use of animal food, the other conditions being the same.

On the whole, then, our conclusion is, that whilst the advocates of Vegetarianism are perfectly justified in asserting that a purely vegetable diet,—if judiciously selected, so as to contain the due proportion of the azotised and non-azotised constituents,—is adequate to the sustenance of Man under every variety of circumstances, and that it is peculiarly adapted for residents in tropical climates, and for the inhabitants of

temperate regions during the heats of summer, they are altogether unsupported in the assertions which they make, in regard to the decided advantages of their regimen over a mixed animal and vegetable diet. It is to be remembered, on both sides, that the comparison is not fair, unless the equivalents of the azotised and non-azotised constituents be the same in the two diet-scales. The vegetable diet must not be judged of by potatoes and greens, but by that which mankind have agreed to call "the staff of life," in which, more than in any other article of human diet, the elements are mingled in the proportion most adapted to the usual wants of the system. Thus taking wheat as our standard, we find that the azotised is to the non-azotised constituent, in the proportion of about 1 to 3; and that about two pounds of wheaten bread is generally sufficient for a man taking ordinary exercise. Now if instead of this, a man takes but one pound of bread, and makes up the constituents of the other pound by an equivalent of meat and potatoes, the dietetic result will be the same; and no adequate proof has been given by the Vegetarian advocates, that the effect upon the constitution will be essentially different. We believe that the Creator intended Man to make use of such food as his tastes and circumstances might dictate; and that he is at full liberty to avail himself of the flesh of the animals which were placed under his dominion, provided that he do so with judgment and moderation.

Into the economical part of the question we shall not enter, since it is altogether beyond our province. With even the smattering of agricultural knowledge to which only we can lay claim, we can perceive many fallacies in Mr. Smith's tables; and in his whole discussion of the subject, he seems to assume that the land at present in pasture might be employed equally well to grow corn, and that corn might be grown without a rotation of green crops, such as are adapted only for the feeding of cattle. We must leave it to the practical agriculturist, however, to settle with Mr. Smith as to the possibility of such a system; which is a matter, however, of no mean social importance. For at present it seems a doubtful question whether corn will be any longer grown in this country at such a cost as to compete with foreign importations, except upon lands which are peculiarly adapted for its production; and whether the British farmer's main support will not henceforth lie in his cattle-feeding and green crops. Considerations such as these have no bearing upon the physiological aspect of the question; but they are of essential importance in the discussion of the economical arguments in favour of the Vegetarian system. To the agricultural economists, however, we shall leave it to set Mr. Smith right upon this point.

It now only remains for us to consider the last branch of our inquiry, which we must do very briefly, not only on account of the length to which this discussion has already extended, but also because the data upon which we can conduct it are as yet very slight. We had hoped to derive a good deal of new material from the treatise of M. Marchand; but have been grievously disappointed. The point on which he chiefly dwells, is the tendency of an animal regimen to augment, and of a vegetable regimen to diminish, the proportion of red corpuscles in the blood; and the consequent propriety of having recourse to the former in those cases in which the proportion of red corpuscles is too low—which state he designates as *aglobulie*; and of resorting to the latter in which it is too high—which

condition he terms *polyglobulie*. From the analyses which he gives, it would certainly appear that the proportion of red corpuscles may be varied by diet, more than that of either of the other constituents of the blood. It is not yet clear to our minds, however, that the decrease effected by the change of diet in *polyglobulie*, or as we should call it *plethora*, is due to the substitution of vegetable for animal nutriment, so much as to the diminution in the quantity of azotised food, which is involved in the change, and to the general lowering of the regimen. So in the cases in which an increase of red corpuscles was ascertained to take place, we have not merely to consider the influence of animal food in place of vegetable, but to take into account the more generous and stimulating regimen which formed part of the treatment. The greater part of M. Marchand's treatise is devoted to the exposition of his views on chlorotic and hysterical disorders, all of which he sets down to the account of *aglobulie*. In this we think that he goes far beyond what his facts will justify; for although in the greater number of cases of hysteria, the proportion of red corpuscles may be below the average, yet it does not follow that this is the essence of the disorder; or that if by generous diet and other adjuncts this condition be removed, the patient will be necessarily cured. A large proportion of the cases cited by M. Marchand are obviously examples of the mischievous proceedings of practitioners of the Broussaist school; who by starving, leeching, and other orthodox methods, had been combating a supposed chronic gastritis, and had been producing a real *aglobulie*, with all its distressing consequences. In such cases there can be no doubt of the marvellous results which will follow the change to the opposite method; or of the benefits of the most nutritious diet that the system of the patient will bear.

In the adoption of an animal diet, however, it is of the utmost importance that fresh vegetables be not altogether excluded; at least if the regimen is to be prolonged for a considerable time. We are led to make this remark the more strongly, by a circumstance which has recently come under our observation. A gentleman who has been for some years a severe sufferer from dyspepsia, and who has of late placed himself under the care of a homœopathic physician and surgeon, has been progressively cutting off one article of vegetable food after another, on account of its tendency to generate lactic acid; until at last he has brought himself to live altogether on bread and meat. The consequence of his persistence in this regimen for some months is, that at this moment he is suffering under an unmistakeable attack of *scurvy*; and it is not unworthy of note, that so little were his homœopathic attendants acquainted with this disease, that they ignored its symptoms altogether, and regarded them as merely the manifestations of general debility. The almost certain occurrence of this disease, when the human body has been too long kept without fresh vegetable food in one form or another, is an obvious indication that man was not intended to feed either upon flesh alone, or upon flesh with the addition of bread. We do not know whether *scurvy* ever manifests itself among the Guachos of South America, whose diet is as exclusively animal as that of any people we are acquainted with; but their case would form no exception to the general rule, even if this disease should prove to be unknown amongst them, until it shall have been ascertained that they *never* have recourse to fresh vegetables. The free and active life they lead is most unfavorable to the production of *scurvy*, which is well known to make its appearance much more readily among those who are suffering

from confinement, bad ventilation, and depression of spirits. And it is probable, therefore, that the occasional use of a very small amount of fresh vegetable food, would exert a sufficient prophylactic power, when all other circumstances concur to keep the disease at bay.

We are strongly inclined to believe, however, that an insufficient supply of *fresh* vegetable food is a not unfrequent cause of disordered nutrition, even though that disorder may not manifest itself in the form of genuine scurvy. It seems to us unquestionable, that if the total withdrawal of this article of diet is productive of such a fearful depravation of the blood, as perverts every function to which the blood is subservient, a diminution of it below the standard requisite for the maintenance of health, must necessarily involve a depravation similar in kind, though less aggravated in degree; and this, if slight, may be expected to manifest itself, not so much in the production of idiopathic disorders, as in favouring any peculiar tendency to disease which may exist in the system, and in preventing or retarding recovery from its effects. This "scorbutic tendency" was fully recognised by our past generation of physicians, who practised in those "good old times" when potatoes were a luxury, and green vegetables in the winter almost unknown, when the middle classes fed upon salted meat during a great part of the year, and when sagacious old women prescribed nettle-tea and scurvy-grass with a course of lenitive "spring physic" for the "cleansing of the blood." And it is worthy of remark, that in the times when even the wealthy lived during four or five months of the year almost exclusively upon bread, meat, and flour puddings, and when, therefore, the diet was far too highly azotised, arthritic and calculous disorders were much more common and severe than they are now. The introduction and universal employment of the potato has done much to correct these two tendencies; on the one hand, by supplying to the blood some element which is needed for the maintenance of its healthy condition; on the other, by diluting the azotised components of the food, so that with the same bulk a much smaller proportion of these is now introduced. But although potatoes alone may suffice to keep scurvy at bay, we doubt if the blood can be maintained in its highest state of purity without the use either of fruits or of fresh or green vegetables; and we would especially suggest the employment of these, in cases where a general disorder of nutrition indicates a perverted condition of the circulating material, and especially where there is a disposition to chronic inflammation, induration, and ulceration, in different parts of the body. The two following cases, though narrated by professed vegetarians, ought not on that account to be passed over as unworthy of notice; but carry with them, as it seems to us, a most important lesson. We cite them as narrated by Mr. Smith.

"Dr. Joshua Porter, of North Brookfield (U. S.), says:—'I have been called to prescribe for a man who has been a flesh eater for more than half a century. He was confined to his house; and had been losing his strength for several months;—still keeping up his old habits. The disease which was preying upon him was chronic inflammation of the right leg: the flesh had been so long swollen and inflamed, that it had become hard to the touch. There were ulcers on his thigh; and some had made their appearance on his hip. This disease had been of seven months' standing; though not in so aggravated a form as it now appeared. After examining the patient attentively, I became convinced that the disease which developed itself locally, was of a constitutional origin; and, of course, not to be cured by local remedies, which had been applied for the period above mentioned. All local applications were discontinued; and the patient was put on a vegetable

diet, after the alimentary canal was freely evacuated. I saw this man three days afterwards: the dark purple appearance of the leg had somewhat subsided; the red and angry appearance about the base of the ulcers was gone, and his strength improved. Three days after I called, I found him in his garden at work. He is now—two weeks since my first prescription—almost well. All the ulcers have healed, with the exception of one or two.

"But the most remarkable cure of this kind, is recorded in the 'Lancet' for May 14, 1842, by Mr. S. Rowbotham, Surgeon, of Stockport. The son of a Mr. Fielding of that town, about three years old, had been ill eighteen months. He was covered, from head to foot, with ulcers: his eyes, nose, ears, mouth, and, in fact, his whole head and face, were involved in one complete mass of fetid running sores and ulcers; and the lower part of his body was equally bad;—so that his little thighs seemed nearly separating from his body. For more than twelve months he had been quite blind; and had never been able to sit down, even on a pillow; but stood upon his foot, and leaned with his elbow upon the nurse;—except at times, when he was able to kneel on a pillow: he had scarcely been able to lie in bed for the same period. Eight of the most eminent medical men had given him up as incurable; and some of them declared, that no known mortal power could even *improve* his condition, much less effect a cure. 'From certain views which I held on the origin of disease,' says Mr. Rowbotham, 'I was induced to recommend a diet consisting almost entirely of ripe fruits and honey, or sugar and treacle. The child commenced this diet on the 13th of September, 1841: he had stewed fruits, mixed with sugar or honey, to all his meals; and was allowed frequently to eat grapes, cherries, plums, apples, pears, and such other fruits as could be obtained. On the 16th, the sores on his back were beginning to disappear; on the 23d, he was very sensibly improved; and on the 30th, one half of his face was clear; the lower parts of his body were much better; and he could sit in a chair, and lie comfortably in bed. He continued daily to improve, till at last his eyes opened; but they were at first very weak, and he could scarcely see any thing: his sight, however, gradually improved. On the 1st of January, 1842, not a single ulcer remained on his body: the skin became remarkably clear and fair; and the features—which, for twelve months, had been in such a state that it was impossible to do more than guess at the position of his nose and eyes—were restored to their wonted appearance.'" (pp. 231-2.)

Now we do not attribute the disorder, in the first case, to the eating of flesh; but to the want of a proper proportion of fresh vegetable food, which had brought about a condition closely allied to the scorbutic. In the second case, we have no indication of the cause of the disease; but from the results of the treatment, we should have little doubt that it must have been of the same nature. We question the advantage of the sugar and honey, which were so largely employed in this case; and should be disposed to attribute the beneficial results of the dietetic treatment to the large proportion of ripe fruits on which the patient subsisted. We recollect having our attention much attracted to this case at the time of its publication, and wishing that Mr. Rowbotham had more fully explained his views "on the origin of disease," which had induced him to recommend this system.

We are strongly impressed with the belief that *fruits*, as well as *farinacea*, ought to form a part of the ordinary diet of man; and we are disposed to think, that from a neglect of this combination much evil has arisen. An unduly large proportion of farinaceous matter certainly predisposes, especially in persons of weak digestive power, to the lactic acid diathesis, and to rheumatic complaints; and we think, that the gradual displacement of the arthritic by the rheumatic diathesis, which the experience of our older practitioners, and the medical writings of the last

century, indicate to have taken place within that period, has a close relation to the change of diet to which we have already adverted. Now if Dr. G. O. Rees's theory be correct, the use of citric acid, and (in a less degree) of all the ordinary vegetable acids, as the malic, tartaric, and racemic, tends to antagonise the results of the generation of lactic acid, and to prevent it from exerting an injurious influence as a *materies morbi*. From the additional experience which we have gained on the curative properties of lemon-juice, since our notice of Dr. Rees's pamphlet (Vol. IV, p. 529), we are confirmed in belief that it is a valuable adjunct in the treatment of some forms, at least, of acute rheumatism, although we should seldom be disposed to trust to it alone. But we are further convinced that it is a most valuable prophylactic; and that many individuals who are liable to suffer from chronic rheumatism, especially during the winter season, may have recourse to it with great advantage. It is further remarkable that, among such individuals, we have not unfrequently found that a positive craving had existed for acid fruits; but that they had not thought it right to indulge in them, for fear of aggravating the tendency to "acid in the stomach," which already existed. We are strongly inclined to believe, that where such a craving has reference to some simple and innocuous article of diet, it may be trusted to as an indication of the benefit which that article will yield; of this we have no more remarkable example, than the eager desire which is manifested by scorbutic patients for fresh vegetables of almost any kind. There are very few persons who have not a positive liking for fruits; and we are sure, that were they made a more regular component of the diet, instead of being merely introduced, as they usually are, into the pies and puddings which are added as a superfluity to a dinner already sufficient, or coming on as dessert when the stomach is already loaded, they would exert a wholesome influence on the system. And we would especially suggest their employment in cases of aggravated dyspepsia, where ordinary farinaceous food and green vegetables seem to aggravate the complaint, and where (as in the case to which we just now adverted) there is a danger of producing a scorbutic diathesis by the too exclusive limitation of the diet to animal flesh and bread. We are by no means sure, indeed, whether the entire dietetic treatment of dyspepsia, ordinarily practised, is not fallacious; and whether, instead of a highly animalised regimen, it would not be preferable to have recourse to a simple vegetable diet. Mr. Smith has collected several cases of the benefits of such a system from the writings of eminent medical authors, who had no particular doctrines to support, such as Abercrombie, Cheyne, and Thackrah; and from the considerations we have already adduced, we think that a strong case has been made out in its favour. We would not advise the large employment of substances consisting of almost pure starch, such as potatoes and rice; but we would give a fair trial to bread and fruits, before having recourse to so unnatural a proceeding as the abandonment of a regimen, upon which mankind in general thrives so well as to prove its adaptation to the human constitution, for one which can only be consistent with health under circumstances altogether peculiar.

There is another element, to whose presence in the food we attach considerable importance; we mean the *oleaginous*. It is provided in large quantity in the first aliment prepared by nature for the offspring of the Mammalia; and it exists largely in the yolk of the egg of all oviparous animals. In the ordinary diet of every nation on the globe,—whether

this be animal, vegetable, or mixed,—we find one or more articles of an oleaginous nature; and there is a natural craving for such substances when withheld, which indicates that they serve some necessary purpose in the system. Although this craving is so far affected by climate, that it leads (as already pointed out) to the largest consumption of oily matter where the extreme of cold has to be endured, it exists with no less intensity even in tropical regions; and we find the Hindoo enjoying his modicum of “ghee,” or rancid butter, with as much relish as the Esquimaux feels for his massive lumps of blubber. The healthy and vigorous body would appear able to exercise, to a certain degree, a converting power over farinaceous or saccharine substances, so as to metamorphose them into oleaginous; but this power obviously has its limits; and it would seem to be lost with a very slight depression of vital energy. There is strong and increasing reason to believe, that a deficiency of oleaginous matter, in a state fit for appropriation by the nutritive processes, is a fertile source of diseased action, especially of a tuberculous character. The beneficial results obtained by the free use of cod-liver-oil (about which there can be no question, although there may be some exaggeration), and the extraordinary immunity from scrofula enjoyed by the Icelandic population, notwithstanding the concurrence of every one of the circumstances usually considered favorable to its development (see Vol. V, p. 456), seem to us to afford strong evidence in favour of the views long and perseveringly advocated by Professor J. H. Bennett,—that oleaginous matter is required concurrently with albuminous in the very first acts of cell-growth; and consequently, that its presence in the food in a form capable of ready assimilation, is favorable to the conservation of health, if not absolutely essential to it. Of the conditions under which saccharine and farinaceous matters can, and cannot, be converted into oleaginous food, we as yet know very little; but it is clear enough, that the possibility of this metamorphosis does not furnish any reason for the entire omission of oleaginous matter from the food, although it may act on emergencies in supplying a deficiency that would otherwise exist.

The special tendency of a diet into which animal food enters, to raise the proportion of red corpuscles in the blood, and of a vegetable diet to reduce them, has an important bearing upon various questions of medical treatment. It seems to us by no means improbable, that the gradual change of regimen to which we have already referred, has had much to do with that change in the type of inflammatory complaints, to which all our older physicians bear testimony. It is universally admitted, that active depletory measures are not now borne as well as they were thirty or forty years ago; and that large bleedings, in particular, are comparatively seldom beneficial. Now we find it to be the experience of those, who have witnessed the treatment of inflammatory diseases in countries where an almost purely vegetable regimen prevails, that large bleedings are not merely undesirable, but that they cannot be borne; the patients being as completely prostrated by the loss of eight or twelve ounces of blood, as a well-fed English artizan would be by the abstraction of thirty or forty. This is easily to be understood. From the time when the analyses of Andral and Gavarret first opened our eyes to the results of bloodletting upon the constitution of the circulating fluid, it has been known that the influence of the loss of blood is exerted, not so much in diminishing the fibrinous or the albuminous constituents, as in lowering the proportion of the red cor-



puscles. Now if the proportion of these be already lower in such as make use of an exclusively vegetable diet, than it is in those who make use of animal food, it is obvious that the former will be less able, than the latter to bear a further reduction. We do not see, however, that it hence follows, that they should be worse subjects for treatment; on the contrary, if a smaller depletion will produce the same result on them, that a larger one does upon others, it must be regarded as just as efficacious. If one man is purged by half the dose that is required to affect another individual in the same degree, we do not see that the former is under any disadvantage. And it would seem to be most assuredly in the former class of cases, that inflammatory diseases, if left to themselves, have the greatest tendency to a spontaneous cure; whilst it is in such systems as have been habitually kept up to an unnatural degree of vigour, that the greatest mischief is done by the rapidity and violence of the inflammatory action, and that the benefit of judicious treatment in arresting it is most obvious. Even putting aside, however, the inferior liability to attacks of acute inflammatory disease, which we believe to exist among most persons, whose diet, whether exclusively vegetable or mixed, does not exceed the natural wants of the system, we do not see that the high feeder can be said to be at any real advantage, when he becomes the sufferer under an attack of pneumonia or meningitis, in virtue of his greater power of bearing depletion; unless the more violent measures which it is necessary to take for the purpose of checking the disease, can be shown to be more decidedly efficacious than the milder treatment which suffices in the opposite case. It is undoubtedly very satisfactory to the medical attendant to feel that he can do something vigorous and decisive; and most gratifying is it to watch the results of his well-advised remedial measures. But after he has thus subdued the violence of the disease, has he really brought his patient into a better condition, than the other was without this excessive depletion? Is the duration of the disease diminished? Or are its ultimate results less injurious?

We propound these queries, because it has often happened to us to hear the inferior capacity of the present generation for bearing "active treatment" spoken of in a tone of regret; as if the power of usefulness of an intelligent medical attendant were thereby diminished, and the patient's chances of recovery were proportionably lowered. To us it seems as if the change in question simply gives to the practitioner of the present day the same control over the course of the disease by milder measures, as that for which he was formerly obliged to have recourse to methods of treatment which were more out of harmony with its natural progress. Thus, adopting for illustration the treatment of acute rheumatism, we find that (in certain cases, at least) there is as much to be done by citric acid, with the assistance of a small bleeding and diaphoretics, as was formerly accomplished (or thought to be) by large and repeated bleedings and the active administration of mercury.

We have purposely refrained from discussing numerous important questions connected with the dietetic treatment of disease; because our object has been rather to consider, in the present article, the *principles* upon which all such treatment should be founded. It is of prime importance to establish what is the diet which is best adapted for the sustenance of man in the state of highest vigour; and this question is to be determined,

not so much by the experience of individuals, as by that of large bodies of men, under a variety of circumstances. On many points, we feel that we are not yet in possession of sufficient information; but, upon the whole, we feel justified in drawing the following conclusions:

1. That a diet, consisting wholly of vegetable substances, is capable of maintaining the body in ordinary health and vigour, provided that due care be taken in the selection of the articles of food, so that they shall afford an adequate proportion of the albuminous compounds.

2. That, by the substitution of oleaginous for farinaceous constituents, a vegetable diet may be adapted to the sustenance of man, even in the coldest climates in which he can support life.

3. That a vegetable diet is peculiarly favorable to the maintenance of health in tropical climates, especially when it is free from any large proportion of oleaginous constituents, and is principally composed of farinaceous and albuminous substances, with a due admixture of *fresh* vegetable matters.

4. That, on the other hand, a moderate admixture of animal food, introduced into the diet as a substitute for the albuminous vegetable compounds (so that there is altogether no excess in the proportion of azotised constituents), exerts no deleterious influence whatever; and that there are few, if any, cases, in which benefit is to be expected from the abandonment of it. Further, that there is no difference, in this respect, between animal flesh and the albuminous compounds supplied by milk or eggs; so that the experience of the professed Vegetarians of the present day supports this position, instead of antagonizing it.

5. That the evils, attributed by the strict or true Vegetarians to the use of animal food, apply only to the excessive or too exclusive employment of it; the state of plethora being most likely to be developed, when the whole amount consumed is too great; and the arthritic diathesis being favoured, when the azotised constituents are in too large a proportion to the rest. In the treatment of each of these states, dietetic means are of fundamental importance; the excess of red corpuscles in the blood, which constitutes the essence of the plethoric condition, being susceptible of considerable reduction by abstinence from animal food; and the formation of an excess of uric acid being kept in check by similar means, where the state of the digestive powers allows it to be put in force.

6. That, on the other hand, injury is frequently done by such a reduction in the proportion of albuminous matters, as results from the adoption of a vegetable diet into which they do not enter in sufficient amount; and that the proportion of the red corpuscles of the blood, in particular, is reduced below its natural standard, giving rise to a great variety of functional disorders. At the same time, a mal-assimilation of the starchy material has much to do with the production of the rheumatic diathesis.

7. That, whether the regimen be purely vegetable or be mixed, fresh vegetables and acid fruits should form part of it; on the one hand, to prevent the scorbutic deterioration of the blood; and, on the other, to antagonize the tendency to the production of the lactic acid diathesis.

8. That, whatever be the regimen adopted, oleaginous matters should form a prominent constituent of it; these being essential to the healthy nutrition of the several textures, besides being specially needed for the support of the animal temperature under exposure to cold.

## ART. IX.

*Some Account of the last Yellow Fever Epidemic of British Guiana.* By DANIEL BLAIR, M.D., Surgeon-General of British Guiana. Edited by JOHN DAVY, M.D. F.R.S. L. and E., Inspector-General of Army Hospitals. —London, 1850. 8vo, pp. 161.

THE nature of the work quoted above is exactly indicated by its title. It is not a general treatise upon Yellow Fever, comprising everything that the author has been able to pick up, either from his own observation, or from the works of others; but is simply an account of a particular epidemic, which came under the writer's notice, and which seems to have been studied with much interest and care. To the text of the author, Dr. John Davy has added various notes, confirming, correcting, or modifying, according to his own views, the statements given in the body of the work.

We shall endeavour, in the first instance, to indicate what materials for observation the author possessed, and then to examine into the method he employed for their appreciation and record.

The Colony of British Guiana in South America (5°—7° N.), resembles in its main features the large tract of country which is variously known as Surinam, Cayenne, or Spanish, Dutch, and French Guiana. Its soil is rich and alluvial; its level is below the sea, from which it is protected by embankments; it is traversed by majestic streams, on the borders and at the mouths of which dwell its inhabitants, and by numerous navigable canals. The climate is very humid; the temperature warm but equable; the prevalent winds are sea breezes from the east and north; the land winds from the west and south are much less prevalent, blow less strongly, and are as easily detected by the uncomfortable feelings they cause, as by the effect they produce on the wind-gauge. There are two rainy seasons, the "*little*," from the latter end of November till January, and the "*great*," from the middle of April to the end of July; but during these periods there are breaks of fine weather, and during the dry season there are rainy days. The land winds prevail during the rainy seasons.

"British Guiana," says Dr. Blair, "is a tropical Holland. Behind the empoldered lands extends an almost endless succession of creeks, savannahs, and forests, and in front fringed with a thicket of aquatic trees, quicksand, and mud banks (the alluvial elements of the country), stretch out far into the muddy sea."

Georgetown, the capital of the colony, is situated at the mouth of the Demerara river. It is a large town, composed of wooden houses, laid out in wide rectangular streets, which cover an area of nearly two miles. The houses are raised from the ground on pillars, and are, for the most part, roomy and well ventilated. However, in two of the principal maritime districts of the town, namely, Water Street, and Robb's Town, the houses are nearly in contact, and the ground-floors are used during the day as shops or stores; in addition to this, the highway of Water Street is formed by the embankment that prevents the overflow of the river-tide, and one side of the street is built on the bed of the river itself, being supported above the level of the water by piles and platforms. Across this street also six sluices pass, carrying the drainage and sewerage into the river. The importance of these topographical details will appear from the fact, that it was in these two localities, the lowest, most crowded, and most

unhealthy of Georgetown, that the epidemic recorded by Dr. Blair spent its chief force.

The population of Georgetown at the time of the epidemic referred to, was about 20,000 souls. The prevalent diseases of the white population of Georgetown and British Guiana generally, are malarious affections of various kinds. In the depth of the rainy seasons, when these are severe, the type of fever becomes adynamic and congestive; in the transition period out of the great rainy season, "continued fevers," which we presume are also malarious, are met with. The basis of all the malarious fever is, however, the intermittent, and chiefly its quotidian type. Of 6395 cases admitted into the Colonial Hospital from June 1846 to June 1847, there were 2938 cases of intermittent and remittent fever; and the annual consumption of quinine in Demarara and Essequibo amounts to 3000 ounces. Spleen diseases, malarious dysentery, and malarious ulcers, are also extremely common.

"The diseases from which the colonists are entirely free," writes Dr. Blair, "are contagious or infectious fevers (except the exanthemata), calculus, diabetes, rabies; those from which they are nearly exempt, are tubercle,\* dyspepsia, aneurism, and malignant tumours; those which are mild and of rare occurrence, are the exanthemata and hooping-cough; morbus Brightii (albuminous urine), when it occurs, is a curable symptom; chronic bronchitis in a tubercular diathesis rapidly recovers after arrival here. The air is so bland, that the knee or other joints are unhesitatingly opened if required; for the escape of inflammatory or hydropic secretions (!), the *subcutaneous* operation is unnecessary." (p. 21.)

In the colony thus briefly described, terrible epidemics of Yellow Fever have prevailed from time to time. Yet these attacks appear to have been always separated by considerable intervals of time, which were filled up by the ordinary malarious fevers. In 1793, and again in 1819, severe yellow fever epidemics occurred; but after the cessation of the latter in 1820, yellow fever seems to have disappeared so completely, that when the next epidemic occurred in 1837, the practitioners in Georgetown recognised the early cases simply from the description given of the disease in books. This entire disappearance of yellow fever in the intervals of the epidemics, from this low and marshy country, in which malarious diseases are never absent, is a point of great importance in the etiology of yellow fever.

In April, 1837, the healthiest month of the year, some cases began to appear of a disease, of which "the oldest practitioner of the town had no previous personal knowledge." After a few cases had terminated fatally and unexpectedly, a consultation of the principal practitioners of Georgetown was held, when the symptoms of one of the cases were minutely looked into, and were held to be those of yellow fever, "*as far as their book-knowledge enabled them to decide.*" These first cases occurred in the Water Street before described, and in an adjoining and similarly situated street; cases then appeared gradually along the length of the city, but Water Street was always the portion chiefly affected. After its appearance in the town, the disease manifested itself among the seamen in the river, and so

\* Dr. Davy here inserts a note, that, in 224 fatal cases, occurring in the Colonial General Hospital, 28 only are noticed, in which tubercles in the lungs were detected; and he expresses a doubt, whether the mortality from phthisis in some regiments serving in the West Indies, which is given in the Army Reports, was not attributable to imported, and not to generated phthisis. We may also direct attention to the much debated question of the antagonism of tubercle and marsh disease. British Guiana would be a good field for observation on this point.

many died, that it soon became impossible to get men to ship from England to Demerara; and in 1838 and 1839, men could only be got by false engagements, and many seamen were brought to Georgetown who had shipped for Norway. The mortality among seamen (the number of whom annually in the port is about 4400 on an average of 12 years), is shown by a table given at page 5. In 1835, 2 died; in 1836, 12; in 1837, 257; in 1838, 326; in 1839, 157. In the following year the number fell to 76, but increased in 1841 to 153, and in 1842 to 135. In the four following years, the mortality declined to an average of 13.

The troops quartered at Georgetown remained "exceedingly healthy" till 1839, when they suffered rather severely from the prevalent fever. At the time when this occurred, the disease had nearly disappeared from the town; but it recurred here a few months later, at a period when the military had ceased to suffer. The epidemic may be said to have lasted in the colony, with various lulls and intermissions, till the great rainy season of 1845, when it entirely ceased, being superseded by a slight but very general influenza.

"Occasionally since then," writes Dr. Blair, "the suffused face and eye, frontal headache, and crimson-edged tongue, have been seen engrafted on the intermittents of the seamen of the shipping, showing that a taint still exists; but since the period named, the yellow fever has ceased to have a separate existence among the diseases of the colony." (p. 47.)

The yellow fever was not during this time confined to British Guiana, but, between 1837 and 1841, attacked, more or less severely, many of the West India islands. Much interesting information is given on these points in Dr. Davy's notes; but as our space compels us to keep closely to the epidemic of Demerara, we must refer to the work itself for these particulars.

The *total mortality* in Georgetown itself during the whole fever-period is not stated, and probably the information was not procurable. In the years from 1839-46 inclusive, 3036 cases, termed yellow fever, were admitted into the Colonial Hospital, of which 404 proved fatal.

The *symptoms* presented by this fever accord with the usual description; alternate flushings and rigors, terminating in twenty-four hours in a complete pyrexial state; supraorbital headache; suffusion of the face; injected conjunctivæ; redness of the mucous membranes of the nares, lips, and tongue; vomiting of yellow bilious fluid; with a tender epigastrium; alvine evacuations dark and fetid. These symptoms lasted for two or three days, (the vomited matters becoming greenish,) and then subsided, so that convalescence seemed to be about to be established. Shortly afterwards, however, (the exact time is not given,) this apparent improvement was found to be deceptive; the eye became dull, and the still-injected conjunctivæ of a dull orange-red colour; a distinct yellow suffusion was also perceptible both on the conjunctivæ and the skin; the vomiting returned, the matter vomited being at first clear and acid; but if it lasted with any severity for some hours, specks were seen as if a pinch of snuff had been scattered in the fluid; and when the liquid was allowed to stand, a dark tenacious "clayey" deposit fell to the bottom of the vessel. The tongue was thickly coated; or if, as sometimes happened, the fur cleared off, there was a fiery redness of the lingual surface. If the finger was pressed on the dusky yellowish or purplish skin, the spot

remained pale for a long time, as if the circulation were very languid. The next symptoms showed still greater disorder of the circulation; epistaxis ensued, or ecchymoses occurred in the skin; or suddenly, perhaps, when turning in bed, a spurt of black vomit was thrown involuntarily over the bed and furniture; oozing of blood now sometimes occurred from the mouth, ears, or anus; the black vomit continued; the extremities became cold; the pulse imperceptible; and the patient died in syncope. In many of these cases the intellect remained perfect, and there appear to have been no stupor, no delirium; and during the last thirty-six or forty-eight hours, no complaint of pain in the head. In some cases, however, there was both delirium and coma, and sometimes there were convulsions just before death. In some of these cases, it is said that black vomit may not occur.

It did not seem essential that the fever should thus run through a stage of accession, one of partial retrocession, and one of final acclimation, so to speak; for cases were seen in which the patient had evidently the first stage only, or the first and second stage, and then recovered. Hence Dr. Blair divides all cases into three varieties, *febris simplex*, *mitior*, and *gravior*. He designates the three stages as those of *excitement*, of *acid elimination*, and of *passive hæmorrhage*, to each of which his special designation, *febris simplex*, *mitior*, or *gravior*, refers. In an after part of the book cases are referred to, in which the person seemed killed almost at once, the third stage only appearing.

When recovery occurred, such sequelæ as abscesses, furunculi, bullæ, swelling and sloughing of the parotid, swelling of the lymphatic glands, or jaundice, were occasionally left. But much more generally convalescence was rapid and immediate, and was attended by the most invigorating feelings of returning health. Very different, indeed, were these feelings, Dr. Blair remarks, from those during convalescence after remittent fever.

With regard to particular symptoms, the pulse was little affected. Its average in 121 cases was  $97\frac{1}{2}$  per minute for the first day of the disease; on the second, on an average of 338 cases, it had sank to 90·8; on the third day, on an average of 406 cases, it had fallen again to 83·5; and on the fourth day, on an average of 388 cases, it only reached 80 per minute. On no succeeding day was the average above 80. These observations are evidently taken from the whole series of cases, mild and severe; it would, however, have been very desirable, to have had the daily average of the pulse in the fatal cases only. The pulse varied considerably on the same day, and varied also on consecutive days, falling sometimes 10, 20, or even 30 beats, or rising as many within 24 hours. In convalescence the pulse was extremely slow; the *position*, however, is not stated.

The heat of the surface in the first days was greatest over the head and chest, and in the hands, arms, and feet. It was most marked in the cerebral variety, but in all lasted only during the early stage. In the stage of "*acid elimination*," the heat was not more than natural, and in that of "*passive hæmorrhage*" the surface was cold.

The yellowness of the skin was *observed in every fatal case*, the tints varied from a dirty parchment to a light lemon, a gamboge, a deep orange, or a chrome or ochre colour. Among 2071 cases included under the terms "*mitior and gravior*," 385 had yellow skin, and of these 178 died. The per centage of its appearance was therefore 18·54, and the mortality of the symptom was 42·23. The time for its appearance was from the third to

the sixth days, and the fourth and fifth days were its days of election. It did, however, occasionally occur on the first day, and was noted in five cases on the eleventh.

With regard to the black vomit, its characters were the same as those so often recorded; it reddened litmus paper, effervesced with carbonates, and the flocculent or granular sediment dissolved readily in liq. potassæ. It occurred in 366 of the 2071 cases already referred to. The white or acid vomiting, which preceded the black vomiting was a still more common symptom, occurring in 417 of 2071 cases. Of the 366 cases in which black vomit appeared, 277 died, or at the rate of 75·68 per cent. The black vomit and yellow skin appeared at the same time, or the latter had slightly the priority. Thus, in 139 cases, yellow skin preceded black vomit in 51 cases, appeared simultaneously in 46, and followed it in 42. The two symptoms coexisted in 144 of the 2071; and of these 144 cases, 122 were fatal, or at the rate of nearly 85 per cent. It would thus appear, however, that as yellow skin was observed in 385 cases, and black vomit in 366, and as the two together are said to have been observed only in 144 cases, many cases had yellow skin without black vomit, and others had black vomit without yellow skin.

The blood after the first stage presented the yellow serum, and this yellowness was found in some cases distinctly to precede the yellow skin. Otherwise the blood, when drawn from an arm, coagulated firmly. A case is cited (p. 84), in which this firm coagulation occurred in blood drawn from the arm, although "dissolved" "molasses-like" blood was at the time oozing from the mouth and anus. Most unfortunately no chemical examination seems to have been made of the blood.\*

An attack of this fever appeared decidedly to protect against a second attack; at least no person who had it in the epidemic of 1819, suffered in 1837. So also, after a month's convalescence, relapses were hardly ever seen. Relapses were, however, not uncommon during the early days of convalescence. Abstracts of 31 cases are given, in which relapses occurred. In 28 of these, which are all that can be used, we find the average time of relapse to be 12·7 days after the earliest symptoms,—convalescence having been generally established on the 7th or 8th day. The longest time after the commencement of the disease at which a relapse occurred is 24 days, and the shortest time is 6 days. In three cases there was a second relapse. Of the 31 cases of relapse, 14 died, or at the rate of 45·5 per cent. Although these cases are too indefinitely reported, to allow us to draw any conclusions about the exact day of relapse, there would certainly seem to be a definite rule respecting the day, from the number of cases in which relapse occurred on the 11th, 12th, and 13th day; and those who have followed Cormack's and Wardell's description of the English relapsing fever, and remember the parallel which these observers have drawn between it and yellow fever, will feel some interest in Dr. Blair's observations. The relapses seem also to have come on, as in the case of our own relapsing fever, quite suddenly; the patient being noted in the morning as quite well, and some even being discharged from hospital; while in the evening of the

\* Dr. Davy mentions in a note, that he has been informed by Staff-Surgeon Collins, that, in every case of decided yellow fever, he found the urine albuminous, a condition which it assumed during the second or third day, and maintained even far into convalescence. Dr. Davy himself noticed, that nitric acid gave a green tint to the urine, apparently from bile. (p. 99.)

same day the relapse occurred.—The relative mortality among fever cases does not very clearly appear. The mortality of untreated cases seems to have been about 29 per cent.; in the hospital records, the rate of mortality among all cases was 13·3 per cent.; the rate of mortality on the “gravior” form, (we presume that attended with passive hemorrhages,) was 24·6 per cent.; and the rate on the two classes of “gravior and mitior” (see Dr. Blair’s terms before referred to), was 19·51 per cent. The following table is given to illustrate the influence of age on mortality:

“FEBRIS MITIOR ET GRAVIOR.	
“ Under 15 years, the mortality was . . .	24·39 per cent.
From 15 to 20 years . . . . .	23·44 ”
” 20 ” 30 ” . . . . .	28·45 ”
” 30 ” 40 ” . . . . .	19·90 ”
” 40 ” 50 ” . . . . .	21·91 ”
” 50 upwards . . . . .	23·22 ”

The value of this table is almost nullified by our want of information as to the number of cases at each age, so that we might form an idea of its correctness.

The *post-mortem* appearances were noted by Dr. Blair in 97 cases. The condition of most parts of the body appears to have been carefully observed, and the number of times each condition occurred is noted numerically. This renders the chapter on the *post-mortem* appearances very valuable; and its value would have been still more increased, had the coexisting conditions of each state been more fully brought out. In the case of each organ also, it is not stated how many organs were examined, so that it is impossible to know how often the same organ appears in the table. The following table of the condition of the liver will exhibit both our author’s method, and the defects to which we allude.

	Cases.
“ Liver—recorded as natural . . . . .	3
” recent adhesions of peritoneum and diaphragm . . . . .	1
” ” diaphragm . . . . .	1
” enlargement of . . . . .	20
” ” right lobe of . . . . .	4
” unusually small . . . . .	4
” remarkably elastic . . . . .	4
” flabby and elastic . . . . .	1
” friable . . . . .	17
” much softened . . . . .	2
” hard and grating . . . . .	1
” dense . . . . .	2
” cirrhosis . . . . .	1
” external surface of deep yellow . . . . .	10
” ” pale yellow . . . . .	9
” uniform yellow colour of investing membrane and parenchyma . . . . .	1
” external surface mottled, yellow and red, and yellow and slate colour . . . . .	1
” pale green . . . . .	1
” greenish brown . . . . .	2
” very red . . . . .	1
” “Bath-brick” colour . . . . .	1
” pale slate colour . . . . .	1



	Cases.
" <i>Liver</i> —ochreous brown . . . . .	2
" reddish yellow, externally and internally . . . . .	1
" mottled externally; "Bath-brick" internally . . . . .	1
" "Bath-brick" externally; speckled internally . . . . .	2
" mottled, pinkish-red externally; clayey-yellow internally . . . . .	1
" mottled, of different shades of yellow, externally; dark yellow, internally . . . . .	1
" light brown externally; anæmiated internally . . . . .	2
" speckled, red on yellow ground, externally and internally . . . . .	1
" red dots, surrounded by yellow rings, internally . . . . .	16
" inflamed . . . . .	1
" portal system gorged . . . . .	13
" " empty . . . . .	2
" parenchymatous congestion . . . . .	17
" anæmiated . . . . .	2
" gorged with bile drops . . . . .	1
" black blood . . . . .	4
" thin serous blood . . . . .	4." (p. 96.)

The defects of this method appear at once.

How many livers were here examined? This does not appear from the table. What was the predominant shade of colour in the enlarged livers? The question cannot be answered. Was the "enlargement" attended with "friability," or the reverse? An answer could only be guessed at by observing, that 20 livers are said to be enlarged and 17 friable, and that there are no higher numbers quoted; so that it would appear possible, that the total number of livers examined was not above 20, and that, therefore, the enlarged livers were friable. But here we beg the attention of our readers to the difficulties thrown in our way by loose and inaccurate methods of arranging materials which may themselves be well recorded. Dr. Blair's tables of *post-mortem* appearances are valuable, no doubt; a better method would have made them infinitely more so.

To turn from this point, the characters of the liver appear to have been mainly those of enlargement, friability, and yellow colour; herein agreeing very closely with the anatomical signs noted by Louis in the epidemic at Gibraltar in 1828.\*

The spleen was perfectly healthy in six cases, friable and soft in 12; congested in 11, and enlarged in 14. Did the enlargement and softening go together here? We presume they did; and if so, the characters of the spleen would be enlargement and softening. It is much to be regretted that the weights are not given. Dr. Davy says, that in the late destructive epidemic in Barbadoes, the spleen "exhibited no uniform, or indeed but rarely any, well-marked morbid appearance." (p. 98.) This would seem very much opposed to the observations of Dr. Blair.

\* Dr. Davy adds in a note: "In the majority of cases, the liver has been found, after death from yellow fever in Gibraltar, of a yellowish hue, somewhat between straw-colour and that of unbleached wax, and unusually friable. The same has frequently been observed before. My friend, Mr. Gulliver, conjectures the cause of the pale hue to be 'merely a bloodless state of the organ,' a draining of the red corpuscles in the black vomit from the portal system." (p. 97.) How manifestly inadequate this explanation is, we need hardly remark. We are all familiar enough with anæmiated livers; and nothing can be more different than the hue in these cases, and the altogether special and peculiar tint of the liver in yellow fever.

The kidneys are said to have been "*inflamed*" in 10 cases,\* and to have had the "*cortical substance hypertrophied*" in 23.

The lungs and heart presented nothing particular. The lungs are said to have been "*gorged*" in 20 cases, and the heart was soft and flabby in 8. The blood was "*almost invariably abnormally thin and black.*" (p. 100.) It is noted as yellow in the heart in 3 cases.

In the head there was congestion of the pia mater; yellow tinging of the membranes; in 10 cases the sinuses and vessels were "*enormously congested,*" in 14 there was "*general vascularity and congestion;*" in 3 there was "*extravasation on brain.*"

The condition of the alimentary canal was minutely noted; though, as usual, we are left entirely in the dark as to the number of cases examined. We extract the tables referring to the ileum and large intestines, as bearing on a point we shall have to consider in a future article.

	Cases.
"Ileum—Contracted . . . . .	1
" Containing much black vomit . . . . .	13
" Hyperæmiated . . . . .	19
" Ecchymosis of . . . . .	1
" Mucous membrane eroded . . . . .	1
" Extensive submucous arborizations . . . . .	7
" Empty . . . . .	1
" Perfectly healthy . . . . .	1
Cæcum—Contracted as in above case . . . . .	1
" Containing dark molasses-like blood . . . . .	3
" Blood and fæces . . . . .	1
" Black vomit matter . . . . .	5
" Much hyperæmiated . . . . .	8
Colon—Much contracted . . . . .	11
" Inflated . . . . .	4
" Containing much black-vomit matter . . . . .	5
" Hyperæmiated . . . . .	3
"       and ecchymosed . . . . .	2
" Green coloured, internally . . . . .	1
" Superficial ulcers on . . . . .	2
" Colon and rectum healthy . . . . .	1

The stomach contained black vomit in 79 cases; blood in 1; other fluids in 11; and it was empty in two. It was, "*generally hyperæmiated*" in 71 cases; ecchymosed in 15; with "*remarkable submucous vascular arborizations*" in 8. It was mammillated in 53 cases, and showed superficial ulcerations and erosions in 15. In 11 cases the author calls it "*inflamed,*" but puts a note of interrogation after the word "*inflamed.*" The mucous membrane of the œsophagus was "*eroded*" in 54 cases; in 11 it was ulcerated; hyperæmiated in 41; "*inflamed*" (?) in 8.

The agminated and solitary glands are described in the same way; but they are placed apart from either the small or the large intestines, so that it is uncertain whether the solitary glands of the large intestines are or are not included in the account. It does not very clearly appear either, to which set of glands, solitary or agminated, the description refers.

\* The author adds in a note: "The pyuritis was generally alight, but occasionally extended to the ureters." What a pity no details are given; the observation, as it stands, is worthless.

			Cases.
"Aggregate and Solitary Glands—	Well developed	.	35
"	Hyperæmiated	.	8
"	Surrounded by blue ecchymosis	.	1
"	Red areolæ	.	1
"	With black 'navels'	.	3" (p.94.)

It would thus clearly appear that there is no deposit and consecutive ulceration in Peyer's patches, as in typhoid fever. The mesenteric glands are noticed as "enlarged and congested" in only 1 case. The author afterwards describes the condition of the intestines as follows.

"The villi were the chief seats of engorgement, sometimes to perfect blackness. The hyperæmia had sometimes a stellated appearance, sometimes dotted or blurred or striated, sometimes arborescent, and generally of a rusty red colour; frequently inflammation, congestion, and ecchymosis were so blended as to defy discrimination. The hyperæmiated or eroded surfaces were generally clothed with viscid mucus; sometimes the stomach was of an uniform deep claret-red colour, as if by the imbibition of imperfectly-formed black vomit, and giving its peritoneal coat a rosy hue; sometimes the general red discoloration of an apparently inflamed piece of stomach or intestine could be scraped off without injuring the mucous tissue. On such a piece being slightly washed of its mucosities, and held up to the light, the appearance resolved itself to a mere film of blood, adherent, apparently, by the undeveloped epithelial matter. The ecchymosis was occasionally in the mucous, frequently in the submucous, and only once in the subperitoneal tissue. The unusual conditions of the solitary and aggregate glands seemed due entirely to surrounding and interstitial congestion and ecchymosis. At the ilio-cæcal valve, the hyperæmia was thickly punctuated, like a closely-shaven black beard." (p. 101.)

The yellowness observed during life lasted after death. In 13 cases the peritoneum was yellow; in other cases, it was white or injected.

No remark is made respecting rigor mortis, except that "in three cases there was remarkable cadaveric rigidity."

The *treatment* of this epidemic need not detain us, as there is nothing new stated, and nothing efficacious pointed out, except that it is said, if 20 grains of calomel, and 24 of quinine, were given within a few hours after the development of the first stage of the disease, and were followed in six hours by a dose of ℥ij of castor-oil, in nine cases out of ten the disease was arrested. For ourselves, we can only say, that we question the diagnosis of such cases; and we are sorry to find Dr. Blair countenancing that abominable system of large doses of calomel, which was first recommended on the strength of a crazy hypothesis, and has since been persevered in by some with a tenacity that one can heartily wish had been applied to a better cause. After the first large dose of calomel and quinine, similar large doses were given, so that "calomel and quinine might be introduced cito, tuto, et jucunde (!)"\*

Such were the main symptoms and post-mortem appearances of the fever described by Dr. Blair.† We shall take another opportunity of

\* Dr. Davy says, in a note: "The above treatment, by calomel and quinine, was tried in the last endemic fever of Barbadoes, and extensively; but, I regret to say, not with like success." It has been tried five hundred times, and it is really too bad to make us go through the same dreary catalogue of calomel, calomel for ever.

† We must not omit to mention, that Dr. Blair mentions (p. 63) the fact of some puppies being affected with fever, loathing of food, yellow eyes, groin swelling, and irritability of stomach. They all died; and, after death, one observer found black vomit in the stomach. Dr. Blair opened one, but found no black vomit. Feathered stock also were reported to him as having died very speedily, the chief symptoms being, drooping of the wings and exhalation of black fluid from the beaks. This reminds us of an observation of an old writer on Indian diseases, Girdlestone, of fowls being affected with cholera, when cooped near the ground.

comparing it with the yellow fever of other regions; and shall, at the present time, pass on to the consideration of its causes, and its apparent mode of spread and development.

1st. *Did the yellow fever of British Guiana arise from contagion?* When Dr. Blair's attention was first called to a suspicious case of fever, different from the ordinary fevers of the place, it was not long in being discovered, that several other cases of the same kind were under treatment by other practitioners. The *earliest* cases of this epidemic, as in so many other examples, appear to have escaped Dr. Blair's notice; and the disease may be considered to have fairly established itself, before his attention was drawn to it. In whom the first case occurred—whether it was imported or not—is not stated by Dr. Blair himself; and, somewhat strangely, he seems to have made no attempt to trace back the cases, rigorously and carefully, in order to determine this important point of evidence. Yet, although the evidence on this point does not appear from his work to have fallen within his personal knowledge, Dr. Blair expresses himself most decidedly against the importation of the disease; and states that he possesses evidence to disprove the assertions made in a letter, published in the 'Medical Gazette' of January 1838, by Dr. Fraser, who attempted to prove the contagion and importation of the disease. Dr. Blair does not give this counter-evidence, however; but contents himself with stating, that the materials are in his possession, but that it is unnecessary for him, unless particularly called upon, to refer to them further.\*

Now, most certainly we do not see why, if Dr. Fraser has made assertions which cannot be supported, he should not be replied to in a fair and temperate manner; and we, therefore, *do* call upon Dr. Blair to bring his evidence forward. We shall not at present allude further to the statements made in Dr. Fraser's letter, as, if these are inaccurate, it would be wrong to give them additional circulation, without relating, at the same time, the real facts of the case. But Dr. Blair must remember, that his simple assertion that Dr. Fraser's facts are erroneous is not admissible as evidence; and if he does not give sufficient reason for his opinion, by relating the facts which disprove Dr. Fraser's statements, he may rest assured that these statements will be adopted by the writers who compile from such documents as these the history of yellow fever epidemics; and if Dr. Blair's assertion be correct, will afford another memorable example of the untrustworthy evidence with which medical writers are contented to discuss these momentous questions of contagion or non-contagion.

Whatever may have been the origin of the epidemic, Dr. Blair informs us that it afterwards exhibited no contagious properties; he states that the nurses in the hospital were not attacked; that the resident surgeons, dispensers, and stewards, escaped with a single exception; that only two of the Georgetown practitioners suffered; that, in the case of a garrison at Berbice, a rigid quarantine did not exclude the disease; that, except Dr. Fraser, no professional or non-professional person ever suspected contagion, during the eight years the disease lingered in the colony.

\* Dr. Blair writes (p. 54): "There is a great want of caution exhibited in Dr. Fraser's letter; hasty statements, probably on mere hearsay evidence, were fancifully arranged by him, without previous inquiry as to their authenticity or genuineness. Those who know Dr. Fraser as I do, will acquit him of any attempt to cheat, either an individual or the public; but the material statements contained in his letter, in support of the contagiousness of our epidemic, are either defective, hypothetical, or fictitious."

The value of this evidence is greatly diminished by the loose way in which it is brought forward. We do not know the number of nurses, of practitioners, or of dispensers; and yet the value of the negative argument used by Dr. Blair mainly depends on the comparison between the numbers of persons thus exposed and escaping, and the number of other persons not so exposed and escaping, and details of the like kind. The failure of quarantine also should be accompanied, to make the evidence useful to us, by an exact statement of how the cordon was formed, and in what manner the first cases subsequently occurred. We strongly feel, that much of the uncertainty respecting the contagion of various diseases has been produced by the inaccurate method of stating facts without sufficient detail, of which we are sorry to find Dr. Blair furnishing an example. Would such a method of inquiry be countenanced in any other science except medicine?

In the appendix at p. 153, there is a copy of a Report of the Guiana Medical Society, in which it is stated, that the first case of the disease occurred in Water Street, in the person of a strong young man, who had arrived three months previously from Dublin. The Society decided, that the disease was not contagious; 1st, because, though intercourse is always kept up with some of the West Indian islands in which yellow fever prevails more or less every year, the disease visits Guiana only at long intervals; 2d, because in the last epidemic it was confined to Europeans, or other strangers recently arrived in the colony; 3d, because the first case (above noted) was not imported; 4th, because the disease spread with great rapidity, and appeared in the shipping and different parts of Water Street at the same time; 5th, because persons coming from the country to Water Street got the disease, and yet returning home did not spread it; 6th, because in the Colonial Hospital, fewer patients labouring under other diseases were attacked, than would have been the case if the disease had been contagious, like smallpox; 7th, that the resident surgeons, nurses, &c. of the hospital were not attacked; 8th, because there was no direct evidence of contagion.—Many of these arguments are rather inconclusive, particularly the 1st, 2d, 4th, 5th, and 8th; the rest are not related with sufficient minuteness. We place the facts merely before our readers, confessing ourselves quite undecided, as to whether the arguments adduced warrant the conclusions drawn from them.

But while on this subject, we cannot refrain from an expression of astonishment at the statement made by Dr. Davy at page 137, respecting the 'Eclair' steamer, and the introduction of yellow fever into Boa Vista. It is hardly credible that Dr. Davy should, without even a reference, pass by Dr. M'William's examination into this point, and should positively quote Dr. King's inaccurate and illogical Report, from which, however, the importation of the disease into Boa Vista can actually be almost proved, although the intention of the writer was to disprove it altogether.\* We can really find no excuse for Dr. Davy: he could not have been ignorant of the existence of Dr. M'William's Report; and knowing its existence, he was bound to look into it. If he has looked into it, and can find any flaw in it, we should be glad to know where it is, as we formerly analysed this Report carefully, and felt ourselves constrained to adopt its conclusions; and any error in this point would have important consequences. But if Dr. Davy has thrown this Report aside, under the influence of his precon-

\* See vol. i, pp. 49, 369; and vol. ii, p. 163.

ceived opinions, we can only say, that such inaccurate and unscientific observations as those here made, (observations in which everything is settled by the rule of a necessarily narrow and fallible personal experience,) delay the progress of knowledge, produce the obscurity which covers this department of medicine, and load our records with erroneous statements and one-sided opinions, which it requires infinite pains and trouble to erase. Let Dr. Davy consider well Dr. M'William's Report; and if he does not discover that yellow fever can be contagious, we trust we shall know the reasons of his dissent.

To return to British Guiana, we must leave the subject of contagion with this conclusion, that the evidence adduced in Dr. Blair's work is of the same loose kind as that which overloads so many of our treatises on yellow fevers; and that we doubt whether it should not be thrown aside altogether, as too inaccurate for scientific use. We pass on at present to another point.

2. *Did the yellow fever grow out of a malarious fever?*—In our analysis of Dr. Bryson's work\* on the African fevers, we adduced the evidence collected by this excellent observer, to show that on the coast of Africa the epidemic yellow fever seemed to take its origin in, or rather to grow out of, and to be a transformation of, the ordinary remittent fevers, to which, by common consent, a local and endemic origin is assigned. In Sierra Leone, the common remittent fever, always present, though in very variable intensity, periodically merges into a severer disease, which appears to be a development of itself. Such an intensified affection also undoubtedly occasionally acquires or exhibits contagious properties, and is capable of being carried to places previously healthy, provided such places present the meteorological features of a certain temperature, a certain moisture, &c., which may permit the poison to exist. Dr. Blair's experience is, however, directly counter to this opinion; and as British Guiana has presented unusually favorable opportunities for inquiry, we are inclined to attach great weight to his views on this point. British Guiana is eminently a malarious country; remittents and intermittents abound there; in no country of the same size are such quantities of quinine used; its practitioners are familiar with all the diverse forms of malarious disease; and yet, when, in its singular cycle, the yellow fever visits the country, all without exception declare it to be a disease different from any to which they have been accustomed. The gradual transition of the malarious fevers into it, appears not to have been noticed; and though the very earliest cases may have escaped detection, it is quite evident, that very few had occurred before the presence of an unusual disease was recognised by more than one practitioner. Although, however, the transition-forms from common remittent into epidemic yellow fever either did not exist, or were overlooked, it does appear, that coincidently with the yellow fever there may have been an unusual prevalence of undoubted malarious fevers. Unfortunately, Dr. Blair does not give us much information on this point. At pages 34, 35, and 44, we find tables in which the amount of intermittents in the Seaman's Hospital is recorded; but the numbers are few, and the standard of comparison, as given by other years, being wanting, we have been unable to make any use of them.† In a note at page 29, Dr. Davy mentions, that in

\* Vol. IV, p. 459.

† At page 33, Dr. Blair makes an incidental statement, which bears upon this point, and which it  
12-21.

August, 1837, when yellow fever appeared among the garrison of Georgetown, intermittent fever was "very prevalent;" for 32 cases of yellow, there were 1435 of intermittent, fever. We must content ourselves, in the absence of data for an accurate comparison of the amount of undoubted malarious disease in yellow fever years, and in years free from this disease, with simply recording Dr. Blair's opinion in regard to the connection of these several affections.

3. *If yellow fever is not produced by a contagious poison passing from the sick to those around them, and is not an offshoot or an intensified form of common malarious fevers, what is its origin?*—Dr. Blair and Dr. Davy appear to adopt the opinion, which considers yellow fever as the effect of a specific poison, so far resembling the poison of malarious fever, in that it has a local origin, and does not depend on the human body for its propagation; not, however, being referable to the precise originating conditions from which the malarious poisons spring, but having conditions of origin and of propagation peculiar to itself.

We have on a former occasion considered this and the opinions allied to it;\* and as it is necessary to restrict ourselves at present entirely to British Guiana, we shall at the present moment merely record Dr. Blair's opinion. Some of the facts on which he supports it will be considered under the next heading.

4. *Leaving the question of the ultimate origin of the yellow fever poison undecided, were there any conditions in British Guiana which seemed to favour its development, or on the contrary to check its propagation?*—Some most important information is given by Dr. Blair on these points; and the opinions which have been lately gaining ground, that yellow fever, like the fevers of temperate climates, is influenced in an extraordinary degree by those circumstances, which are commonly included under the term of "sanitary conditions," is fully borne out. In British Guiana, as in Jamaica, in America, in Carthage, in Sierra Leone, in Seville, or in Gibraltar, we find the same class of unhealthy conditions; which, differing somewhat in detail in each place, produce in all the same effects of depressing the health of the bodies exposed to them, and of affording to the morbid agent the conditions which seem essential for its complete development.

The most general of Dr. Blair's facts will be best given in his own words:

"In the preliminary remarks," he writes, "the mud-flats and sandbanks which form the *foreshores* of British Guiana have been referred to; of such as these no doubt the whole colony was originally formed. They shelve out into the sea in front of the poldered land, and are composed of banks or flats, of fine siliceous sand (caddy) or shell sand, or drift-mud, in alternate ridges, or superimposed in the order as stated. The sea sometimes encroaches on the land, and these foreshores are washed away, and plantation-embankments injured or destroyed, and the safety of the town threatened. Sometimes the sea again recedes, new land rises in the sea front,

would have been well to have related at greater length. "Till 1842," says he, "I had charge of the plantation hospitals of 'La Penitence,' 'Ruinweld,' 'Rome,' and 'Houston,' on which were many Madeira emigrants, and I found that, in proportion as these emigrants approached town or coastward in their locations, the yellow fever predominated; and, as they receded, the intermittent fever asserted supremacy,"—as if the intermittent had disappeared from the localities where the intense disease prevailed.

\* Vol. I, p. 51.

demanding laborious and expensive operations to keep the draining trenches and sluices patent. These oscillations are periodical and alternating, but their causes and laws have never been determined or investigated; and indeed they have hitherto excited little remark, except at the moment when the obstructions to drainage, or destruction of dams, buildings, and embankments, have entailed expense or loss. Future observation may connect these oscillatory movements of our foreshores with meteorological cycles, in which the force and direction of the wind will be found material elements. There appear to be three epochs of oscillation determined."

Dr. Blair then cites official documents to prove that government works were undertaken, in consequence of the encroachments of the sea, in 1797 to 1800, in 1821-22, and in 1847, and remarks, that each period of encroachment must have been preceded by a corresponding epoch of alluvial deposition. He then continues:

"The periods of the *acme*\* of accumulation seem singularly coincident with the prevalence of epidemic yellow fever, and the period of wash, or encroachment of the sea, with the disappearance of the disease. The plan now published of the military grounds shows the condition of our foreshores in 1836 and 1847. The land which had been making all along the shore steadily till after 1837, has been, for the last three or four years, melting away, and now the military works are threatened with destruction; the river bank on both sides is washed; *old barriers*, as at plantation Ruimveld, require to be *renewed*; churches, as at St. Swithin's, to be removed; and along the east coast, a retreat is in many places required for the whole plantation embankments. *Simultaneously with these scavenger operations of the sea, the yellow fever epidemic has disappeared.* In connecting these phenomena with each other, it can be done now in a general way only. They are simply associated phenomena; whether by accident, or in the relation of cause and effect, or as joint effects of a common cause, very many future observations may be required to decide. The present coincidence, however, is of importance as a finger-post to the whereabouts the pathogenic causes are to be sought." (pp. 114-15-16.)

These observations are very important, although at present necessarily inconclusive. The inquiry into possible similar conditions in other yellow fever districts, situated on the shore, as so many of them are, would be a point worthy of the consideration of the new Epidemiological Society, which, under the able auspices of Dr. Babington and Mr. Tucker, has been lately founded in London, and from which, if their funds can enable them to undertake careful associated observations, on a given plan, great results may be anticipated.

After these remarks on the shore, Dr. Blair passes to the meteorological features of the latter years of the epidemic. Several tables are given, the importance of which depends mainly upon comparison with previous years. They are, however, very valuable, as the commencement of accurate meteorological observations at this colony. If they are continued up to the next yellow fever epidemic in 1888, or whenever it may be, results may be drawn from them. It would appear that great heat and drought are not necessarily productive of yellow fever, nor do they seem unfavorable for its development. There appears to have been a season of extraordinary heat and dryness in 1833, without any production of the disease. Such a fact as this, even by itself, and without reference to the geographical distribution of disease, seems conclusive against the opinion that great heat alone can originate yellow fever.

The popular opinion in British Guiana, Dr. Blair informs us (p. 52),

\* The Italics are our own.



is, that yellow fever is dependent on deficient drainage; and although we know that this opinion, when exclusively maintained, is erroneous, there would seem little doubt that it is partly based on really accurate observation. It contains so much truth as this, that yellow fever flourishes where drainage is deficient; this insalubrious condition of things acting on the specific poison of yellow fever, as it will on the poisons of typhus, of cholera, or of plague; and predisposing the human system to its reception, as it does in regard to the other zymotic poisons. We have already referred to the fact, that the epidemic commenced in the parts of the town which were exposed to the effluvia from all the drains of the city; and that the greater number of cases were furnished by these quarters.

"The focus of disease, in this locality, seemed to be the mud-lots of Georgetown, and their immediate neighbourhood. Many strange and true predictions of attacks of the epidemic from known exposure, occur among my notes,—many neglected warnings, followed by fatal penalties. Some merchants at last retreated from their dwellings above their stores in Water Street, when any unseasoned member of their family rejoined it from Europe. The further retired in town, the safer the residence was felt to be. Projects for building a river wall were openly discussed, and £100 premium was awarded by the town-council for the best plan. Ships were moored and unmoored to get rid of unhealthy positions in the river, and men held their breath in passing the stellings." (p. 33.)

In the harbour itself, however, a table given by Dr. Blair (p. 35) seems to show, that ships at some distance from the shore (750 feet) suffered more than ships anchored only 450 feet off; but the difference between the distances is not sufficient; and Dr. Blair also mentions the fact, that the ships which lie closest in-shore are generally North Americans, who seldom stop longer than a week. When ships passed the town, and proceeded up the river into the interior, for the purpose of cutting wood, they remained free from yellow fever, although the men "were exposed to all the vicissitudes of weather, using tremendous exertions in felling and hauling the heavy logs, and often working with half the body immersed in water, and the whole force of the sun acting upon them while engaged in the laborious occupation of heaving the logs aboard." (p. 33.) At this time the ships in the mouth of the river were losing a third or a half of their men. The "*very mouth*" of the river seemed to be the most unhealthy position for ships.

One instance of insalubrious site mentioned by Dr. Blair is very instructive:

"To the east and north-east of the military grounds, and on the front lands of plantation Thomas and Kitty, there were many hundred acres of jungle, forming a well-sheltered swamp. To the north, during the time of the epidemic, there also existed a naked marsh of about 250 acres. During each high spring-tide the sea covered the surface of the marsh. The soil was composed of the usual constituents of our foreshores, viz., clay, caddy,\* and drift-mud. The surface was jagged by a vast number of half-rotten stems and roots, probably the vestiges of some previous cultivation, projecting several feet above the level of the swamp. On the sea-margin, a forest of young 'corrudas' and other trees, which delight in a brackish, humid soil, was springing up; the marsh was tufted with a coarse grass, under whose half-withered leaves myriads of insects were sheltered; innumerable crabs burrowed throughout; fragments of drift-wood, bones, dead spawn, dried mollusca, and small fish, left by the retreat of the tide, were scattered profusely over the

\* "*Caddy*" is formed from water-worn grains of quartz. (Dr. Davy.)

surface. Near to the public road were small gullies, communicating, apparently, with the jungle, lined with cryptogamic plants, and containing frothy, putrid-looking water. Within the trenches, aquatic larvæ and exuvie abounded, and over them clouds of mosquitoes and sand-flies. *In close proximity, and to leeward of the marsh, stood the military hospital. It is said, that almost every case admitted to this hospital, during the epidemic, BECAME YELLOW FEVER,\* NO MATTER WHAT THE AILMENT ON ADMISSION; and it ultimately became such a terror to the soldiers, that the utmost difficulty was experienced in persuading them to enter it when sick.*" (pp. 5-6.)

Dr. Blair informs us, that he ascertained these particulars in 1840, in consequence of an inquiry having been made by the secretary at war, AS TO WHY, IN A FEW MONTHS, SIXTY-NINE PER CENT. OF ALL THE WHITE TROOPS HAD PERISHED.

Is not this frightful statement a most bitter satire, either on the incapacity of the medical advisers, who could sanction such an hospital, or, what is more likely, on the obstinacy and contemptuous disregard of medical advice, in which our military rulers indulge? Poor Secretary at War, even *he* was staggered at 69 per cent. He had been pretty well used to a rate of mortality which probably was at all times quadruple what it might be; for the same unhealthy conditions would affect and intensify all diseases, although their terrible force would probably not arouse the attention of a secretary at war, until a pestilence, like that of 1837, *killed in a few months 69 per cent. of all the European troops.* With such facts before him, the Secretary makes inquiries, and learns what kind of charnel-house it is, into which sick soldiers, serving in Demarara, are thrust to die. But why not inquire previously? Why not censure, and more than censure, why not try by court-martial, for ignorance and incapacity, the medical officer who had sanctioned it, or the officer in command who had erected it, despite of warning? Against such men, a coroner's jury, in this country, would return a verdict of *manslaughter*, with as much justice as against Mr. Drouet of Tooting. When will our rulers learn, that the frame of a man is delicately made, and cannot with impunity be looked upon as an automaton, who is expected to go through the manual and platoon, pipeclay his belts, and appear on guard, but whose power of wearing out, or of being injured, is a point which his masters think it quite unnecessary to look into? Are we, in all parts of our immense dominions, except those in which kind Nature has granted her best gift of almost perfect salubrity, are we at Demerara, at Kurrachee, at Barbadoes, at Bellary,† and at fifty other places, on a minor scale, for ever to witness these frightful tragedies; and are we for ever to be called upon to protest, in the name of humanity, and in that of our science and calling, against the gross neglect and ignorance to which these tragedies are owing, and yet to protest in vain? Such an hospital as that which exists, or which in 1840 did exist, in Georgetown, ought to be at once condemned, without waiting for the advent of some more mortal sickness than is usually met with in it. It may be considered certain that, without yellow fever at all, such an hospital intensifies all diseases, and renders some mortal which would otherwise be curable. On the men who maintain it, after full warning of its effects, rests inevitably the blood of those among their fellow-men who have fallen victims to its pernicious influences.

\* The influence of the *marsh* appears here well marked. In ordinary years it produced common intermittents and remittents; in yellow fever years it produced yellow fever.

† See Vol. II, p. 81; and Vol. III, p. 26.

These facts recorded by Dr. Blair, as to the effect of local conditions upon the poison of yellow fever, agree with what we previously knew. The exact influence of each separate unfavorable condition remains yet to be worked out. Sanitary science is in its infancy, and deals at present only in generalities.

5. *Such being the external conditions, as far as laid down by Dr. Blair, which seemed to favour the spread of the poison, what facility of reception was afforded to it by the human systems subjected to its influence?*

Dr. Blair notices the two cardinal points, that new comers from cold climates (*no matter of what race*, p. 59) were especially liable, and that long residence in hot climates was a preservative against attack.\* Yet at the beginning of the epidemic, "Europeans of many years' residence, and some of the black and coloured population, and Indians, suffered from the disease" (p. 59), as if the virulence of the disease was too great, even to be restrained by this barrier. A first attack also seemed to preserve from a second.

Indulgence in spirituous liquors appears to have had no certain effect. "Abstinence, as shown on board of the American teetotal ships, was no protection." (p. 60.) The numbers, however, are not given, so that the exact influence of habits of this kind is uncertain. Dr. Davy, in a note, makes the following interesting remark. Of 96 men of the 88th Regiment, attacked with yellow fever, in 1847-8, 52 were called "drunken characters," of whom 18 died; 44 were designated "temperate" and of these, 3 only died. In the 66th Regiment, however, of 98 attacked, 15 died, and of these, 10 were considered temperate;† so that here the temperance did not prove such a safeguard.

It was noticed, also, that anxiety of mind evidently increased the force of the disease, when present, and probably predisposed to its reception. If a sailor was brought to the hospital when the hearse was there, the effect became so notoriously bad, that at last the hearse was obliged to be brought down secretly.

We have now given, as far as our space would permit, a faithful analysis of Dr. Blair's work. In doing so, we have preferred to give his own statements to the reader, without comparing them in detail with the opinions which others have derived from the study of yellow fever in other countries. We have adopted this course as one more consonant with the professed nature of the work under review, and as not leading us again into ground we have so lately trodden in our former articles. We now part with Dr. Blair and his editor, with the conviction that their labours are of much worth. If we have felt disposed to criticise occasionally, it is not that we had not excellent material which we might have praised; but because, with such great powers of observation as Dr. Blair evidently possesses, and with such a field for observation as he has had, we feel that had he investigated this medical problem with the same logical rigour and severity as he would a chemical or an astronomical theorem, the fruits of his labours would have been far more abundant and important. For the power of obser-

\* We are surprised to find Dr. Davy reasoning upon these facts, and making suggestions founded on them, as if they had not been recognised and pointed out over and over again.

† The word temperate is rather indefinite. A soldier's definition of a drunkard is a liberal one, as far as spirits are concerned.

vation in medicine is a kind of tact, which ought to be cultivated with the same assiduity as the chemist practises when he learns how to manage his delicate manipulations, or the astronomer when he wields his wondrous tube. In medicine, the observation and recording of phenomena has been held to be an easy and trifling task, which any tyro was competent to do. Hence, half the error and the uncertainty of medicine. Inaccurate—that is erroneous and incomplete observation—has been the cause that, till within these few years, the fevers of cold countries have been so absolutely uncomprehended, and that the fevers of hot countries are still shrouded in obscurity. The most valuable addition any one could at present make to our knowledge of tropical fevers, would be a simple record of all the cases in an epidemic. These cases should be observed with the keen tact of a Chomel, and recorded with the fidelity of a Louis. We want no explanation or word of comment added to them; we want merely the cases. Then, when the numbers are sufficient, we should certainly begin to put order into this chaos. And let not any one, who may have the opportunities, be deterred from this task, by that fallacious, and, we beg to say, most reprehensible argument, with which some people may favour him, viz. that his cases will be “tedious,” “heavy,” and “unread.” Unread, they will be, certainly, by some of the profession, who consider their routine practice as great an effort as their intellect will bear; but read and analysed we will venture to say they will be, by those who think no labour too great if they can fix safely the foundations of medicine, and for whom, if *accurately* reported, no cases can be too long, no observations too minute. Only before the task is commenced, let the observer feel that his powers are equal to it; and let him bear in mind the example of Louis, who recorded most carefully for a long time, that he might train himself to this duty, and then, throwing his probationary cases aside as too uncertain for use, began to make those remarkable series of observations, which have linked his name for ever with the greatest improvement in modern medicine, the employment of a correct method of studying his science.

It may be well supposed, that holding those opinions as to the uncertainty in which the subject of tropical fevers is still involved, we should believe it to be impossible, at the present moment, to discuss fully what exact relations exist between these diseases and the fevers of our own country. The place of yellow fever, in the nosological chart, is, indeed, no nearer to our typhus or typhoid fever, than that of oriental plague, or even cholera may be. The term “fever” is a conventional phrase, which implies no closer alliance than a certain conformity in some unimportant outward characters. But there are other forms of fever in this country, which approach nearer to yellow fever; and even with our present imperfect knowledge, a comparison of these diseases may be useful. We propose to do this hereafter, or, it may be, to enter upon the whole subject of comparison of the diseases included under the term “fever.” But as a preliminary inquiry, we intend, on another occasion, to place succinctly before our readers, the present state of knowledge of the fevers of this country, which recent investigations have done so much to forward.

## ART. XI.

*The Diseases of the Breast, and their Treatment.* By JOHN BIRKETT, F.R.C.S. F.L.S., Assistant-Surgeon to Guy's Hospital.—London, 1850. 8vo, pp. 264. With Twelve Lithographic Plates.

THIS book is a publication of the dissertation to which the Jacksonian prize for 1848 was awarded by the Council of the College of Surgeons. There can be no doubt that the subject was one well chosen by the Council for competition; no Monograph on Diseases of the Breast having appeared in our language, since Sir Astley Cooper's work was published in 1829. During the last twenty years, the progress of pathological anatomy has been so rapid, that a well-digested account of our increased knowledge of these diseases was much required; an account embodying the substance of what was previously known, with additions made after personal observation, and collection of the scattered reports of other writers. Such an account the announcement of the subject of the Jacksonian prize has drawn forth in the shape of the work we now proceed to review.

The Treatise opens with a short section on the configuration and anatomy of the human mammary glands. In this we are surprised to find Mr. Birkett denying that the nipple contains erectile tissue. He believes that its enlargement during suckling depends upon the dilatation of the ducts within it, and not upon an increased flow of blood to the part; and further, that the contractile property of the nipple depends upon the presence of yellow elastic fibre tissue. Perhaps this explanation of the comparatively permanent enlargement during suckling may be correct; but we all know (and Mr. Birkett subsequently refers to the fact, p. 35), that in the unimpregnated female, under certain states of mental emotion or local irritation, the nipple becomes suddenly enlarged and tense, and as rapidly subsides to its former flaccid condition. This cannot be owing to dilatation of the ducts. Even the nipple of the male is in some degree capable of erection. How this erection can be due (as Mr. Birkett affirms it to be), to the contraction of the "elastic fibre tissue, without any vascular turgescence," he does not attempt to explain. In the case of the nursing female, he tells us that, "When the infant is not at the breast, the nipple is retracted by the contractile power of this fibre-tissue, and resumes its quiescent state" (p. 35); and such contraction, we should have supposed, would produce the same result at any other time. Mr. Birkett takes no notice of the demonstration by Prof. Kölliker of the presence of the "smooth muscular fibre" in the tissue of the nipple, as in the erectile tissue of the genitals. (See p. 244 of the present Volume.)

Mr. Birkett deduces from his observations on the anatomy of the mammæ, that from birth to the age of commencing puberty the glands are in a *rudimentary* stage; that during their development, about the age of puberty, they are in a *transition* stage; that their *fully developed*, but inactive stage, is contemporaneous with the catamenia; that their most perfect condition, or *active* stage, is, after birth of a child; and lastly, that a stage of *atrophy* ensues, "as a consequence of decreasing ovarian energies." In accordance with these stages or conditions, he arranges the materials of his Essay; treating—

- " I. The diseases before puberty.
- II. The diseases during the establishment of puberty.
- III. The diseases after the establishment of puberty.
  - 1. During pregnancy, the puerperal period, and lactation.
  - 2. At any age after puberty." (p. 8.)

We question the propriety of this arrangement. We think it would be far better to treat the diseases of each of the different tissues or structures which compose the breast, stating how these diseases are modified at the various stages of life. Much repetition, and some apparent confusion, might be thus avoided.

In the section on the "Abnormal Conditions and Diseases of the Rudimentary Organs before the commencement of Puberty," a case is quoted of abscess of the breasts of an infant only twenty-five days old, preceded by secretion of milk in both glands. In another case of a female child three days old, swelling of the breasts coincided with sanguineous discharge from the genitals. In a third, at the age of three months, the mamilla was involved in an abscess. And in a girl sixteen months old, irritative discharge from the vulva coexisted with considerable swelling of the right breast.

The "Condition and Diseases of the Gland about the Age of Puberty," is the subject of the second section. The anatomical changes at this period are growth or development of gland-tissue, and deposition of fat. Morbid action is rare; but a case is quoted of medullary carcinoma in a girl only thirteen years of age, who died after amputation of the breast, carcinomatous tumours being found in the liver, lungs, and mesenteric glands. In two other cases, at eight and sixteen years of age, carcinoma has been observed. Instances of precocious development are also quoted.

The third section is entitled, "The Mammary Glands after Puberty; their Anomalies and Diseases." It will be convenient on many accounts to follow the terms used by the author; we therefore extract the following passage, in order that these terms may be clearly understood:

"Varieties in form and shape occur, to some extent; yet a correct knowledge of the usual disposition of the glandular tissue is essential to correct diagnosis. This tissue does not surround the nipple, taking this as an assumed central point, equally upon every side; nor can a gland be bisected into two symmetrical halves by a vertical incision passing through the nipple. With a view to the description of the precise position of tumours, I have been accustomed to divide the gland by a vertical and horizontal line, intersecting each other in the nipple, into four quarters.

"The vertical line divides the gland into the internal *sternal*, and the external *axillary* portion.

"The horizontal line divides it into a superior *cervical*, and an inferior *abdominal* portion.

"In the *sternal*, the gland is not so thick as in the *axillary* half, nor is it, when unsupported, and the figure nude, so much developed in the *cervical*, as in the *abdominal* half. In fact, the *sterno-cervical* quarters gently slope off to the nipple, whilst the fulness and roundness is chiefly peculiar to the *axillary-abdominal* portions. The axillary border of the gland, not unfrequently, feels as if it passed a little behind the axillary border of the *pectoralis major* muscle. There is, in fact, a preponderance of gland tissue in the last-named section of the organ, although, by some writers, this larger size of the lower part of the gland is attributed to the gravitation of the tissue of the organ." (pp. 19-20.)

Anomalies in the number and situation of the breasts are rare; but cases

of both deficiency and excess are recorded. Among the remarks upon supernumerary mammæ we find the following :

"It is stated, that in Idalium, Greece, and Egypt, the occurrence of this deformity is frequent, and it is attributed to the influence which the imagination exerts upon the pregnant woman, whilst contemplating the statues of Isis and Diana, which are represented with numerous mammæ." (p. 23.)

This, if true, would be very curious, and might give rise to much ingenious disquisition. But we doubt the whole of the facts. Who makes the statement? Is the deformity really frequent? Do pregnant women contemplate statues of Diana and Isis in Greece and Egypt? Are these goddesses represented with numerous mammæ? We believe that a pregnant woman might travel all over Greece and Egypt, and not find a single statue of either Diana or Isis with more than the usual number of mammæ; indeed, she would have considerable difficulty in finding one of any description in Greece, and would only meet with low relief, not statues, of Isis, in remote and depopulated parts of Egypt. If by *Idalium*, the author means a small town of that name in Cyprus, the only place of the name we ever heard of, we should be glad to know what proportion of the inhabitants have been observed with superfluous mammæ.

The *Diseases of the Breast of the Adult Female* are next described, commencing with the following observations :

"Of inflammation and its results.—In this organ, as in others composed of similar textures, the stages and the products of inflammation are precisely the same. It must, however, be recollected, that each mammary gland is an association of small glandular masses, or, in other words, that each lobe is a perfect and individual gland, enjoying exclusively its own vascular and nervous system, having its own proper duct, the single orifice of which is at the nipple. A large quantity of interlobular cellular tissue unites these lobes together, and the whole is at last surrounded by the investing fibrous membrane, or mammary fascia. Both around the margins and in front of the gland is a large quantity of fat—the subcutaneous; and there also exists much fibre tissue in the nipple, between the ducts and under the areola. Hence, inflammation and its results may affect, and be limited to, the nipple or areola, the cutaneous and subcutaneous tissue, the lobes, individually or collectively, and the uniting fibro-cellular tissue: it may be either *intra-lobular* or *inter-lobular*, or both combined.

"It would be idle to adduce reasons to prove the importance of attending to the above anatomical facts, when called upon to treat that disease, to which I have applied the term MAZOITIS. The following is the division of the subject which I shall adopt:

- "A. Inflammation and its results in the cutis and subcutaneous tissues covering—
  1. The nipple.
  2. The areola.
  3. The gland itself.
- B. Inflammation and its results in the tissues behind the gland.
- C. Inflammation and its results in the tissues within the investing fibro-cellular envelope or fascia of the gland—
  1. Of the lobes or gland tissue.
  2. Of the uniting tissue." (pp. 28-9.)

Collodion is recommended as a most useful application to fissures of the nipples.

A practical hint of some importance is given with regard to the direction of incisions, made for the evacuation of pus, about the nipple. They should be made in the direction of radii, from a centre formed by the

nipple. The ducts converge to this centre; and the incision being parallel to them, their transverse division is avoided.

We find little else worthy of notice in the section on Inflammation. The author confesses that his distinction of inflammation within the fascia, as affecting the glandular and interlobular tissues separately, is not one which can be made out in practice.

In the treatment of abscess, the author attaches considerable importance to local support, by strapping up the breast with plaster, and then bandaging it. Even before the occurrence of suppuration, he thinks this practice "prevents further mischief." He treats the sinuses which follow chronic abscess, by passing along them a seton or ligature of double silk. One of its ends being passed through the loop, they are tied together, sufficiently tight, every other day, or less frequently, according to circumstances, until all the tissues included in the ligature are divided. The sinuses heal up gradually from the bottom; and the plan is said to be very efficacious, and to have cured many sinuses, which had continued several months.

*Diseases depending upon the development of Cysts and intra-cystic Growths*, are next considered.—These are the *hydatid carcinoma* of Sir C. Bell, *hydatid disease* of Sir Astley Cooper, *sero-cystic disease* of Sir B. Brodie, and the unilocular or multilocular cysts of French authors. The author states, that—

"Two classes of cysts are found in the mammary gland, each characterised by marked anatomical peculiarities.

"Each class also presents fluid or solid contents, perfectly characteristic, and differing essentially in their nature and origin.

"They may be arranged as follows:

1. Cysts depending upon dilatation and a morbid condition of the lactiferous ducts or acini.
11. Cysts produced by a peculiar action in the fibro-cellular envelope of the gland tissue, and the consequence of a morbid state of the function of nutrition." (pp. 64-5.)

The cysts or cystoid formations of the first of these two classes may be developed near the nipple—*sub-areolar*; or in the substance of the gland—*intra-glandular*; and thirdly, on the posterior surface of the gland—*sub-glandular*.

"The simplest form in which these cysts exist is that in which they are commonly found upon the posterior surface of the gland.

"Rarely assuming the state of an important disease, their presence indicates a certain morbid action, and they are the type, as it were, of this class. Hence I shall describe their anatomy and their contents.

"The size varies from an almost imperceptible point to that of a filbert, but, I believe, is rarely larger. Their colour, which depends upon that of the contents, varies from yellow, pale red, or brown, until it becomes almost black, or of a dirty green tint. They possess an external investment of the fibro-cellular tissue, and the lining membrane consists of a very beautiful coherent epithelium. Each epithelial cell is oval, contains a nucleus, and is somewhat granular in texture. Thus, this epithelium closely resembles that of the ducts.

"The contents of these cysts are of a mucous nature, of a pale yellow, reddish, or dull green tint. They exhibit a granular basis, fat globules, milk globules, and colostrum corpuscles, with epithelium. The colour probably depends upon decomposition, or the presence of hæmatin.

"Thus the examination of the contents of these cysts clearly proves their origin;



for, as they exhibit all the substances secreted by the gland, it is fair to presume they have the closest affinity to, if they are not identical with, some portion of the true glandular tissue." (pp. 65-6.)

Mr. Birkett explains the genesis of these cysts on the supposition that a duct is first obstructed, that secretion goes on into it, and that the parietes become consequently dilated, causing a varicose condition, not a general dilatation, but "detached enlargements." The portions of undilated duct gradually contract, until all traces of them are lost; and the dilated portions thus become small detached cysts, imbedded in condensed cellular tissue, their contents becoming more solid.

He next describes a morbid condition, simulating more important diseases, which depends upon dilatation of the larger lactiferous ducts, and the collection of a coherent mass within them of more or less solidity. Thus the tumour consists of a series of dilatations of the ducts, presenting the same structure as the ducts, and communicating with each other by small openings, through which a bristle may be passed. The growth in the interior consists of epithelium scales, nucleated bodies, and colostrum corpuscles. It has no character in common with either the cysto-sarcomatous or carcinomatous new formations. The contents may be withdrawn from the ramifications of the principal tubes, and only appear to adhere to their walls when entering a smaller duct. Mr. Birkett believes, in opposition to the opinion of Sir B. Brodie, that this cystiform affection is a non-malignant or "non-contaminating" disease, and that it may coexist with almost all the forms of the other diseases to which the breast is liable.

*True cyst formations* are next minutely described; and the author, as he states, arrives at the following conclusions, from observations and original researches only:

"1. That certain collections of fluid take place in the areolar tissue of the mammary gland.

"2. That a cyst or closed cavity is formed, and lined with tessellated or hexagonal epithelium.

"3. That the physiological relations of this fluid may be regarded as differing from ordinary serum, and that it may be considered as a superabundant effusion of blastema.

"4. That the intra-cystic growths, being developed within the sphere of nutrition of the mammary gland, present more or less resemblance to the gland tissue, both to the unassisted eye, as well as when examined with more minuteness.

"That, from the observations above made, these growths may be regarded as an imperfectly developed gland tissue; and

"That their tissue presents no resemblance to that of the carcinomata; and that there exists no evidence to prove that it has the power of contaminating other tissues, or of being generated in any other organ belonging to the body.

*Nomenclature.*—To the new growths now described, the term cysto-sarcoma has been applied; and I see no good reason to change the term; remembering, however, that this merely implies the mode of growth, and not their histological affinities, which it is one of the chief objects of this essay to establish.

"The term sero-cystic, applied to the first class by Sir Benjamin Brodie, is, in a practical point of view, sufficiently intelligible, and may therefore be retained; but it is only applicable to these, and not to that condition of the ducts which I have described as varicose and containing epithelial deposits, mingled with the secretion of the gland.

"Sero-cystic sarcoma indicates very accurately that condition of a new growth,

in which both the fluid effusion and solid formation simultaneously exist, and may therefore be retained." (pp. 80-1.)

General observations follow upon tumours consisting of a single cyst, or several cysts containing fluid; of a single cyst with fluid and a solid growth; and of cysts containing solid growths, some with fluid and others without, each variety of disease being illustrated by cases. The section is a decidedly useful one, and is followed by another on the *Diseases depending on Hypernutrition*. *Hypertrophy* of the mammæ is first considered. This chapter need not detain us; and we pass on to what Mr. Birkett calls "Imperfect Hypertrophy." This is described as *lobular* and *general*. Among the tumours composed of imperfectly developed gland tissue, the lobular imperfect hypertrophy holds the first place. It is regarded by the author as synonymous with the pancreatic sarcoma of Abernethy, and the chronic mammary tumour of Sir Astley Cooper, being a "*new growth* of more or less perfectly formed gland tissue, developed upon a lobe of the breast." (p. 124.) The gland tissue may be almost perfect, or very imperfect. In some cases the tumour precisely resembles the rudimentary condition of the gland in the male, consisting merely of areolar tissue and secreting terminal cells. In others, the terminal cells are much larger than natural, and are united together by normal cellular tissue, but no ducts can be detected. From this close approach to the true gland tissue, development gradually retrogrades, until the new growth can no longer be distinguished from cysto-sarcomatous tumours. In one interesting case a tumour was composed of tolerably well-formed glandular tissue, the same imperfectly developed, normal fibro-cellular tissue, and imperfectly formed fibro-cellular tissue; all being inclosed together in a fibrous capsule, to which the normal tissue of the breast was firmly adherent. Here, in one specimen, the various stages through which these tumours pass, could be observed.—The true *painful tumour* of the breast Mr. Birkett regards as one variety of lobular imperfect hypertrophy, and, being a new growth, is to be distinguished from lobular chronic induration of the normal tissue. He has himself traced nervous filaments into new growths, consisting of more or less perfect gland tissue, surrounded by condensed cellular tissue in which the nervous filaments ramified. He believes that all the cases called "fibrous tumours," are examples of some variety of lobular imperfect hypertrophy, the elementary gland tissue predominating, although there may be also an hypertrophied condition of the fibrous tissue. Imperfect hypertrophy may also be *general*; that is to say, the tumour may have completely usurped the place of the whole of the normal mamma. Several instances of this condition are quoted.

Under the head of "*Hemorrhagic Diseases of the Breast*," we find a few remarks on superficial ecchymosis, or discharge from the nipple during suckling in very weak women, and on effusion into the cellular tissue, causing tumours composed of fibrin. In the treatment of what may be called "vascular developments," or tumours composed of "fibre tissue so arranged as to form a reticulated structure, or an immense assemblage of minute cells communicating with large veins," Mr. Birkett recommends strangulation of the entire mass, by passing ligatures under the tumour in such a manner as to arrest the supply of blood without destroying the skin.

After some cases illustrating the morbid development of fat around and

behind the gland, within its fascial envelope, and between its lobes, the author passes on to the DISEASES OF INNERVATION, which he divides into two classes:—1. *Without induration*, with or without temporary enlargement; and 2. *With induration*, general or partial, which is often, but not always temporary. We observe nothing original in the account of these diseases, except that in two breasts removed on account of induration and extreme morbid sensibility, Mr. Birkett “found the glandular tissues atrophied, the ducts in some parts dilated, a few scattered and very small duct-cysts containing fluid, and the uniting areolar tissue very much condensed and indurated.” (p. 169.)

In a short section on Atrophy of the Breasts, cases are quoted from French writers to illustrate the action of iodine upon the mammary glands; but not one word on this important subject is added from the author's personal observation. During sixteen years' experience at Guy's, he must surely have had ample opportunities of judging what this alleged action amounts to, and ought to have informed his readers of the result of his experience. In his account of true hydatid cysts, however, some reparation is made by the narration of the following case, the first on record in which the presence of echinococcus has been demonstrated in a mammary cyst.

“Case LXXXI. *Hydatid cyst in the breast, containing echinococci; removed by Mr. B. Cooper.*—October 28th, 1846. Mary Doyle, æt. 51, had a lump in her left breast six years. She never suffered any pain in it, but wished to have it removed, as it was a source of annoyance. It felt firm, but not hard. When removed, an incision was made into the tumour, and some clear fluid escaped. The collapsed cyst presented all the characters of an hydatid. Upon its internal surface I saw minute projecting bodies, which, under the microscope, displayed a very beautiful arrangement of concentric laminae. I detected echinococci with the microscope, and an abundance of their peculiar tentaculæ.

“It was equal in size to a walnut of ordinary dimensions.” (p. 183.)

Several instances of variable, temporary, and sudden increase in the size of the breasts, of serous infiltration of the areolar tissue surrounding them, and of “*hydrops saccatus mammæ*,” are quoted; but Mr. Birkett has never seen an instance of any of these conditions. Nor are there any additions to our previous knowledge in the succeeding sections on the diseases of lactation, and the deviations from the normal condition of the nipple and areola. We therefore pass on to the account of the “Diseases depending upon Carcinomatous Degeneration of the Mammary Glands.” The anatomical and physiological history of these growths is not attempted, the object of the author being “to consider the various forms of this disease with which the surgeon meets in the breast; and, after pointing out a few of the anatomical differences, to state what appears to be the most appropriate plan of treatment to be adopted in particular instances,” (p. 211.)

The following are the varieties of carcinoma which we are told are met with in the female breast:

1. Lobular carcinoma; attached to or involving only one lobe.
2. Intra-glandular carcinoma, infiltrating the entire gland.
3. Encysted carcinoma, generally *Carcinoma medullare*.
4. Carcinoma of the nipple.
5. Carcinoma, commencing on the skin, either of the nipple, areola, or over the gland.” (p. 212.)

After a general history of the progress of ordinary cases of carcinoma, some interesting information is given in a statistical form, deduced from notes of 147 cases, we presume from personal observation, although we are not told so, nor is any guide afforded us as to the nature of the cases. In these 147, the earliest age at which cancer was developed was 8 years, only 3 cases occurring between 10 and 20. Before 38 it is a comparatively rare form of disease of the breast. Between this age and 51 it is very common, 70 cases out of 147 occurring between 39 and 50 inclusive. M. Lebert found a larger number of cases between 50 and 60 than between 30 and 40; namely, 29 at the former, and 20 at the latter age, out of 91 cases. In Mr. Birkett's notes this is reversed, the numbers being 29 and 32 out of 147. In Dr. Lever's analysis of 120 cases of cancer of the uterus, the age between 40 and 50 was also the most liable to the disease. With reference to the influence of marriage and sterility, the common belief that celibacy and sterility favour the development of cancer is refuted by Mr. Birkett's notes of 116 cases. Of these 79 were married, but 37 single; and of 55 married women, 47 were prolific, and only 8 sterile. This is again supported by Dr. Lever's 120 cases of cancer of the uterus, of which, while married women stood in the proportion of 86·6 per cent., and widows 7·5 per cent., single women only suffered at the rate of 5·83 per cent. The right and left breasts were affected in nearly equal numbers. Both breasts were affected in only 17 cases of 140.

The following extract upon contamination of the axillary glands will be read with interest :

"It appears that, in the majority of cases, the affection of the axillary lymphatic glands is contemporaneous with the implication of the skin during the second stage of development; for, of 64 cases, in which axillary enlargement occurred, in one only the skin was not affected; in 29, the skin was involved in the diseased mass; in 11, ulceration was commencing; and in 23, ulceration was established.

"I have had an excellent opportunity of demonstrating the manner in which small carcinomatous tubercles are developed in the course of the lymphatic ducts.

"The subject was an elderly woman, in whose breast was a very hard and chronic carcinomatous growth. In its neighbourhood were several small oval tubercles; and, upon dissecting the lymphatics on the posterior surface of the pectoralis major muscle, the minute tubercles were seen at distances from each other along these vessels. This preparation is in the museum of Guy's Hospital." (pp. 231-2.)

With regard to the important question, at what stage carcinomatous tumours may be exercised in the hope either of prolonging life or diminishing suffering, Mr. Birkett believes, that while removal is followed by the greatest success when performed in the early stages of disease, and that the chance of success diminishes with the development or progress of the disease, still that life may be prolonged and great suffering saved by operating even when extensive ulceration has taken place. He believes, also, that when these tumours are of a very chronic character, almost certain freedom from disease, for some years, will follow their removal; while in cases of rapid development, the operation is almost sure to be followed by relapse. Those cases are the most successful, in which the disease has passed through its successive stages at the slowest rate.—In these opinions we entirely concur; and we believe they are held by most practical men, who wish to avoid the errors to which a too strict adherence to the doctrines of certain modern pathologists would lead. We fully recog-

nise the constitutional origin of the disease, or the contamination of the fluids and organs of the body generally, and the occasional rapid development of carcinomatous growths in internal organs after removal of external cancer; but we have also been led by experience to believe, that in many cases the external deposit has freed the blood from all the morbid matter it contained; and that the removal of this deposit has prevented constitutional contamination, or secondary changes in the blood, which would have taken place had the tumour been allowed to go through its various stages.

The following opinions of the author regarding excision are worthy of notice.

"1. When the disease is intra-glandular and in the first stage, the entire organ had better be removed. It is the more safe proceeding.

"2. When in the same situation, and in the second or third stage, the removal of the entire gland is indispensable. In both cases, a portion of the skin over the tumour should be excised, leaving sufficient to prevent tension upon the newly cicatrized parts.

"3. When extra-glandular, and, in the first stage, the tumour, as well as the lobe of the gland to which it is affixed, should be removed.

"4. When in the same situation, but in the second or third stages, the same parts, with plenty of skin, must be taken away. I believe, however, it is more safe to remove the entire gland.

"5. When the disease commences in, or immediately beneath, the skin, excision of the new growth must be speedily performed, and a considerable portion of the surrounding cutis be removed with it.

"6. If the disease commences in the nipple, and this organ is destroyed, and there are no indications against an operation, I believe the only safe plan is the removal of the whole breast." (pp. 238-9.)

With regard to the Anatomy of Cancer, Mr. Birkett looks upon the development of nucleated bodies between the fibres of areolar tissue, as the essential element of carcinoma. The new growth varies in consistence with the relative proportions of those elements to each other. In some of those growths, "the minute bodies, *nuclei*, predominate," in others the "*nucleated globules or cells* seem to constitute the chief material between the fibres." The fibrous carcinoma "presents a large quantity of fibre tissue, with a preponderance of nucleated globules." The medullary carcinoma contains "a very small quantity of fibre tissue, with a preponderance of the *nuclei*."

The only other observation we shall add on the subject of cancer, is that on post-mortem examination of the bodies of 37 persons who died in Guy's Hospital with carcinoma of the breast, the bones were also affected in 11 cases; the fibrous tissues in 23, including 17 in which the serous membranes were diseased; the organs of innervation were not affected in any case, the veins in but 1, the lymphatic glands in 19, the liver in 14, the kidneys in 4, the lungs in 7, the spleen in 2, the pancreas in 2, the uterus and ovaries in 8. In 12 cases serous effusions were observed, and in 4 cases pleural effusion was inflammatory.

The work is concluded by a few pages on the Diseases of the Male Breast. The only fact we need notice is one of some interest in connection with the assertion, that males have secreted milk. Mr. Birkett accidentally found in the dissecting room a man whose mammary glands were largely developed, and states that they were composed of a "large quantity of fibrous

tissue, forming a stroma, and of the terminal vesicles of gland tissue, which contained very minute epithelial cells. Here we find all the essential elements of a gland, but in a rudimentary form." (p. 255.) It will be remembered, that this rudimentary glandular tissue has been described as forming the substance of tumours in the female breast.

Having given so full an analysis of this work, some critical opinion as to its merits and value may be expected from us. We do not withhold this opinion, although we find it rather difficult to convey the exact expression which its perusal has left upon our minds. Our readers will be able to form their own judgment as to the value of the information it contains, from the extracts we have submitted to them; and we doubt not that this judgment, like our own, will be highly favorable; but it must be confessed that these extracts form some of the most valuable portion of the book. We have already objected to the arrangement, and might object still further to the somewhat confused method in which even this arrangement is carried out, and to a style which gives one the idea that the author cannot clearly express his own meaning, or exactly define what he wishes to convey. We have also been surprised to find how few among the many cases brought forward to illustrate the opinions of the author, are drawn from his own experience in Guy's Hospital. Surely after so many years spent at that enormous establishment, Mr. Birkett need not have gone so frequently to books of the last century, and to obscure continental writers, for facts in support of his statements. We looked for rather less of mere compilation, from a surgeon blessed with the opportunities for extensive observation and original research enjoyed by Mr. Birkett; and have been proportionally disappointed at finding his own contributions to surgical science, valuable as we have represented them to be, less important than we think we had a right to expect. Not only are the additions to our previous knowledge of the minute anatomy of the diseases of the breast, more scanty than the author appears to consider them in his Preface; but there is a deficiency where it might have been least expected, namely, in points of practice. The means of diagnosis between the different varieties of mammary tumours in the living body are by no means fully detailed; nor are the methods of treatment described in a manner to be wished for from a clinical surgeon. Even the application of fluid pressure, so admirably and successfully adopted by Dr. Arnott, is neither described nor appreciated. Many interesting subjects are treated of far more cursorily than is satisfactory; and not unfrequently abnormalities are mentioned apparently for no other purpose than that of giving them a name.

Mr. Birkett must forgive us if we appear a little severe in these strictures. They would not have been made, had the book come before us without the *prestige* of having received the Jacksonian Prize by the award of the Council of the Royal College of Surgeons, or had it been the work of a man in a less distinguished position. We trust that he will take them in good part, as a hint that high station brings with it responsible duties, that the performances of a London hospital surgeon are scrutinised with somewhat jealous care, and therefore that in justice to his own fair fame he will endeavour, in his future publications, to correct defects which we should most gladly have suffered to pass unnoticed in an author of less sterling merit.

## ART. XI.

*On the Theory and Practice of Midwifery.* By FLEETWOOD CHURCHILL, M.D. M.R.I.A., &c. &c. With 105 Wood Engravings. Second Edition, corrected and improved.—London, 1850. Fcap. 8vo., pp. 496.

THIS Manual has been sufficiently long before the public, to allow its merits to be adequately considered; and we believe that we state the general opinion of the profession, as well as our own, when we say, that it may justly lay claim to the merit of laying before the student a condensed view of the present state of the science and art of Obstetrics; whilst its chief fault, like that of the 'Treatise on the Diseases of Children,' by the same author, reviewed in our last Number, is that in bringing together a valuable collection of the opinions of various writers, more or less distinguished, upon important points of physiology and practice, it too frequently leaves the student (for whom it is avowedly intended) insufficiently informed as to the author's own views, and the line of conduct he would himself pursue in a given emergency. We shall adduce an illustration of our meaning, from one of the novelties of the present edition, namely, the discussion of the merits of the treatment of *placenta prævia* recommended by Drs. Simpson and Radford. Nearly five pages are occupied with a summary of the controversy which has taken place on this point, and with an examination of Dr. Simpson's statistics; and the following is Dr. Churchill's conclusion:

"I have thus examined with care this very difficult subject; and although I would be very far from pronouncing dogmatically upon it, I feel bound in duty to state that, except in the cases I have mentioned, I could not consent to substitute the new method of treatment for the old, and even in those cases I would recommend the utmost caution."—(p. 412.)

Nine classes of cases are mentioned by Dr. Churchill; and we cannot but believe that the student would be sorely puzzled to make out precisely what are those in which Dr. Churchill would "consent to substitute the new method of treatment for the old," and what are the "cautions" which he would interpose. Believing, as we do, that there is a class of cases in which the new method is greatly to be preferred to the old, and that the prompt and judicious adoption of the practice in question has saved many lives, and is likely to save many more, we feel the greater anxiety that such knowledge on the subject as it is thought desirable to communicate to the obstetric student should be definite in its character. And we are sure that if Dr. Churchill had kept his analyses and estimates of the data obtained by Dr. Simpson and others, for his own private consideration, and had furnished his readers with his conclusions thereon, in a form suitable to their guidance through the difficulties of practice, he would have conferred a much greater benefit on them than he has done by the introduction of a summary, concise and well-executed as it is, of the leading points of the controversy. The same remark we would make on the statistics which are introduced in various parts of the book. We fully coincide with Dr. Churchill as to the estimate of their value which he gives in his preface; but we think that the Critical Dissertation on the comparative merits of different modes of practice is rather the place for them, than the Student's Manual, in which the learner has a reasonable expectation that he shall

find rules laid down for his guidance, rather than the data on which his teacher thinks that such rules may be constructed.

Since the publication of the first edition of this work, great advances have been made in our knowledge of the physiology of the female generative system; and of these Dr. Churchill has in many instances availed himself. Several time-honoured errors, however, are retained; such as the statement that the menstrual secretion "differs from blood in containing no fibrine" (p. 50); and that "not only does each impregnated vesicle give rise to a corpus luteum, but nothing else does, at least in the human subject, so that the presence of a corpus luteum is a proof of impregnation." (p. 73.) On the former of these points, we would refer our readers to the researches of Dr. Letheby and Mr. Whitehead, and our own remarks thereon (Vol. I, p. 216); and on the latter to almost any recent physiological treatise,\*—it being now generally admitted, that corpora lutea differing in no respect but size from those consequent on impregnation, may be formed after the ordinary maturation and discharge of an ovum.

In the description of the decidua, again, the Hunterian doctrine as to the formation of the decidua reflexa is still retained, as if it had never been rendered doubtful by the microscopic researches of Professor Goodsir, as well as by other considerations; and we do not find the least allusion to Professor Goodsir's researches on the structure of the placenta, whilst Dr. R. Lee's exploded doctrine of the entirely foetal nature of that organ, and of the absence of any direct communication between the placenta and the uterine blood-vessels, is still retained, as if for the express purpose of puzzling the student, who cannot be supposed to know that Dr. Lee's views, confirmed as they seem to be by the authority of eight names (not to mention an *et-cetera*) cited in their support, are no longer held by a single physiologist of repute. It is well known that the researches of Dr. J. Reid, Professor Weber, and others, have *entirely settled* this question, as far as the *venous* communication is concerned; why, then, is the student dragged through the mazes of the whole previous discussion, and left in doubt at last? The whole treatment of this subject shows no evidence of any new information gathered upon it by Dr. Churchill since he penned his first edition; and yet the work of Messrs. Baly and Kirkes, to which we have already referred, would have enabled him to supply very recent and accurate information upon the whole of this department, without taking the trouble to look elsewhere. We may mention that we have lately learned that Professor Schröder Van der Kolk availed himself of the many opportunities of examining into the connection of the placental and uterine vessels, afforded by the peculiar fatality of cholera among women far advanced in pregnancy; and that he has not only confirmed the accuracy of the statements of Reid, Weber, &c., but has clearly made out the *arterial* connection to be very much what the Hunters had described it.

In the chapter on Parturition, Dr. Churchill introduces a notice of Dr. Tyler Smith's hypothesis of ovarian excitement as the determining cause of ovi-expulsion, on which he thus remarks: "Admitting that ovarian excitement thus excites uterine action, I do not think Dr. Smith

\* See, for example, Messrs. Baly and Kirkes's '*Recent Advances in Physiology*,' in which it is laid down (p. 57) that a body possessing all the essential characters of a corpus luteum, but not larger than a small pea, does not afford any evidence of impregnation.



has satisfactorily explained the cause of that excitement occurring regularly at the tenth menstrual period rather than any other." We are glad to see even this degree of caution in the acceptance of Dr. T. Smith's doctrines, which seem to have gained a considerable degree of currency among obstetricians who are not accustomed to weigh the value of physiological evidence; but we should have been glad to learn from Dr. Churchill, why he is so ready to admit that "ovarian excitement thus excites uterine action." So far from this doctrine being supported by further inquiry, it has been, we understand, entirely disproved, and the validity of all our objections to it fully sustained, by the experiments of Professor Simpson, who has removed the ovaries from sows already pregnant, and found that parturition came on at the usual time, and in the accustomed manner. We hope that the detailed results of these experiments will be soon made public.

We have a great confidence in Dr. Churchill's good sense, when he fairly sets himself to master a subject; and as we have had so much occasion to find fault with him for not sufficiently individualising his instructions, we gladly present a favorable specimen of an opposite kind, from his observations on the administration of Chloroform. He first gives the inferences which appear to him fairly deducible from the *facts* recorded by others, irrespective of the *opinions* of the various writers who have engaged in the controversy; and then offers the practical conclusions which may be regarded as his own opinion, "formed after much thought and reading, and after some slight personal experience." "I would not wish," he continues, "to put them forth dogmatically, for I do believe that we are not yet in a condition to define accurately, or to speak positively on the subject. I confess that I can neither agree with those who think that chloroform can do no evil, and therefore ought to be used in every case, nor yet with those who regard it as in all cases injurious, and therefore to be reprobated:"

"1. In most *obstetric operations*, anæsthesia appears to me to be of great use, not so much because it is supposed to relax the soft parts, or to moderate uterine action, as because it enables the patient to bear the additional pain we inflict without outcry or movement. It surely must be a great advantage in performing a dangerous operation, that the patient should lie still, and not by her struggles increase our difficulty, and the risk of injury to herself. If the tissues be relaxed, which is doubtful in many cases, it is of course an additional advantage; and if it happened in a case of turning that the uterine action were suspended, of course the operation would be all the more easily completed; but these are rather accidental advantages than essential consequences. In operative midwifery, therefore, chloroform may be given until anæsthesia is produced, before commencing, and its effects may be kept up during the operation, *provided* that there be no counter-indication to its use, and that no unpleasant symptom arise; in either case it should be given up altogether. In any operation for terminating labour in a case of convulsions, I should be unwilling to use chloroform, on account of the nervous excitement it occasionally produces, notwithstanding that it is said to have been employed beneficially in the treatment of that disease: in like manner I should fear to use it in cases of alarming hemorrhage, lest it should give rise to severe collapse. I mention these cases as illustrative of the caution which appears to me necessary in the present state of our knowledge. Further experience may prove this reserve to be unnecessary, or may confirm its propriety.

"2. As to its exhibition in *natural labour*: as I do not believe that in the large majority of cases, convalescence is at all impeded by the suffering, I cannot see the

*necessity*, or even the propriety of urging the employment of anæsthesia in every case; and I do feel that even greater caution ought to be used than in operative midwifery. We may be justified in running some risk where an important point is to be gained, such as perfect quietness during an operation, which we should not be justified in incurring merely to relieve pain. Thus in hysterical or nervous patients, in those labouring under nervous affections, or organic disease of the lungs or heart, &c., I do not think we ought to employ it.

"But on the other hand, as pain is undoubtedly an evil in itself, if there be no counter-indication, and if the suffering be either great or prolonged, I cannot see that we are prohibited from the employment of anæsthetics, more especially as it is not necessary in such cases to produce insensibility. It is quite possible to afford immense relief, to 'render the pains quite bearable,' as a patient of mine observed, by a dose which does not produce sleep or impair the mental condition of the patient, and which all our experience would show is absolutely free from danger.

"In my own practice I have never urged a patient to use chloroform in natural labour, and, on the other hand, I have not felt justified in refusing a moderate dose of it when the patient urgently desired it, and none of the conditions were present which seemed to me to counter-indicate it.

"The period at which it has been administered varies with different practitioners; some commence before the os uteri is dilated, others about the time the head escapes through it. There can seldom be any necessity for its use, I think, before the os uteri is fully dilatable, and it is more likely to interfere with the uterine action at an early than a later period. At the commencement of the second stage would, I should think, be soon enough, and this seems to be Dr. Simpson's practice.

"There is a difference of opinion as to the extent to which the anæsthesia should be carried. Professor Simpson prefers inducing complete insensibility at first, and then keeping up just so much of the effect as he deems advisable. Dr. Rigby prefers commencing with smaller doses in natural labour, and increasing them if necessary; and the Obstetric Committee of the American Med. Association, in their Report, agree with this view. Of course, if we are to operate, the patient should be placed thoroughly under the influence of the chloroform before we commence, and its effects kept up by occasional inhalation. But in ordinary cases, as I have said, I prefer beginning with a moderate dose and watching its effects, and if necessary, increasing the anæsthesia.

"The dose should be administered at the beginning of each pain, and increased when the head is passing over the perineum. The anæsthetic state may be kept up for hours without mischief, especially when complete insensibility is not required.

"I have tried various modes of administration, instruments specially contrived for the purpose, sponge, lint, &c., and I believe that by far the best is the one originally proposed by Dr. Simpson, viz. a clean white pocket-handkerchief folded funnel-shape; into which half a drachm or a drachm of the chloroform is to be poured, and which may then be placed near the mouth of the patient, and after a few respirations, over both mouth and nose. It is a good plan to allow the patient to hold the handkerchief herself, unless we wish to produce deep anæsthesia, as it will fall from her hand when sleep commences." (p. 253-4.)

We are strongly inclined to the belief that, on the whole, Dr. Churchill's view is among the most sensible that has been put forth on this subject: to which, however, we shall take an early opportunity of recurring more in detail.

## ART. XII.

*Confession of Professor John W. Webster.* (Boston Evening Traveller, July 3, 1850.)

It has probably appeared to our readers, that the account which we gave of the trial of Dr. Webster, in our last Number, terminated somewhat abruptly (p. 31). The terrible drama, however, had not then reached its close. The curtain had fallen for a time, on the conclusion of the trial and the delivery of the verdict; but the last and most solemn act remained; and we preferred awaiting the proceedings which had still to take place, without endeavouring to anticipate, by any speculations of our own, what the course of them might be. These proceedings have been fraught with such deep interest, not only as regards the result to the wretched convict himself, and the judicial question of his criminality, but also with respect to those more general doctrines of evidence and proof, which we employed this remarkable trial to illustrate, that we think our readers will not be unwilling to be made acquainted with them more fully than they are likely to have been through other channels of information; and we shall endeavour to guide them in their consideration of the value to be attached to Dr. Webster's subsequent statement, by the same general principles as those by which we tested the validity of the first result.

The delivery of the verdict of the jury, unanimously agreed upon with very little hesitation (as shown in the account of its proceedings given by one of the body, see p. 31), affected the prisoner far more strongly than any of the previous proceedings had done. Throughout the trial, he had maintained a calm demeanour and hopeful expression; and it was obvious, that he placed great reliance on the facts and arguments advanced in his defence. When the Foreman pronounced the word "Guilty," however, "the prisoner started like a person shot; his hand dropped upon the rail in front, his chin drooped upon his breast; and after remaining thus a moment or two, he sank into the chair, covering his eyes with his hands. A death-like silence followed, and all eyes were fixed in sadness on him whose hopes had now fled. For nearly five minutes the prisoner remained in this state, apparently unconscious, when Judge Merrick, his counsel, went and conversed with him." The next morning, when brought up for sentence, "his appearance betokened extreme melancholy; he was downcast and nervous, and appeared to be suffering from terrible emotions." The address of the Chief Justice, whilst expressing the complete concurrence of the Court in the verdict of the jury, and his own conviction of the justice of the sentence which he was called upon to pronounce, was full of feeling for the condition of the unhappy prisoner.

"As we approach," he concludes, "this last sad duty of pronouncing sentence, which is indeed the voice of the law, and not our own, yet, in giving it utterance, we cannot do it with feelings of indifference, as a mere formal and official act. God forbid that we should be prevented from indulging and expressing those irrepressible feelings of interest, sympathy, and compassion, which arise spontaneously in our hearts; and we do most sincerely and cordially deplore the distressing condition into which crime has brought you! And though we have no word of present consolation, or earthly hope, to offer you, in this hour of your affliction, yet we devoutly commend you to the mercy of our Heavenly Father, with

whom is abundance of mercy, and from whom we may all hope for pardon and peace!" (Report of the Trial, &c., p. 306.)

During the speech of the venerable Judge, continues the narrative, "it would have been difficult to determine which was the most affected, himself or the unfortunate man to whom his remarks were addressed." "The prisoner's deportment, after his return from his cell, was marked by calmness, and he seemed much bowed down by affliction. Thoughts of his family were uppermost in his mind."

It is the custom in New England to allow a considerable period to elapse between sentence and execution; and it was soon apparent, that Dr. Webster and his friends were not disposed to a passive acquiescence in the decision of the Court, but that they would take every possible means to prevent it from being carried into effect. Various measures were adopted with this view. A "writ of error" was moved for, on technical grounds, in the Supreme Court; by which it was hoped that a new trial might be obtained. What could have been expected, however, from a new trial, save delay, we cannot see; the jury having been almost of Dr. Webster's own choosing, and neither they nor the judge having shown the least partiality; whilst the prisoner's counsel had done all that learning, eloquence, and skill could accomplish in his behalf. This attempt was unsuccessful.

At the same time, an earnest appeal for pardon was made to the Governor of the State, by Dr. Webster's wife and daughters, who expressed the firmest conviction of his innocence. And on the 24th of April, the following petition was presented by Dr. Webster himself, which we give in full, because our readers ought to have it before them, when they apply themselves to the consideration of the value to be attached to Dr. Webster's subsequent confession.

"PETITION FOR PARDON.

*"To his Excellency George N. Briggs, LL.D., and to the Honorable Council of the State of Massachusetts.*

"Having been convicted before the Supreme Judicial Court of the murder of Dr. George Parkman, I would most respectfully and humbly petition your Excellency and the Honorable Council, to be permitted to declare, in the most solemn manner, that I am entirely innocent of this awful crime; that I never entertained any other than the kindest feelings towards him; and that I never had any inducement to injure in any way him whom I have long numbered among my best friends.

"To Him who seeth in secret, and before whom I may ere long be called to appear, would I appeal for the truth of what I now declare, as also for the truth of the solemn declaration, that I had no agency in placing the remains of a human body in or under my rooms in the Medical College in Boston, nor do I know by whom they were so placed. I am the victim of circumstances, or a foul conspiracy, or of the attempt of some individual to cause suspicion to fall upon me, influenced perhaps by the prospect of obtaining a large reward.

"When first charged with this dreadful crime, I did not publish to the world a declaration of my innocence, or any explanation of the circumstances tending to bring suspicion on me, solely in consequence of entire ignorance of the course I ought to adopt, and implicit reliance on the calmer judgment of others. I had, however, prepared for publication a document to that effect; but as there was a strong disposition, from the first, to misinterpret and misrepresent my every look, action, and expression, it was deemed most advisable for me to preserve and maintain silence; the document was therefore, with no little struggle on my part, withheld. Immediately upon my arrest, every means was resorted to, to bend even

the most trifling appearances in my laboratory, and insignificant circumstances, to add to suspicion, and to pervert them to my disadvantage.

"In the state of mind in which I was, silence was constantly urged upon me, and I complied, more strictly perhaps than I ought to have done. Every method of poisoning the public mind, and of exciting prejudice against me, was resorted to; falsehoods, imputations, and fabrications were daily diffused; and I soon perceived that the contradiction of one would lead to others, and that the refutation of them all would be an endless task. I therefore submitted in silence and resignation, believing that the time must shortly arrive when He, who bringeth light out of darkness, would cause the truth to appear, and my innocence be made manifest to all.

"Had I previously been aware of the use that was to be made of some circumstances on my trial, to give an unjust and erroneous impression, if unexplained, I should have been provided with evidence to explain them most satisfactorily.

"Some of the statements, references, and circumstances, however, could not be fully explained or disproved without the testimony of my wife, of which, unfortunately, I could not avail myself. I now pray your Honours that the evidence may be reviewed by you, and that the testimony of my wife may be heard and received, as also my own statements and explanations.

"Repeating in the most solemn and positive manner, and under the fullest sense of my responsibility as a man and as a Christian, that I am wholly innocent of this charge, to the truth of which the Searcher of all hearts is a witness, I would humbly and respectfully pray, that the privilege I have asked may be granted. I do this under the full belief, that the testimony and explanations I would now offer, are such as will disprove many things, impair very greatly the evidence of at least two witnesses, and place in their true light circumstances now obscure.

"On this review of my case, your Honours will, I trust, find sufficient reasons for reversing the decision of the Court, and for the interposition of mercy.

"The knowledge of my feelings and habits, and of my various engagements and occupation of time, both before and after the disappearance of Dr. Parkman, have, from the first, been sufficient assurance to my afflicted family of my innocence; and neither that, nor their trust in Him who has sustained both them and me in our days and nights of sorrow and sadness, have been shaken by the unlooked-for result of my trial. They would pray to be permitted to unite with him who is their sole earthly dependence, in this petition, believing that your Excellency and the Honorable Council will find sufficient grounds for granting to me a pardon, and of restoring to them the husband and father,—for which I most respectfully and humbly pray.

(Signed)

J. W. WEBSTER."

"Boston; April 24, 1850.

Could any language more forcibly assert his *entire innocence* of the crime imputed to him, than the solemn appeals which Dr. Webster makes to the Maker and Judge of all, as the witness to his truth? And can any reliance whatever be placed upon the subsequent assertions of a man, who would thus implore the Almighty's sanction to a series of direct and positive falsehoods? And, be it observed, that the falsity lies, not merely in his own declarations of innocence, but in the assertion that he is the victim of a "foul conspiracy;" that "some individual" has been attempting to cause suspicion to fall upon him, "influenced, perhaps, by the prospect of obtaining a large reward;" that "every method of poisoning the public mind, and of exciting prejudice against him, was had recourse to;" and that "falsehoods, imputations, and fabrications, were daily diffused;"—all this obviously pointing to Littlefield, the janitor of the Medical College, of whose entire truthfulness in the statements made against himself, he was all this time as well aware, as he was of his complete innocence of the

crime which he would lay to his charge. Can any picture of moral depravity be darker than this?—This petition was withdrawn on the 4th of June, for reasons which will presently appear.

Another effort was made by some of Dr. Webster's friends in the Legislature, to get his punishment commuted by a change in the law. In one of the adjacent states (that of Maine, we believe), capital punishment, even for murder, is now virtually, although not nominally relinquished; for its law requires, that an interval of *twelve months* shall elapse between trial and execution; and when this length of time has passed, there has been such a general feeling against carrying the sentence into effect, that we believe it has been of late commuted in every instance. It was proposed to assimilate the law of Massachusetts to that of Maine; and in this way it was hoped that Dr. Webster's life might be first prolonged, and, finally, saved. This move, however, was of no avail.

We do not know the precise date at which it became apparent that neither of these attempts would be successful; but it was not until all hope from other means had been given up, that Dr. Webster made an entire change in his tactics. On the 2d of July, the following petition was presented:

*"To his Excellency the Governor, and to the Honorable Executive Council of the State of Massachusetts.*

"John White Webster, a convict, under sentence of death, in Boston jail, in behalf of himself, and of his wife and children, respectfully petitions, that the sentence awarded against him by the law, may be commuted to such other less horrible and ignominious punishment as your honorable body may mercifully decree.

"Your petitioner fully admits, that he was tried before a fair and impartial tribunal; and that under the law as it exists, his jury, composed as it was of honorable and high-minded men, could have returned no verdict other than they did. But he respectfully reminds your honorable body, that the two great moral ingredients of the crime of murder, malice and premeditation, have never been found against him by a jury, but have been necessarily inferred by the arbitrary rules of the law, from certain general facts which your petitioner will not deny, but the extenuating details of which, no man in your petitioner's situation can ever possess legal evidence to prove. These details your petitioner has confided to the friend who presents his petition, with authority to state them to your honorable body, in the hope that you will find therein reason to extend to your petitioner and his family, that mercy of which the law has made you the dispensers.

*"Boston; June, 1850.*

J. W. WEBSTER."

The petition was referred to the Committee on Pardons, of which the Lieutenant-Governor was chairman; and the gentleman referred to by Dr. Webster,—who proved to be the Rev. Dr. Putnam, a much-respected clergyman of Boston,—was admitted to state the case in Dr. Webster's behalf. This, he assured the committee, he came forward to do, "not as an advocate pledged to a side, but in good faith as expressing his own personal belief." We think it of importance, that the circumstances under which Dr. Webster's confession was made to him, should be taken into account, in estimating the credit to be attached to that statement; and we shall, therefore, give Dr. Putnam's account of them in his own words:

"My acquaintance with Dr. Webster before his trial had been of the slightest and most casual kind. Soon after his sentence, I received from him a request, that

I would visit him as a clergyman during his imprisonment. It was a service not to be declined.

"I had followed the reports of the trial, and acquiesced in the verdict as a righteous one; and had no thought but that the sentence was to be, and ought to be, carried into execution. I did not make it my object to draw a confession from him early, or to lead him to commit himself one way or the other, on the question of his guilt or innocence. I carefully avoided every remark and inquiry that might tempt him to make any false declaration. He seemed to understand me, and neither denied nor declared his guilt. I expected he would finally be induced to communicate to me whatever he knew about the disappearance of Dr. Parkman, and about the remains found at the college. But I was in no hurry about this. I thought I should be more likely to obtain from him the exact truth, by waiting till a favorable time. Accordingly it was my object for the first weeks to become acquainted with him, to win his confidence and attachment, by attention and sympathy, and to endeavour to make those impressions of a moral and religious nature, which were suited to his situation as a more or less sinful and certainly dying man. As time passed, I seemed to myself to have succeeded in these objects, almost beyond my hopes.

"At length, on the 23d day of May, I had made up my mind to address him in a wholly new strain, and to demand of him a full statement of facts. I then believed myself to be on such terms with him, that I could abruptly and authoritatively demand his confidence. I did so, and was not disappointed in the result. On entering his cell that day, I told him that I was going to broach a new and important subject to him, and he must listen to me seriously, and not reply till I had done. I then said to him, that he must have felt all along that there was one barrier to our free communication; one point on which we did not understand one another; that the embarrassment which attended the avoiding of that point, obviously went far to defeat the satisfaction and profit to himself which ought to result from our interviews. I said that he must certainly have some knowledge respecting the fate of Dr. Parkman, which I had not, and that the unshared secret must be to him an oppressive and intolerable burden; that the time had come when he ought to share it with some one, and, under the circumstances, with me; that I had scrupulously foreborne hitherto to press him on this point, and urged it now only because I believed it would be for his relief and peace of mind; that I thought he must feel by this time that he owed me the truth, and that he could trust me; that he need not fear to tell me the whole truth, for I was not there to reproach him, nor to judge him, but to comfort him in his distress, and to help him in making peace with God and his conscience, and to assist him, if I might, to live while he lived, and to die when he should die, with the humility of a sinner and the firmness of a man, and I trusted the hope of a christian; that, in order to my being of any real service to him, there must be truth and true relation between us. I cautioned him not to answer me hastily, nor to speak till he was prepared to tell the whole and absolute truth,—that I would endeavour to put a favorable construction upon his silence; that I was in no hurry, and that he might take a day or two more to consider whether my advice to him to make a full disclosure, was not reasonable and good.

"I spoke to him some time in a strain which I have thus indicated. He seemed to me much affected by what I said, and when I paused, he said immediately, 'I am ready to tell you all. It will be a relief to me.' He then proceeded to relate the facts, which I have since embodied in the statement now to be presented; and I put to him a great number of questions, all of which he answered promptly, and with every appearance (it seemed to me) of an honest purpose to tell the truth. Some of the minor facts and explanations were given by him on a subsequent day; but the outline of the whole narrative, and the more important details were given at the interview of May 23d.

"It is important to observe, that at that date the writ of error was pending, and also that Dr. Webster's petition for a full pardon, with strong declarations of entire innocence, was in the hands of the governor. If the writ should fail, he

considered everything as staked upon that petition, the declarations it contained, and the documents and affidavits he believed would be obtained for its support. His immediate family, firmly and sincerely believing him entirely innocent, were engaged in seeking facts and papers to sustain his petition. I am confident, that at that time he had not the remotest idea of approaching the Executive in any other way than according to the tenor of the petition, nor began to contemplate the question, whether commutation would be a practicable, or even a desirable alternative. His whole thought, so far as he entertained any hope, was of pardon on the ground of innocence. Once in the course of his narrative, he suddenly paused, and said, with an appearance of anxiety, 'What if the writ should be granted, and a new trial follow, might not you be summoned and compelled to reveal all that I have said to you?' I told him, no; that the government would not put me into his cell as his confidential friend, and then try to use me as a spy; that it would be an outrage not to be thought of, and that I would not consent to be so used, whatever might be the consequences to myself. I had previously told him that I should never reveal his statements to any one while he lived, without his consent; and that if I survived him, he must leave all to my discretion. I feel sure that it had not occurred to his mind, that his statements to me could ever be used by me with a view to his advantage; but he had a moment's solicitude lest I might be compelled to reveal them to his harm. He seemed to me to make his disclosures simply because he was unwilling to deny my earnest request, wished to manifest his confidence in me, and, at the same time, was glad to have the opportunity of relieving his mind of its dreadful secret."

Dr. Putnam adds, that he did not make his demand of Dr. Webster at the suggestion of any legal or other friend of his, and that no one knew of his intention to make it. And neither Dr. Webster's statement, nor the fact that he had made any, was communicated by Dr. Putnam to any person, for more than a fortnight after he had received it. During this time, however, he urged Dr. Webster to withdraw the first petition to the Executive, in which his entire innocence of the crime imputed to him was so strongly protested; and in compliance with Dr. Putnam's advice, this petition was withdrawn (as already stated) on the 10th of June. The subsequent steps were taken with the concurrence of Dr. Webster's legal adviser.

The following is the very remarkable *Confession of Dr. Webster*, as reported to the Council by the Rev. Dr. Putnam:

"On Tuesday, the 20th of November, I sent the note to Dr. Parkman, which, it appears, was carried by the boy Maxwell. I handed it to Littlefield unsealed. It was to ask Dr. Parkman to call at my rooms on Friday, the 23d, after my lecture. He had become of late very importunate for his pay. He had threatened me with a suit, to put an officer into my house, and to drive me from my Professorship, if I did not pay him. The purport of my note was simply to ask the conference. I did not tell him in it what I could do, or what I had to say about the payment. I wished to gain, for those few days, a release from his solicitations, to which I was liable every day, on occasions and in a manner very disagreeable and alarming to me, and also to avert, for so long a time at least, the fulfilment of recent threats of severe measures. I did not expect to be able to pay him when Friday should arrive. My purpose was, if he should accede to the proposed interview, to state to him my embarrassments and utter inability to pay him at present, to apologise for those things in my conduct which had offended him, to throw myself upon his mercy, to beg for further time and indulgence for the sake of my family, if not for my own, and to make as good promises to him as I could have any hope of keeping.

"I did not hear from him on that day, nor the next (Wednesday), but I found that on Thursday he had been abroad in pursuit of me, though without finding me. I feared that he had forgotten the appointment, or else did not mean to wait for it.



I feared he would come in upon me at my lecture hour, or while I was preparing my experiments for it. Therefore I called at his house on that morning (Friday) between eight and nine, to remind him of my wish to see him at the College at half-past one, my lecture closing at one. I did not stop to talk with him then, for I expected the conversation would be a long one, and I had my lecture to prepare for. It was necessary for me to save my time, and also to keep my mind free from other exciting matters. Dr. Parkman agreed to call on me as I proposed.

"He came, accordingly, between half-past one and two. He came in at the lecture-room door. I was engaged in removing some glasses from my lecture-room table into the room in the rear, called the upper laboratory. He came rapidly down the steps and followed me into the laboratory. He immediately addressed me with great energy: 'Are you ready for me, sir? Have you got the money?' I replied, 'No, Dr. Parkman,' and was then beginning to state my condition, and make my appeal to him. He would not listen to me, but interrupted me with much vehemence. He called me 'scoundrel' and 'liar,' and went on heaping upon me the most bitter taunts and opprobrious epithets. While he was talking he drew a handful of papers from his pocket, and took from among them my two notes, and also an old letter from Dr. Hosack, written many years ago, and congratulating him (Dr. Parkman) on his success in getting me appointed professor of chemistry. 'You see,' he said, 'I got you into your office, and now I will get you out of it.' He put back into his pocket all the papers except the letter and the notes. I cannot tell how long the torrent of threats and invectives continued, and I can now recall to memory but a small portion of what he said. At first I kept interposing, trying to pacify him, so that I might obtain the object for which I had sought the interview. But I could not stop him, and soon my own temper was up. I forgot everything. I felt nothing but the sting of his words. I was excited to the highest degree of passion; and while he was speaking and gesticulating in the most violent and menacing manner, thrusting the letter and his fist into my face, in my fury I seized whatever thing was handiest—it was a stick of wood—and dealt him an instantaneous blow, with all the force that passion could give it. I did not know, nor think, nor care where I should hit him, nor how hard, nor what the effect would be. It was on the side of his head, and there was nothing to break the force of the blow. He fell instantly upon the pavement. There was no second blow. He did not move. I stooped down over him, and he seemed to be lifeless. Blood flowed from his mouth, and I got a sponge and wiped it away. I got some ammonia and applied it to his nose, but without effect. Perhaps I spent ten minutes in attempts to resuscitate him; but I found that he was absolutely dead. In my horror and consternation I ran instinctively to the doors and bolted them—the doors of the lecture-room and of the laboratory below. And then what was I to do?

"It never occurred to me to go out and declare what had been done, and obtain assistance. I saw nothing but the alternative of a successful removal and concealment of the body, on the one hand, and of infamy and destruction on the other. The first thing I did, as soon as I could do anything, was to drag the body into the private room adjoining. There I took off the clothes, and began putting them into the fire which was burning in the upper laboratory. They were all consumed there that afternoon, with papers, pocket-book, or whatever else they may have contained. I did not examine the pockets, nor remove anything except the watch. I saw that, or the chain of it, hanging out, and I took it and threw it over the bridge as I went to Cambridge.

"My next move was to get the body into the sink which stands in the small private room. By setting the body partially erect against the corner, and getting up into the sink myself, I succeeded in drawing it up. There it was entirely dismembered. It was quickly done, as a work of terrible and desperate necessity. The only instrument used was the knife found by the officers in the tea-chest, and which I kept for cutting corks. I made no use of the Turkish knife, as it was called at the trial. That had long been kept on my parlour mantel-piece in Cambridge, as a curious ornament. My daughters frequently cleaned it—hence the marks of oil

and whiting found on it. I had lately brought it into Boston to get the silver sheath repaired.

"While dismembering the body, a stream of Cochituate [water] was running through the sink, carrying off the blood in a pipe that passed down through the lower laboratory. There must have been a leak in the pipe, for the ceiling below was stained immediately round it.

"There was a fire burning in the furnace of the lower laboratory. Littlefield was mistaken in thinking there had never been a fire there. He had probably never kindled one, but I had done it myself several times. I had done it that day for the purpose of making oxygen gas. The head and viscera were put into that furnace that day, and the fuel heaped on. I did not examine at night to see to what degree they were consumed. Some of the extremities I believe were put in there on that day.

"The pelvis and some of the limbs, perhaps all, were put under the lid of the lecture-room table in what is called the *well*—a deep sink lined with lead. A stream of cochituate water was turned into it, and kept running through it all Friday night. The thorax was put into a similar well in the lower laboratory, which I filled with water, and threw in a quantity of potash which I found there. This disposition of the remains was not changed till after the visit of the officers on Monday.

"When the body had been thus all disposed of, I cleared away all traces of what had been done. I took up the stick with which the fatal blow had been struck. It proved to be the stump of a large grape-vine, say two inches in diameter, and two feet long. It was one of two or more pieces which I had carried in from Cambridge long before, for the purpose of showing the effect of certain chemical fluids in colouring wood, by being absorbed into the pores. The grape-vine, being a very porous wood, was well suited to this purpose. Another longer stick had been used as intended, and exhibited to the students. This one had not been used. I put it into the fire.

"I took up the two notes, either from the table or the floor, I think the table, close by where Dr. Parkman had fallen. I seized an old metallic pen lying on the table, dashed it across the face and through the signatures, and put them in my pocket. I do not know why I did this rather than put them into the fire; for I had not considered for a moment what effect either mode of disposing of them would have on the mortgage or my indebtedness to Dr. Parkman and the other persons interested; and I had not yet given a single thought to the question as to what account I should give of the objects or results of my interview with Dr. Parkman.

"I never saw the sledge-hammer spoken of by Littlefield, and never knew of its existence,—at least, I have no recollection of it.

"I left the College to go home, as late as six o'clock. I collected myself as well as I could, that I might meet my family and others with composure. On Saturday I visited my rooms at the College, but made no change in the disposition of the remains, and laid no plans as to my future course.

"On Saturday evening I read the notice in the 'Transcript' respecting the disappearance. I was then deeply impressed with the necessity of immediately taking some ground as to the character of my interview with Dr. Parkman: for I saw that it must become known that I had had such an interview, as I had appointed it first by an unsealed note on Tuesday, and on Friday had myself called at his house in open day and ratified the arrangement, and had there been seen and probably overheard by the man servant; and I knew not by how many persons Dr. Parkman might have been seen entering my rooms, or how many persons he might have told by the way where he was going. The interview would in all probability be known, and I must be ready to explain it. The question exercised me much, but on Sunday my course was taken. I would go into Boston and be the first to declare myself the person, as yet unknown, with whom Dr. Parkman had made the appointment. I would take the ground that I had invited him to the College to pay him money, and that I *had* paid him accordingly. I fixed upon the sum by taking the small note and adding interest, which it appears I cast erroneously.

"If I had thought of this course earlier, I should not have deposited Pettie's check for ninety dollars in the Charles River Bank on Saturday, but should have suppressed it as going so far towards making up the sum which I was to profess to have paid the day before, and which Pettie knew I had by me at the hour of the interview. It had not occurred to me that I should ever show the notes cancelled in proof of the payment; if it had, I should have destroyed the large note, and let it be inferred that it was gone with the missing man, and I should only have kept the small one, which was all that I could pretend to have paid. My single thought was concealment and safety. Everything else was incidental to that. I was in no state to consider my ulterior pecuniary interests. Money, though I needed it so much, was of no account with me in that condition of mind.

"If I had designed and premeditated the homicide of Dr. Parkman, in order to get possession of the notes, and cancel my debt, I not only should not have deposited Pettie's check the next day, but I should have made some show of getting and having the money the morning before. I should have drawn my money from the bank, and taken occasion to mention to the cashier, that I had a sum to take out that day for Dr. Parkman, and the same to Henschman, when I borrowed the ten dollars. I should have remarked, that I was so much short of a large sum that I was to pay to Parkman. I borrowed the money of Henschman, as mere pocket-money for the day.

"If I had intended the homicide of Dr. Parkman, I should not have made the appointment with him twice, and each time in so open a manner, that other persons would almost certainly know of it. And I should not have invited him to my room at an hour when the college would have been full of students and others—and an hour when I was most likely to receive calls from others; for that was an hour—just after the lecture—at which persons, having business with me or in my rooms, were always directed to call.

"I looked into my rooms on Sunday afternoon, but did nothing.

"After the first visit of the officers, I took the pelvis and some of the limbs from the upper well, and threw them into the vault under the privy. I took the thorax from the well below, and packed it in the tea-chest, as found. My own impression has been, that this was not done till after the second visit of the officers, which was on Tuesday; but Kingsley's testimony shows that it must have been done sooner. The perforation of the thorax had been made by the knife at the time of removing the viscera.

"On Wednesday, I put on kindlings, and made a fire in the furnace below, having first poked down the ashes. Some of the limbs—I cannot remember what ones, or how many—were consumed at that time. This was the last I had to do with the remains.

"The tin box was designed to receive the thorax, though I had not concluded where I should finally put the box. The fish-hooks, tied up as grapples, were to be used for drawing up the parts in the vault, whenever I should determine how to dispose of them. And yet, strange enough, I had a confused double object in ordering the box and making the grapples. I had before intended to get such things, to send to Fayal,—the box to hold plants and other articles, which I wished to protect from salt water and the sea air, and the hooks to be used there in obtaining coralline plants from the sea. It was this previously-intended use of them that suggested and mixed itself up with the idea of the other application. I doubt, even now, to which use they would have been applied. I had not used the hooks at the time of the discovery.

"The tan put into the tea-chest was taken from a barrel of it that had been in the laboratory some time. The bag of tan brought in on Monday was not used, nor intended to be used. It belonged to a quantity obtained by me a long time ago, for experiments in tanning, and was sent in by the family, to get it out of the way. It being sent just at that time was accidental.

"I was not aware that I had put the knife into the tea-chest.

"The stick found in the saucer of ink was for making coarse diagrams on cloth.

"The bunch of 'filed' keys had been long ago picked up by me in Fruit Street, and thrown carelessly into a drawer. I never examined them, and do not know whether they would fit any of the locks of the college or not. If there were other keys, fitting doors with which I had nothing to do, I suppose they must have been duplicates, or keys of former locks, left there by the mechanics or janitor. I know nothing about them, and should never be likely to notice them amongst the multitude of articles, large and small, of all kinds, collected in my rooms. The janitor had furnished me a key to the dissecting room, for the admission of medical friends visiting the college, but I had never used it.

"The nitric acid on the stairs was not used to remove spots of blood, but dropped by accident.

"When the officers called for me on Friday, the 30th, I was in doubt whether I was under arrest, or whether a more strict search of my rooms was to be had; the latter hypothesis being hardly less appalling than the former. When I found that we went over Cragie's bridge, I thought the arrest most probable. When I found that the carriage was stopping at the jail, I was sure of my fate; and before leaving the carriage, I took a dose of strychnine from my pocket, and swallowed it. I had prepared it in the shape of a pill before I left my laboratory on the 23d. I thought I could not bear to survive detection. I thought it was a large dose. The state of my nervous system probably defeated its action partially. The effects of the poison were terrible beyond description. It was in operation at the college, and before I went there, but more severely afterwards.

"I wrote but one of the anonymous letters produced at the trial,—the one mailed at East Cambridge.

"The 'little bundle,' referred to in the letter detained by the jailer, contained only a bottle of citric acid, for domestic use. I had seen it stated in a newspaper, that I had purchased a quantity of *oxalic* acid, which, it was presumed, was to be used in removing blood-stains. I wished the parcel to be kept untouched, that it might be shown, if there should be occasion, what it really was that I had purchased.

"I have drawn up in separate papers an explanation of the use I intended to make of the blood sent for on Thursday, the 22d, and of the conversation with Littlefield about the dissecting-vault.

"I think that Pettee, in his testimony at the trial, put too strongly my words about *having settled* with Dr. Parkman. Whatever I did say of the kind was predicated, on the hope I entertained that I should be able to pacify Dr. Parkman, and make some arrangement with him, and was said in order to quiet Pettee, who was becoming restive under the solicitation of Dr. Parkman."

Dr. Putnam then states, that after receiving the above communication from Dr. Webster, he addressed him with all the earnestness and solemnity he was master of; and conjured him, as a dying man, about to enter into the presence of his Maker, to tell him truly whether he had ever entertained the thought of putting an end to Dr. Parkman's life. "As I live, and as God is my witness, never. I was no more capable of such a thought, than one of my innocent children. I never had the remotest idea of injuring Dr. Parkman, until the fatal blow was struck. Dr. Parkman was extremely severe and sharp-tongued, the most provoking of men; and I am irritable and passionate. A quickness and brief violence of temper has been the besetting sin of my life. I was an only child, much indulged, and I have never acquired the control over my passions which I ought to have acquired early; and the consequence has been—this." "But," rejoined Dr. Putnam, "you notified Dr. Parkman to meet you at a certain hour, and told him you would pay him, when you knew you had not the means of paying him." "No," replied Dr. Webster, "I did *not* tell him I should pay him; and there is no evidence that I told him so, except my

own words, spoken after his disappearance, and after I had taken the ground that I had paid him. These words were one of the miserable tissue of falsehoods, to which I was committed from the moment I began to conceal the homicide. I never had a thought of injuring Dr. Parkman."

Dr. Putnam then communicated to the Council some further explanations which he had received from Dr. Webster. These referred to his reasons for inquiring of Littlefield about the defect in the vault attached to the dissecting apartment, from which a noxious effluvium escaped, and for his application to Littlefield to procure some blood for him, for experimental purposes,—two points of evidence which we did not bring forwards, as they did not seem to us to have any distinct bearing on the question;—and also included a more particular account of the keys found in his room (which is not given in the Report before us, but which we should much wish to have received), as well as a statement of the uses to which he purposed applying the tan and the pieces of grape-vine which had been brought in from his house.

Let us now apply ourselves to the consideration of this remarkable document, and endeavour to estimate its value. Of course not the least credit can be attached to it as having been given on the solemn affirmation of the unhappy prisoner; his previous repeated and solemn affirmations of entire innocence having left him no claim to be believed, however awful the circumstances under which the declaration was made. This was quite admitted by Dr. Putnam; although it is obvious that he was himself impressed with the strong conviction, that in this instance at least Dr. Webster had spoken the whole truth. But if Dr. Webster had previously made to Dr. Putnam personally, the asseverations which he made in court, and in his petition to the executive, we scarcely think that Dr. Putnam would have been then so confident. At any rate, *we* have no reason whatever for believing the one statement more than the other; and must therefore judge of it by its internal evidence alone.

Now, in the first place, our readers will observe, that Dr. Webster fully admits that Dr. Parkman came by his death by his hands, at the time, and in the place, charged in the accusation; and that he had subsequently endeavoured to get rid of the body, which he had first dismembered, by burning it piecemeal in the furnace in his laboratory. We endeavoured to show, in our analysis of the evidence, that it was *impossible* to draw any other inference from it than this; for that, although the several circumstances adduced might every one of them be possibly explained in some other mode, yet that no other hypothesis *could* be framed, which should tally with the *combination* which they presented,—any other explanation suggested for one evidentiary fact, being inconsistent with that put forward to account for another. We argued that, in such a case, the result is as certain, as if there had been supplied what is commonly termed *demonstrative* evidence in its behalf; in fact, such evidence must be regarded as really demonstrative, since it fulfils the essential condition of demonstration,—viz. that neither the contrary, nor any other inference, can be entertained as possible. The general result, it will be at once seen, has fully justified us in the position we took; and on looking back at our summary of the evidence, it will be seen, that among all the facts which we brought pro-

minently forward as proving the homicide, there was but a single one to which Dr. Webster gives a different turn; and this was the very trivial one of the removal of the tan from his house to the Medical College, which he affirms to have been a coincidence merely, and to have had no reference to any use to be made of it. But as he *did* make use of it, we think it just as likely as not, that he took advantage of the wish of his family to get it out of the way, and that he ordered it to be sent in at that particular time for his own purposes. The statement which he makes respecting the fishing-hooks and the tin box is curious enough. He admits that he did procure the former, and order the latter, at that particular juncture, for the purpose imputed; but he affirms that he had "a confused double object in ordering the box and making the grapples;" and the original purpose he now assigns was not an unlikely one in itself, as he had a daughter residing at Fayal for her health. We suspect the fact to have been, that when casting about as to how he should fish up the portions of the body from the privy-vault, the thought of the grapple he had previously planned came into his mind; and that when considering as to the readiest way of disposing of the thorax, the tin-box which he had previously thought of ordering, presented itself as the most feasible receptacle. But the fact of his assigning another and far more improbable purpose, when ordering the box, would seem to show, either that lying was more habitual to him than truth, or that his mind was in such a state of bewilderment that he scarcely knew what he was saying.

Dr. Webster's confession further tallies exactly with the interpretation which we gave, of his having voluntarily made known to Dr. Francis Parkman and others the fact that Dr. G. Parkman had visited him at the Medical College. If our readers will turn to the bottom of page 14, and compare *our* explanation of this circumstance with Dr. Webster's own statement, they will find the coincidence to be exact.

Of course, all the evidence which was procured to show that Dr. Parkman was seen in Boston subsequently to the time when it was suspected that he had been murdered, now falls to the ground. We showed (p. 24) to our own satisfaction, that this kind of evidence was attended with so much fallacy, and that in this particular case it possessed so little weight,—from the vast preponderance of the negative evidence as to Dr. Parkman's *not* having been seen in the streets, during the hours affirmed to have been spent by him in wandering about the city,—as not to deserve the slightest credence. Here, again, we are fully justified by the result. The five witnesses probably all spoke what they believed to be truth; and yet they were all mistaken, either as to the person, the day, or the hour.

Of course, also, all the charges against Littlefield fall to the ground; and his character remains unimpeached, as we maintained it to be.

The question now to be considered, however, is that of *premeditation*. The whole purpose of Dr. Webster's statement is,—while admitting the fact that he killed Dr. Parkman, the proof of which he found to be too strong against him for his denial to make the least impression,—to show that he had formed no preconceived plan of doing so, but that the crime was the result of sudden impulse. Now if our readers will turn to the latter part of our account of the trial (p. 30), they will see what were the grounds on which, in our estimation, the proof of "malice prepense" chiefly rests. In the first place, there is the fact that a homicide has been

committed, and that no *proof* of sudden provocation has been adduced; from which the law draws the *presumption* that the homicide was premeditated. This, however, may be a harsh construction, as Dr. Webster complains that it is, where the circumstances are such that no proof of provocation *can* be adduced, for want of witnesses. It fully justified the jury in the verdict they returned; but we are now investigating the case as one of moral probability, altogether apart from legal presumption; and shall therefore throw aside this consideration altogether. In the absence, then, of any possible knowledge of the actual occurrences at the fatal interview, have we any indication, in the antecedent and subsequent events, of Dr. Webster's intentions? We especially dwelt upon two circumstances: *first*, that he had brought Dr. Parkman to the Medical College under the pretext of paying him, when he could have had no intention of paying him, well knowing that he had not the means to do so; and *second*, that he had taken into his possession, and done his best to cancel, both the promissory notes found by him on Dr. Parkman's person, which there seemed no reason for his doing, if he had not intended in this way to get rid of his pecuniary obligation. Now it is to be remembered, that in all reasoning upon evidence, we must presuppose the correctness of the data; the fallacy of *testimony* being one that attaches to direct, as well as to inferential evidence; and an error in the decision, if it should be made, being chargeable, not upon undue reliance on the inferences deduced from the probative facts, but upon the invalidity of the probative facts themselves,—to which source of fallacy *direct* evidence is just as liable as *circumstantial*, or even more so.

Now it is a most remarkable fact, that in the present case, the proof of this point rested on *Dr. Webster's own assertion*,—repeated, as this was, over and over again, from the first statement which he made to Dr. F. Parkman, to the last solemn appeal which he made during the trial,—that he brought Dr. G. Parkman to the Medical College for the purpose of paying him, and that he *had* paid him, the amount due on the smaller of the two notes. Dr. Webster had no one to blame but himself, therefore, for the damning inference which was drawn from this statement. Of its truth or falsity, it does not appear to us that we have any sure means of judging. Dr. Webster made his first statement on this point, in the form in which he hoped that it would best succeed in diverting suspicion from him. It did not succeed. The fragments of Dr. Parkman's body were found on his premises, and he was charged with the murder. Still he adheres to his first statement. He denies all knowledge of the homicide, and throws upon others the *onus* of having concealed the remains in his laboratory. But the evidence is too strong for him; the crime is brought home to his charge; and clear proof having been given that he had not the means of paying Dr. Parkman the sum which he affirmed himself to have handed over, the assertion which he so perseveringly made became one of the strongest evidentiary facts against him. Yet notwithstanding the bearing which it must have been evident that it had upon his case, he persists in forcing his counsel to reiterate it; and his last address to the jury, previously to the judge's charge, contains a repetition of it in another form:—"That money paid to Dr. Parkman I had positively laid by, from day to day, in this little trunk [in which the notes were found]; and, unfortunately, no one can be produced who saw me pay it." In the course

of his subsequent reflections, however, he sees how fatal this assertion has been to him; he sees in it the proof, on which the greatest reliance was placed, of predetermination to murder; he makes up his mind to avow the homicide, and to endeavour to clear himself from the higher measure of crime; and he now turns round upon himself, and declares that his statement was "one of the miserable tissue of falsehoods to which he was committed from the moment he began to conceal the homicide." Surely he can be no more credited in his subsequent denial, than he was in his first assertion; and it has been entirely his own fault, that the latter was the one upon which his fate was decided. Had he made his present statement *at the trial*,—had he then openly avowed the homicide, and urged all that he could in favour of the probability of the account of it which he now gives,—we have not the shadow of a doubt that he would have been found guilty of the minor offence only. The judge and jury would have been too glad of an excuse for taking this course; and in the absence of any other distinct evidence of premeditation, the legal presumption would have been set aside by other presumptions to the contrary. Such a course would have been in full accordance with public feeling, which, previously to the trial, was so far in Dr. Webster's favour. But Dr. Webster took the higher ground of entire innocence; he did not even suffer his counsel to put forward the hypothesis of provocation, they having done it as a part of their duty in despite of his directions; and the line of defence he took was such as to make him, if not altogether innocent, guilty of the gravest crime. He played for the highest stake he could fix,—death, or release. And having lost, he now comes forward, and charges, as a hardship of legal presumption, what was really the consequence of his own scheme of defence.

As there is not the slightest possibility of determining from Dr. Webster's own assertions, whether he did or did not induce Dr. Parkman to come to the Medical College on a false pretence, we must fall back on the presumptions derivable from other facts. Now it may be asked, in the first place, whether it is probable that Dr. Webster would have written to Dr. Parkman, and afterwards called upon him at his own house, to ask him to come down to the Medical College, merely to inform him that he had no means of payment, and to entreat him for more time? Could he not have foreseen that such a course, instead of enabling him to exert his powers of entreaty and persuasion on Dr. Parkman under more favorable circumstances, would only tend to increase Dr. Parkman's irritation against him? Of course, Dr. Parkman's feeling would be—"If Dr. Webster had only *that* to tell me, why did he not tell me so when he called at my house this morning, instead of giving me the trouble to seek him here?" But further, it is obvious from Dr. Webster's last statement of what took place at the fatal interview, that Dr. Parkman *did* come expecting to be paid; whether from any positive intimation he had received to that effect, or from the presumption that Dr. Webster would not have brought him there except to pay him, we cannot of course be sure. And this inference is confirmed by the fact, that Dr. Parkman had brought the notes with him,—for no purpose, that we can see, if Dr. Webster had led him to understand that he only sought his presence there for the purpose of again begging a postponement of the time of payment. On the whole, we cannot but believe, that if Dr. Webster did not actually hold out the



distinct expectation or promise that he would pay Dr. Parkman on the day fixed, he intentionally so worded his request for Dr. Parkman's calling, that the latter supposed it to be for the purpose of a settlement, and came accordingly at the hour appointed by Dr. Webster. Again, his call upon Dr. Parkman in the morning, seems to us to indicate an extraordinary motive for bringing Dr. Parkman to the Medical College on that particular day, and at that particular hour. Why, otherwise, should he not have rather rejoiced at the delay, which Dr. Parkman seemed to be voluntarily affording to him? Why should he fear lest Dr. Parkman had forgotten the appointment, when he himself had everything to fear, and little enough to hope, from the interview? He had previously done everything he could to put off the meeting until that particular Friday, not having (as he expressly admits) the slightest hope that he should be able to pay his debt on that day; and then he shows a remarkable anxiety that the day should not pass by without having the interview, and having it, too, at the Medical College. If our readers bear in mind the peculiar circumstances that rendered that day peculiarly fit for his purpose (p. 13), they will see a coincidence between these facts, which seems to bear too much of preconcert to be accidental.

There is another point which now strikes us forcibly, as indicating that Dr. Webster intended to get rid of his debt to Dr. Parkman by some other means than the honest one. We have seen (pp. 11, 12) that at the beginning of November, he had received a sum of money from class-fees, which was much more than sufficient to liquidate the smaller note, for the payment of which Dr. Parkman was so importunate. Yet, instead of employing any of this to stop the mouth, even by a partial payment, of a creditor who had it in his power to ruin him by exposing his dishonesty, Dr. Webster pays away various sums to other creditors, of whose pressure upon him he does not make any complaint, and thus puts it out of his power to discharge his debt in full. Even on the morning of the fatal interview, he had 139 dollars in his bank, and he then received from Mr. Pettee 90 dollars more, making rather more than half his debt to Dr. Parkman. Is it not strange that he should not have even offered to pay *half* his debt to this man who was threatening him with destruction, and have thus strengthened his entreaty for time and mercy in regard to the rest?

Taking the following circumstances, then, into account;—1st. Dr. Webster's evasion of Dr. Parkman's claims until Friday the 23d of November, although he had the opportunity of settling with him earlier in the month;—2d. Dr. Webster's obvious anxiety to bring Dr. Parkman to the Medical College on that particular day, although, if many days more had gone by, it would have made no difference with respect to the purpose for which he states himself to have sought this interview;—3d. Dr. Webster's having led Dr. Parkman to suppose, by implication at least, if not by actual assurance, that he brought him there for the purpose of paying him, when he distinctly admits that he had no such intention;—and, 4th. The absence of any attempt on Dr. Webster's part, by an offer of partial payment, which he had the power of making, to mitigate the anger of Dr. Parkman, and thereby obtain more time for the remainder;—and taking also into account the reasons which would probably render that particular day more suitable than any other, in Dr. Webster's estimation, for the perpetration of this horrid deed;—we think that a strong

case has been made out on the affirmative side of the question of premeditation.

It is argued in behalf of Dr. Webster, however, that "the fact that he made the appointment with Dr. Parkman in so open a manner, in a building so much frequented, and at an hour so unfavorable to secrecy," tells strongly *against* the idea of premeditation. This, however, by no means appears to us in so forcible a light. It must be remembered, that Dr. Webster's lecture-room and laboratory were remarkably isolated, as our former description (p. 12) will show, from the rest of the building; so that even in the busiest part of the day, he might be in complete seclusion there. Further, it appears from the evidence, that whilst the lecture succeeding Dr. Webster's was going on, it was more likely than not, that a visitor to Dr. Webster might enter his apartments without being observed by any one; and it appears, *on Dr. Webster's own showing*, that he appointed Dr. Parkman to come at the close of his lecture, as being the time when the interview might best take place without the chance of interference. As it turned out, Dr. Parkman *did* enter without being observed by any one in the building; and Dr. Webster *did* remain in his rooms without interruption, for four hours after the commission of the murder. We cannot admit, then, that the argument urged by Dr. Webster, and repeated by Dr. Putnam, has any exculpatory weight. It must be remembered that, if Dr. Webster really arranged his scheme beforehand, he would feel it necessary to choose a time neither so early as to interfere with his regular duties, nor so late as to attract attention from its contrariety to his usual habits. He had never been accustomed to remain at the Medical College during the whole evening; and his doing so would in itself have been a suspicious circumstance. Even as it was, his continuance in his laboratory until six o'clock, did not pass unnoticed. We do not consider, then, that any argument, either favorable or unfavorable to Dr. Webster, can be drawn from his selection of the hour for the interview.

But it is urged that, if a premeditated murder had been Dr. Webster's intention, he would have scarcely made the appointment so openly, sending an unsealed note through two or three hands, and then himself calling at Dr. Parkman's house. This consideration, we think, is entitled to much greater weight; and it would tend very strongly to exculpate Dr. Webster from the charge of premeditation, did not other parts of his conduct indicate that he had acted with an extraordinary want of foresight. What, for example, could be more imprudent, or more likely to lead to a discovery of his dishonesty, than his applying to Dr. Parkman's own brother-in-law for a loan, upon the security of the cabinet of minerals which he had previously mortgaged to Dr. Parkman himself (p. 11)? Besides, he might have calculated, that the very absence of concealment in this matter would tend to divert suspicion from him; just in the same manner as he has confessed, that his spontaneous communication to Dr. F. Parkman, on the Sunday after the murder, was (as we had anticipated) the result of a calculation of this kind.

The absence of any preparation, on Dr. Webster's part, for disposing of the body of Dr. Parkman, is dwelt on by Dr. Putnam as another strong indication of the absence of premeditation.

"He is a chemist. He had an extensive laboratory, and a complete apparatus. He was giving experimental lectures at the time, and might, therefore, have col-

lected any quantity of chemical substances without suspicion. He knew that there was a simple process by which the body might be entirely dissolved in a few hours. There need not have been any trace of it left on Saturday morning. It is not credible that he should have been meditating the homicide from Tuesday to Friday, and yet make no such provision first. No such provision was made. He had to dispose of the body in the most clumsy and dangerous way, and to leave much of it undisposed of. He appears to have been taken by surprise, unprepared."

Now on this we have only to remark, that considering the very bungling way in which Dr. Webster proceeded *after* the homicide,—making so little progress in the disposal of the body in the course of six days, with all the means and appliances at his command, that he had then only got rid of the head, the hands and feet, and the viscera,—we think this argument altogether devoid of force. The question has been asked by every one,—“if Dr. Webster *were* the guilty party, how was it that he had not much sooner destroyed all traces of his guilt?” And it has been attempted, on this ground, to establish a presumption of his entire innocence. As the fallacy of such an inference has been completely proved by Dr. Webster’s confession, we cannot allow Dr. Putnam’s use of the facts to possess any probative force, sufficient to countervail other presumptions.

With regard to the circumstances of the interview itself, and the mode in which Dr. Parkman came by his death, we do not think the statement of Dr. Webster at all inconsistent with probability. The case *might have* stood exactly as he now represents it. All the knowledge we have of the characters of the two men would lead us to suppose that Dr. Parkman might use very harsh language, and even threats, towards a dishonest creditor; whilst on the other hand, we can easily conceive Dr. Webster to have been overborne by the torrent of the feelings thus excited, to such an extent as to commit personal violence on his opponent. Nor do we think that there is any abstract improbability in the account which he gives of the results of the blow, as compared with the instrument with which it was inflicted. From one of the Reports we have seen, of the proceedings of the Council of Massachusetts, we gather that it was held to be in the highest degree improbable that a fatal blow could have been given with so light a stick; but the annals of medical jurisprudence contain many cases, in which death ensued from a blow less violent than this may be presumed to have been.

With regard to the account given by Dr. Webster of the steps which he took for the disposal of the body, we have seen it stated, that it has been proved, by measurement of the “well” under the lecture-table, to which he refers, that it could not have possibly contained what he states that he put into it; but on this point we have no certain information.

Dr. Putnam further argues the absence of premeditation, from the very blundering nature of Dr. Webster’s conduct respecting the promissory notes, which he supposes that Dr. Webster would scarcely have been guilty of, if he had previously formed his plan. Here, again, we have in reply, that the absurd conduct of Dr. Webster in preserving these notes at all, and especially the larger one, when he had had six days to consider of the best course to take with regard to them, is another proof of how little, in a case like his, the conduct can be interpreted according to the ordinary rules of common sense. The adage “*Quem Deus vult perdere*

*prius dementat*," has proved itself true in numberless instances; and Dr. Webster's case is, in a variety of ways, a most remarkable exemplification of it. He did not make up his mind until two days after the homicide, as to what account he should give of the transaction; having previously gone and paid-in his 90 dollars to the Bank, without a thought of the bearing which his doing so would have on the statement he was concocting. He kept the small note, apparently because he intended to assert, that his liability on account of it had ceased, in virtue of his possession of it and of the erasure of the signature. But why did he keep the large note also? He took great pains to get rid of the clothes and of the most recognisable parts of the body of Dr. Parkman; and yet he kept the larger note, the possession of which was one of the most damning facts in the evidence against him, whilst it could be of no pecuniary benefit to him whatever; and this he did, although he might have burnt it with a thousandth part of the trouble that it cost him to get rid of those other *indicia*, some remnants of which led to the determination of Dr. Parkman's identity. To a man of Dr. Webster's temperament, the contemplation of a terrible crime is just as likely to interfere with the due exercise of foresight and discrimination, as the actual commission of it would be; and it is not inconsistent, therefore, with the idea of premeditation, to suppose that Dr. Webster was as weak and silly *before* the murder, as he showed himself to be *afterwards*. Dr. Putnam admits that "upon either hypothesis,—that of premeditated, or sudden homicide,—there is something inexplicable about these notes;" and he really does not think, that it was in Dr. Webster's power fully to clear up the mystery, if he were ever so much inclined to do it.

The foregoing are the chief assertions contained in Dr. Webster's confession, and the most important arguments founded thereon, that have a bearing upon the question of premeditation. For ourselves we can only say, that we are most happy that the responsibility of deciding upon it has not fallen to *our* lot, and that we have been able to discuss it as one in which we have no personal interest whatever. It will have been obvious that our own leaning is in favour of the hypothesis of premeditation; but still, if the case had now to be tried for the first time, we should not ourselves feel justified, upon that evidence, in returning a verdict of wilful murder. The case is altogether a most remarkable one in its juridical bearings. Dr. Webster is found guilty of *homicide*, upon circumstantial evidence, which, as we have shown, could leave no doubt in any mind accustomed to judge of the probative value of facts and inferences. But the proof of premeditation, constituting the crime of *murder*, chiefly rested upon the assertions which the prisoner himself had made, and upon the line of defence he adopted on his trial, which precluded any other issues than these two,—either that Dr. Webster was entirely innocent, or that he was guilty on the gravest charge. The question which came before the Executive Council may be considered under two points of view, according as the Council deemed its duty to be, to sit in judgment upon the case *de novo*, and to try the question of Dr. Webster's guilt or innocence of premeditated murder for the first time; or to consider whether sufficient proof of the *absence* of premeditation had been adduced by Dr. Webster, to justify them in *reversing* the verdict of the Jury, and absolving him

from the punishment to which it rendered him liable. We presume that the Council felt its duty to lie in the latter of these courses; and if so, we cannot question the righteousness of its decision. Most assuredly Dr. Webster and his friends have no title to do so. A criminal cannot be allowed to claim a second trial (which the former course of proceeding would virtually be), because he finds that the false line of defence, which he had deliberately and advisedly selected, led to an issue which he might have escaped by telling the truth at first. Moreover, we would ask whether Dr. Webster has not forfeited all claim to the merciful consideration of his case, by the fearful perjury which he has added to his crime; whether such perjury be not a sin of the deepest die, not only against Heaven, but against society, as tending to destroy all confidence in human truth; and whether, if he be not really guilty of premeditated murder, his subsequent conduct has not equally rendered him deserving of the highest penalty of the law?

We write whilst the wretched convict still lives, but under the doom of death not now to be averted by argument or entreaty. No words of ours can influence his fate. Before they meet the eyes of those for whom they are designed, he will have gone to answer for himself before the tribunal of the Omniscient Judge, who seeth not as man seeth, and who can and will rectify, in His adjudication of punishment or reward, all the errors of those whom He permits to act as administrators of justice on earth. May *he* find mercy before that tribunal! And may *we* all learn to avoid the beginnings of evil; to abstain from that careless improvidence, which, by placing a man in a position of pecuniary embarrassment, tempts him to have recourse to dishonorable means for staving off its consequences; from that indulgence in "evil thoughts and sinful desires," which gradually and almost imperceptibly familiarises the mind with ideas that would have been once driven from it with horror and disgust; and from that habit of yielding to the impulses of "quickness and brief violence of temper;" which, even on the most lenient view of Dr. Webster's case, has been productive of doubly-fatal consequences, and has involved two families in the deepest sorrow.

In the midst of the gloomy picture which imagination raises before us, it is pleasing to find one ray of light. Dr. Webster has shown an anxiety to do justice to those on whom, in his attempts to extricate himself, he had cast the most unfounded imputations. We extract the following from a Boston paper of July the 25th.

"An interview took place yesterday afternoon, between Dr. Webster and Mr. Littlefield, at the solicitation of Dr. Webster, in the presence of Mr. Andrews, the jailer. On their entrance, Dr. Webster stepped forward, and very cordially and affectionately seized the hand of Mr. Littlefield, remarking, that he had long desired to see him; that he could not feel at ease until he had made his acknowledgments to him; that he felt that he had done him great injustice; and he asked his forgiveness. Mr. Littlefield promptly and feelingly replied, that he forgave him with all his heart, and expressed his pity and sympathy for him. Mr. Littlefield also told him, that it was a painful duty he had to perform when he took the stand and testified against him, but that he felt it a duty which he had no right to shrink from; and if he had stated anything wrong, it was unintentional, and he was very sorry, and asked forgiveness. Dr. Webster replied, that he had misrepresented nothing,—that he had told the truth. Dr. Webster said, however, as a dying man, that he could not bring the sledge-hammer to his recollection. As the interview was drawing to a close, Dr. Webster again took the hand of Mr. Littlefield,

thanked him for calling, and expressed a wish to see Mrs. Littlefield. Mr. Littlefield remarked, that they had always, while at the Medical College, got along agreeably and pleasantly together, and that he (Dr. Webster) had always treated him kindly, &c. Dr. Webster said, that a kind and friendly feeling had always existed on his part towards him (Mr. Littlefield) and his family. Dr. Webster spoke of his present situation, and said he was resigned to his fate, but felt deeply for his family. During the entire interview, both parties seemed deeply impressed, and they parted in tears."

An interview has subsequently taken place between Dr. Webster and Mrs. Littlefield; in which he begged her forgiveness for the injury which he had done her, and expressed his sympathy in the affliction under which she was suffering from the loss of a child. We trust that this may be taken as an indication that the unhappy criminal has been employing the time left to him on earth in rectifying, so far as may be, the consequences of his misdeeds, and in preparing himself for his awful change.

#### ART. XII.

1. *Compendium de Médecine Pratique, ou Exposé Analytique et Raisonné des travaux contenus dans les principaux Traités de Pathologie Interne.* Par M. LOUIS DE LA BERGE, Docteur en Médecine, Agrégé à la Faculté de Médecine de Paris, Chef de Clinique Médicale à la même Faculté; M. ED. MONNERET, Agrégé à la Faculté de Médecine de Paris, Médecin du Bureau Central des Hôpitaux; et M. LOUIS FLEURY, Agrégé à la Faculté de Médecine de Paris, Membre Correspondant de l'Académie Royale de Médecine de Belgique. Ouvrage Autorisé par le Conseil Royal de l'Instruction Publique et par le Conseil de Santé des Armées de Terre.—Paris, 1836-46. 8 vols. 8vo, pp. 698, 638, 642, 636, 639, 634, 615, 496.
2. *Guide du Médecin Praticien, ou Résumé Général de Pathologie Interne et de Therapeutique Appliquées.* Par F. L. I. VALLEIX, Médecin des Hôpitaux de Paris, Membre Titulaire de la Société Médicale d'Observation et de la Société Anatomique, Auteur de la 'Clinique des Maladies des Enfants Nouveau-nés,' du 'Traité des Névralgies,' &c.—Paris, 1842-47. 10 vols. 8vo, pp. 576, 600, 627, 559, 632, 609, 586, 599, 847, 1006.
3. *Handbuch der medicinischen Klinik.* Verfasst von Dr. CARL CANSTATT, königlich-bayerischem Gerichtsarzte und Mitglieder mehrerer gelehrter Gesellschaften. Zweite vermehrte Auflage.—Erlangen, 1843-47. 4 vols. 8vo, pp. 382, 1102, 919, 1109. Also published with the second title of *Die specielle Pathologie und Therapie vom klinischen Standpunkte aus bearbeitet von Dr. CARL CANSTATT, &c.*
4. *Handbuch der Pathologie und Therapie.* Von Dr. C. A. WUNDERLICH, Professor der Medicin, Vorstand der medicinischen Klinik zu Tübingen. Dritter Band.—Stuttgart. 8vo, pp. 1238.

In the concluding portion of our former article on the Diseases of the Digestive System (see p. 68), we noticed at some length the general etiology of this class of affections. In the early part of the present article, our attention will be directed to the consideration of the general symptoms (or, to adopt Wunderlich's term, the phenomenology), the diagnosis, and

the treatment of these disorders; and in the latter part we shall notice some of the most important of the individual diseases of this class.

In treating of the Phenomenology of the Diseases of the Digestive Canal, Wunderlich adopts the following arrangement. He divides the symptoms into—

- A. *Those connected directly with the intestinal canal; and,*
- B. *Those which are indirect, and depend on the influence of intestinal diseases on other parts.*

The former are subdivided into—

- 1. *Subjective sensations.*
- 2. *Objective physical relations.*
- 3. *Indications afforded by the digestive power.*
- 4. *Indications afforded by the motions of the intestinal tract.*
- 5. *Indications afforded by the character and properties of the dejections.*

I. Wunderlich considers the *subjective sensations* occurring in the intestinal canal in the following order: 1. Those referred to the œsophagus. 2. Those referred to the stomach. 3. Those referred to any other portion of the canal, as far as the lower extremity of the colon. And 4. Those referred to the rectum and anus.

The real seat of sensations referred to the œsophagus, often exists in reality in the stomach. The latter organ, if unduly filled either with food or gas, often gives rise to a sensation of fulness in the œsophagus, extending even to the pharynx, when no unpleasant sensations are referable to the gastric region itself. Again, the burning sensation (pyrosis), although referred by the patient to the upper part of the œsophagus, in reality depends on the condition of the stomach, being a common symptom of imperfect digestion, and often occurring after the use of salt or fat meat, or any other indigestible species of food; or after too free indulgence in spirits or wine; indeed, in ravenous eaters, in drunkards, and in persons suffering under certain chronic diseases of the stomach (especially such as are accompanied by severe vomiting, as hæmatemesis, cancer, and ulceration), it may almost be said to be a constant symptom. The sensation of fulness in the œsophagus, and the burning sensation (pyrosis), are often associated with a feeling of constriction; this latter sensation may, however, occur independently of the others, and in cases where neither the œsophagus nor any other portion of the canal is in reality affected, as in cases of hysteria. In stricture of the œsophagus, this sensation is most perceptible when the food passes the affected spot. Actual pain in the œsophagus is seldom observed; it only occurs in œsophagitis (a comparatively rare affection, which we shall notice subsequently), and in ulceration, stricture, and cancer of this organ; it is then excited or heightened by the passage of food. The patient himself can seldom determine the exact seat of the pain, or rather of the disease causing it; a much more accurate knowledge on this point can be obtained, by observing how far a catheter or sound can be introduced before the pain is excited. Indeed, it may be laid down as a general principle, that with the exception of the pain induced in certain cases by the passage of the catheter, all these subjective phenomena are insufficient to determine even the existence of any œso-

phageal disease, and, *à fortiori*, are totally inadequate to establish a diagnosis between the individual affections of this organ.

The sensations arising from the stomach are partly general or instinctive, and partly localised in the epigastric region. While the former are unquestionably transmitted along the pneumogastric nerve, the latter are probably dependent on the sympathetic system.

The general sensations are those connected with the appetite. Although in most diseases of the intestinal canal the desire for food is very much diminished, there are some forms of chronic disease in which it is unabated, or even increased. Thus the appetite is often very much augmented in cases of obstruction of the œsophagus, of dilatation of the stomach, and of worms, especially of tape-worms; occasionally in the chronic vomiting of pregnancy; and sometimes (although comparatively rarely) in cases of long standing diarrhœa.

Wunderlich makes the practical remark, that the anorexia accompanying diseases of the intestinal canal not unfrequently depends upon the nature of the diet to which the patient has been restricted, or upon the abuse of medicines, rather than on the co-existing diseased action.

The sensation of *satiety*, when only a very little food has been taken, approximates to, although it is not identical with, anorexia. In cases of very contracted stomach, (as, for instance, when there is cancer of that organ, but the pylorus remains open,) of thickening of the mucous membrane of the intestines, of very slow digestion, of tension of the stomach or intestine downwards (as, for instance, by a tumour of the omentum), or of pressure and displacement of the stomach upwards (as by abdominal tumours, ascites, tympanitis, or advanced pregnancy), we frequently remark, that although there is a good appetite, a feeling of repletion is induced by a very small portion of food.

He considers that there is a specific difference between the sensations of *loathing* (for so we presume we must translate *Ekel*), and *nausea* (*Uebelkeit*), although they are so constantly associated, that it is difficult to isolate one from the other; the former being mere aversion to food, whilst the latter is the indescribable subjective sensation which precedes vomiting. He regards them, however, as both having their seat in the pneumogastric nerve, and does not give any reason for regarding them as distinct. Although these *loathings* are often (like longings for particular foods) only matters of caprice, there can be no doubt that they are often, on the other hand, instinctive. Thus, in gastric catarrh there is an actual aversion to animal food and fatty matters; and in certain forms of irritation of the stomach, spirits and tobacco (even to those who usually indulge in them) become perfectly unendurable.

*Thirst* is also placed by Wunderlich amongst the general or instinctive sensations that originate in the stomach. There unquestionably is considerable thirst in inflammatory affections of the stomach, and it is often intense when there have been abundant discharges from the intestinal canal, especially in cholera and dysentery; it is, however, like the loathing and nausea, a symptom of very little diagnostic value.

Amongst the local sensations in the gastric region, he notices a *feeling of emptiness*, as occurring not only after a prolonged deprivation of food, but in cases of great contraction of the stomach from other causes, and when that organ is displaced in a downward direction; and further, occa-



sionally after sudden changes in other portions of the abdominal cavity, by which the previously compressed stomach suddenly obtains a greater space, as, for instance, after the removal of a peritoneal effusion. It often, however, occurs in gastric affections, without our being able to assign any rational reason for its existence. An abnormal *sensation of fulness* occurs, not only when the stomach is distended with gas or a morbid accumulation of its own secretions, but also in cases of protracted digestion, and when there is extensive cancerous deposition in the organ itself, or in its immediate vicinity. The *feeling of cold* in the stomach is rare; it occasionally occurs in chronic affections of the stomach (probably in the neuroses). An abnormal *feeling of heat* is of far more common occurrence; it accompanies hyperæmic conditions of the stomach, and consequently precedes hæmatemesis; although very severe in inflammatory affections, it also occurs in cases which pertain to the neuroses of this organ. Hence its diagnostic value is considerably diminished.

The *pains* which are described by patients as existing in the region of the stomach are of the most varied nature. Sometimes the feeling is one of intense pressure, sometimes of constriction, of stabbing, or of laceration; or the pain is compared to that which might be caused by a glowing coal, or as if the stomach were being clawed by a wild animal; sometimes it is radiating, sometimes concentrating. But however accurate these delineations of the pain may be, they not only teach us nothing with certainty regarding the nature of the disease, but they do not even, in themselves, afford evidence that the stomach is the seat of any morbid affection. For pains in the gastric region, and answering in all respects to one or other of the above descriptions, may also occur in spinal irritation, in uterine diseases, in thoracic affections, in diseases of the liver and diaphragm, in rheumatism of the abdominal walls, in peritonitis and colitis, in windy colic, in diseases of the omentum, in aneurisms of the descending aorta, &c. In a diagnostic point of view, the conditions under which the pains are excited are of greater value than the nature of the pains themselves.

"Gastric pains in persons whose digestion and appetite are unaffected, and which are not exacerbated by hunger or by food of any kind, most probably depend on the spinal cord, or on some organ adjacent to the stomach. Pains which are diminished by strong pressure, may be put down as neuralgic. Pains which are distinctly increased when the hand is gently placed on the epigastric region, but which, on considerable, but unexpected, pressure, are not at all, or, at all events, not proportionally increased, are either imaginary or sympathetic. Pains continuing for days or weeks, or recurring at definite intervals without any apparent cause; also pains which come on suddenly with great severity, and disappear in the same rapid and complete manner, may with probability be regarded as gastric neuralgia; they may, however, depend on the temporary accumulation of gas. Pains emanating from the *scrobiculus cordis*, and spreading in various directions, may arise from cardialgia, rheumatism of the abdominal walls, or peritonitis. Gastric pains which are suspended on the introduction of food into the stomach, depend on some neuralgic affection, or on the presence of parasites. Pains which exist both while ordinary food is taken and while fasting, but which disappear on the patient's partaking of stimulating food or drink, depend on anæmia of the stomach. Pains exacerbated on the introduction of the smallest quantity of food into the stomach, indicate the probable existence of gastritis or degeneration. Pains coming on when the patient lies on his belly, make us suspect, that there may be ulceration of the anterior wall of the stomach. If pains supervene an hour or so after mealtime, we fear disease of the pylorus. Lancinating pains are sometimes remarked in cancer. When

there are dragging pains in the stomach, and, indeed, in every dubious kind of sensation in that neighbourhood, we should never neglect to examine whether a hernia may not be present." (Wunderlich, vol. iii, pp. 788-89.)

The subjective feelings connected with the portion of the intestine extending from the duodenum to the sigmoid flexure, are almost entirely devoid of value. When there is decided pain in these regions, we should ascertain whether, and in what way, it is modified by pressure. If it diminishes on pressure, there is usually little to be apprehended, although occasionally the pains of an incipient enteritis or colitis are thus alleviated; if, on the other hand, it is increased by pressure (or, at all events, not diminished), we must carefully examine if there are any intestinal displacements (hernia, invagination, &c.), especially if, at the same time, there are nausea and constipation. There are, perhaps, no cases in which a perfect *anamnesis* (or previous history) is of more importance, either in reference to diagnosis or treatment, than in those of severe abdominal pains; thus, we should ascertain whether our patient has been engaged in an occupation in which lead is used, whether he has formerly passed worms, or whether he has recently been exposed to cold or wet.

Pains in the rectum and anal region are far more distinct and acute than those in the superior portions of the intestinal canal, and, consequently, are more serviceable in reference to diagnosis. There is nothing in Wunderlich's remarks on this subject to call for observation.

II. *The objective physical relations* are next considered. The form and relative position of the viscera may, to a considerable extent, be determined by direct investigation, although with less accuracy than in the case of the thoracic organs.

The objective examination of the oesophagus can only be made by means of the oesophageal sound, by which we can ascertain the degree of its permeability, the position of a stricture, whether there are diverticula, &c.

The objective examination of the intestinal canal, from the stomach to the commencement of the rectum, can only be made through the abdominal walls. After pointing out the information to be derived from the inspection, tactile examination, percussion, and temperature of the abdomen, he proceeds to the objective examination of the rectum and anus, which can be instituted in a far more perfect manner than that of the other parts of the intestinal canal. He regards the educated finger as affording diagnostic indications almost equally certain with those yielded by the speculum.

"The anatomical relations which are elucidated by a properly conducted physical examination, are:

"1. In reference to the colour of the part. While a red colour indicates a hyperæmic state, a blue one indicates distended veins.

"2. In reference to its temperature, which, if abnormally elevated, is indicative of hyperæmia and inflammation.

"3. In reference to the state of the canal—its narrowness or width, the simple closure, powerful contraction, or paralytic feebleness of its sphincters, the occurrence of one or more strictures, perfect occlusion, or the existence of fistulous openings, or of prolapse of the mucous membrane.

"4. In reference to the existence of tumours—as for instance, abscesses in the walls of the rectum, arising either from periproctitis or from follicular inflammation, induration of the submucous areolar tissue, vegetations sprouting on the mucous membrane, hemorrhoidal tumours (usually two or three nodules), syphilitic growths (usually numerous), and carcinomatous deposits, which are characterised by their hardness, and usually occur an inch within the anus.

"5. In reference to the destruction of tissue—ulcers.

"6. In reference to the nature of the contents of that portion of the pa  
(Wunderlich, vol. iii, p. 799.)

III. From his remarks on *the indications afforded by the digestive powers*, we have condensed the following observations on the different forms of dyspepsia.

1. There may be absolute, or almost absolute, loss of the digestive power; the appetite being entirely gone, and any food which is taken exciting severe pain, and being very soon vomited. This condition occurs in acute gastritis, and towards the close of all gastric diseases which are going to terminate fatally (as cholera, and ulceration). It is occasionally manifested in nervous affections of the stomach, when it may exist for a long time with occasional remissions, and may lead the practitioner into the error of supposing that the case is one of ulceration or cancerous degeneration.

2. Mild food of easy solubility, and taken in small quantity, may be digested; a condition which we observe in cases of slight gastritis and gastric catarrh, and in many cases of degeneration and ulceration; and which forms, as it were, the intermediate link between healthy digestion and the complete loss of the digestive power.

3. Ordinary kinds of food, even when taken in considerable quantity, may be digested; but some particular substances, which would afford no trouble to a perfectly healthy stomach, may remain for a day or longer undigested, and perhaps then be vomited, giving rise to a feeling of pressure or weight on the stomach, and to the development of acidity, hindering the digestion of other matters, and sometimes even exciting gastrointestinal catarrh. We often meet with this form of dyspepsia in persons between forty and fifty years of age, who have indulged too freely in the pleasures of the table. It seems to be induced by repeated attacks of gastric catarrh; it usually increases as the patient advances in years, and sometimes proceeds to such an extent, that there are at last few articles of food which the stomach will bear.

We often observe it in cases of convalescence from diseases in which the digestive organs have been much implicated.

We are totally ignorant of the anatomical or physiological condition which induces this functional disturbance.

4. Digestion may be laborious, and only effected with the aid of stimulants, as spices, strong wines, coffee, &c. This condition differs from the former in being always chronic. It is most commonly associated with pyrosis, cardialgia, or some other gastric symptom.

5. Although the gastric digestion may be effectually performed, it is laborious, exciting feelings of weight in the stomach, of thirst, dryness of the mouth, shivering, lassitude, heat, and heaviness of the head; the hands and feet being cold, and the face flushed. These symptoms usually disappear in an hour or two after each meal. This condition chiefly occurs, as a transitory affection, in the period of convalescence from acute diseases in which the stomach has been implicated; sometimes, however, it has a persistent tendency, in which case it is probable that there is more or less hyperæmia of the stomach.

6. The gastric digestion may be much protracted; easily digestible matters remaining for many hours in the stomach, and imparting their odour to the eructations. The patient, in cases of this nature, complains

of lassitude and indisposition. The physician must be cautious in forming his diagnosis; for this condition may depend on irritation and on slight gastric catarrh, as well as on a torpid discharge of function; in which latter case it coincides with our fourth category.

7. Painful sensations may supervene towards the conclusion, or after the termination, of gastric digestion. They occur both in diseases of the pylorus and duodenum, and also in those of the pancreas and liver.

8. The gastric and intestinal digestion may be imperfect, and a portion of the food be rejected in an undigested state. This is a doubtful sign, from which no very certain conclusion can be drawn.

9. During the intestinal digestion, there may be an extraordinary development of gas, partly escaping by eructation, and partly retained, and giving rise to much pain. The causes of this development of gas, except so far as they depend on the nature of the food, or on a stagnation and accumulation of the fæces, are altogether unknown.

10. The contents of the intestine may pass too rapidly through the canal; but regarding this condition of the viscera we have no definite knowledge.

11. The contents of the intestine may be retained too long; and losing their moisture, may become hard, and assume the character of concretions. This condition depends on the nature of the food, on the occurrence of mechanical impediments, on the circumstance of the mucous membrane not being sufficiently sensitive to the impression excited by its contents, or on the imperfect contraction of the fibres of the muscular coat.

IV. In his remarks on the signs to be derived from the movements of the intestinal tract, he first notices *vomiting*, which, although ordinarily induced by some irritation of the stomach or duodenum, either primary or secondary, may also occur when the stomach is healthy or even paralysed, and the duodenum perfectly normal, if there is great irritation of the diaphragm, and the abdominal muscles contract powerfully.

The conditions under which vomiting may occur, independently of any direct irritation of the intestinal tract, are numerous. It may arise from a peculiar state of the brain or spinal cord, from impressions produced on the senses, from a poisoned state of the blood, or it may be excited merely by a fit of coughing or laughing. Our author mentions the following as the most important of the *intestinal conditions* giving rise to this symptom.

It may arise from simple overloading of the stomach. Lukewarm fluids, fatty food, matters evolving much gas, blood effused into the stomach, bile, large quantities of mucus or gastric juice, usually excite vomiting.

It may arise from various local mechanical actions, from bodily injuries, prolonged jolting (as in travelling in carriages without springs), from blows or pressure on the epigastric region (tight-lacing will sometimes excite it in young women, and an accumulation of gas in the transverse arch of the colon is a very frequent cause in children), the irritation of foreign bodies (intestinal worms for instance) in the pharynx, œsophagus, and stomach.

It may be consequent on displacement of the stomach;—as, for instance, when it lies in large hernial sacs, or when it sinks into the lower abdominal region, or when it is drawn from its natural position by the enlargement of adjacent organs, by morbid products, or by traction of the omentum

and transverse colon: or on congenital and acquired anomalies in the structure of the stomach;—as, for instance, too wide a cardiac extremity, too contracted a pylorus, or annular cicatrices.

It may depend on rapid and severe hyperæmia of the stomach. This is probably the cause of the vomiting in cholera, typhus, yellow fever, hepatitis, and dysentery.

It may be connected with alterations in the texture of the stomach, with induration, cancer, softening, ulceration at various spots, or inflammation of the peritoneal coat.

It may very probably arise from an independent affection of the gastric nerves. To this cause may be attributed the vomiting that occurs in cardialgia, and that prolonged vomiting (lasting sometimes for months, and even years), which we not unfrequently observe in young women, and in which we are unable to detect any local or other changes.

It may arise from severe irritation of the duodenal mucous membrane, from inflammation, distension with bile, or tumours; or from hinderance to the passage of the feces from any cause, the antiperistaltic motion gradually extending upwards from the point of obstruction till it reaches the stomach, and then causing vomiting.

After a vivid description of the sensations preceding vomiting, our author gives the following description of the act itself, and of the dangers accompanying it:

“The motions on which the act of vomiting depends, consist in a powerful contraction of the diaphragm and the abdominal muscles, accompanied in most cases by a contraction of the stomach itself in all directions, but especially from the pyloric extremity towards the fundus. It is only rarely, and in an imperfect manner, that vomiting takes place without these contractions of the stomach; and much more rarely, if indeed ever, that it occurs without abdominal pressure. By the combined contraction of the muscular fibres, the contents of the stomach are ejected into the cavity of the mouth, and gush out by the mouth and nostrils. The person vomiting ordinarily feels, especially at the commencement, dragging pains in the region of the stomach, the testicles are retracted, the anus is spasmodically contracted, the blood is prevented from returning from the head, the face is frequently red, turgid, and bedewed with perspiration, the eyes protrude, all the arteries in the head may be felt to throb, and the head itself feels as though it would burst. The urine frequently escapes, there is often a discharge of thin feces, and in cases of leucorrhœa mucus is expelled from the vagina. Hernias may be produced, pulmonary caverns and abscesses may burst, and even apoplectic extravasations may occur from these efforts. Particular spots, as on the thorax, abdomen, or spinal column, in which there was previously no special sensation, now become painful. There are often general tremors, and spasmodic rigidity of certain muscles, the vomiting frequently terminating in a severe spasm of the diaphragm, of the muscles of the neck, and of the œsophagus; at its termination it sometimes leaves the patient in a state of great prostration.

“Immediately after the vomiting the patient almost always feels himself relieved; he looks somewhat pale and depressed, but his distress has temporarily disappeared, the pulse has become quiet, a uniform warmth is diffused over the body, the head is freer, and the patient enjoys a feeling of comfort and quietude strongly contrasting with his sensations before he vomited. He is now only annoyed by severe thirst, and a troublesome sensation of heat in the throat. This condition of quietude is however usually only of short duration, the precursory symptoms of another fit of vomiting soon supervene, which is usually more severe and painful than the first, for as the stomach is now comparatively empty, the efforts at vomiting must be much more forcible in order to eject the small amount of gastric and duodenal

secretion which has accumulated since the previous act. The matters which are ejected during these severe efforts are at first sour, then bilious; and finally, in very prolonged vomiting, may contain *feces* (ileus). In proportion to the frequency of the vomiting, are the prostration and distress of the patient. He can hardly raise himself, and there is a sensation of swimming in the head; an indescribable feeling of misery and weakness overpowers him, he is ready to fall, is dejected and faint-hearted, and feels that he has attained to the highest pitch of corporeal and mental disorder. The slightest personal effort is an impossibility. At length the mental apathy and perfect indifference, which rendered him careless of all surrounding objects, of his dearest friends, or of his own personal decencies, somewhat moderate; he resigns himself passively to his fate, and has hardly energy to desire for a cessation of his miseries, and scarcely knows whether death or recovery is preferable." (pp. 797-8.)

In considering the diagnostic value of vomiting, our author very correctly remarks, that the physician must always attempt to ascertain its actual and direct cause, although indeed this is not always possible. He may usually satisfy himself with comparative certainty whether or not it depends on overloading of the stomach, on the ingestion of poisonous matters, on a hernial incarceration (especially if at the same time there is obstinate constipation), on epidemic diseases accompanied with vomiting, on pregnancy, or on the presence of cerebral injury or disease. In reference to diagnosis, much may be learned from the nature of the vomiting, from the repetition or non-repetition of the act, and from the general circumstances under which it occurs.

If there be only a single fit of vomiting, or two or three rapidly succeeding each other, gastric irritation is the most probable cause; and if the tongue, which before vomiting was foul, is now clean, and the appetite is restored, we know that this irritation has been removed by the vomiting. Obstinate vomiting, recurring at very short intervals, is indicative of some cause which is not easily removed; as, for instance, poisoning, intense hyperæmia, textural changes, incarceration of some portion of the intestine, foreign bodies, a morbid condition of the blood, &c. Obstinate vomiting, continuing for a length of time, indicates a firmly-seated neurosis, or a chronic alteration in the form or structure of the stomach. The comparative ease or difficulty with which the act of vomiting is accomplished, may afford some assistance in diagnosis. When vomiting comes on spontaneously, that is to say, independently of the introduction of food, &c. into the stomach, we must fear severe disease of the stomach or intestines, or an affection of the brain or of the blood. As a general rule it may be laid down, that the rapidity with which the act of vomiting follows the ingestion of food, varies in a direct ratio with the acuteness of the affection, and inversely with the distance of the diseased part of the intestinal tract from its upper extremity.

*Constipation* and *Diarrhœa* are duly noticed in this section, the movements of the intestinal canal being diminished or imperfect in the former case, and increased and more frequent in the latter.

It is impossible to give a strict definition of "constipation," as the idea associated with the word is merely relative; our author conceives, that when the bowels have remained unmoved for two or three days, constipation may be regarded as established. He observes, that cases are not very rare, in which the bowels remain unmoved for two or three weeks; and we can confirm the remark from our experience in a large metropolitan dis-

pensary. It is in women that we usually find this torpid condition of the bowels.

As very serious errors in treatment may result from mistaking the cause and origin of constipation, our author enters somewhat fully into the different causes of this symptom, and the means of diagnosing them.

It may arise (he observes) from a mechanical impediment to the progress of the fæces, depending on the mal-position of some portion of the intestine (as in hernia, invagination, twisting of the gut, &c.), on an error in structure (as on projecting valves, diverticula, tumours, cicatrized contractions, imperforate anus, &c.), on dryness of the mucous membrane (as, for instance, at the commencement of gastric catarrh), on the presence of hardened lumps of fæces, or of foreign bodies obstructing the intestinal tube, on pressure exerted on the intestine from without (as by tumours, adjacent organs undergoing degeneration, or even by some other portion of the intestine), on spasmodic contraction or hypertrophy of the muscular fibres (which, however, is rare, and only occurs between the sigmoid flexure and the rectum, or at the sphincter ani), or on painful affections of the anus (as, for instance, fissures).

Again, it may depend on imperfect contraction of the muscular fibres, weakness of the muscular layer, or actual paralysis of it. Amongst the most important conditions giving rise to this state of debility and paralysis of the muscular coat of the intestinal canal, we may notice :

*a.* Congenital weakness and imperfect formation of the muscular coat, as is the case in weakly, prematurely born infants.

*β.* Imperfect activity and want of tone in the muscular layer consequent on deficient nutrition, in persons of torpid habit of body, in bedridden persons, in those who lead very sedentary lives, in aged persons, in those in whom the portal circulation is impeded, &c.

*γ.* Muscular inactivity consequent on the absence of sufficiently stimulating matters in the food, or on the presence of thick mucous investments or plastic exudations, separating the intestine from its proper stimulus; or, finally, on a deficient capacity of the mucous membrane to convey impressions, and thus to excite the muscular fibres to contraction. The first of these conditions presents itself in cases when the food has for a length of time been non-nutritious and scanty; the second in cases of intestinal catarrh; and the third, which is the most frequent, after the abuse of drastic purgatives, after the prolonged use of a highly stimulating diet, after a long course of narcotics, and on the subsidence of other intestinal diseases.

*δ.* An atonic state of the muscular fibres in consequence of some peculiar condition of the blood, (as, for instance, when the biliary constituents are retained in the blood, in saturnine intoxication, &c.)

*e.* Excessive distension of the intestine by gas or fæces, preventing efficient muscular contraction.

*ζ.* Transitory deficiency of muscular activity, after it has been powerfully manifested, as after diarrhœa, or the use of strong aperient medicine.

*η.* Paralysis of the muscular fibres occurring in peritonitis, and in diseases of the brain and spinal cord; and finally, that substantive paralysis of the intestine which is usually restricted to a limited spot, at which the fæces are retained, till they are at length forced onwards by a powerful contraction of the superior portion, which still retains its muscular energy.

We believe that, under the above heads, are included all the proximate causes of constipation; it must, however, be borne in mind, that two or more of them often co-operate in the same individual.

The following remarks are condensed from our author's observations on the diagnostic import of the different varieties of diarrhœa:—

The occurrence of easy and painless evacuations is merely a negative sign; it shows only that there is no impediment to their passage, and that the rectum is not diseased; on the other hand, they become difficult and painful when there is some impediment to their free passage, when the matters discharged are very tenacious and viscid, or very thick, hard, and firm; or, finally, when, the intestinal canal being nearly emptied of its contents, the muscular contractions continue.

Painless evacuations occur at the commencement of slight diarrhœas, and occasionally in those of an obstinate chronic character, as, for instance, in the diarrhœa of phthisical patients. Pain is excited in the parts through which the discharge passes, when they are inflamed, excoriated, or ulcerated, when tumours protrude into the canal, and when hard fecal masses are present, and the muscular efforts are not duly proportioned to the mass they have to propel. It is difficult to say on what the colic-like pain depends, which often precedes or accompanies the fecal evacuations, not only in comparatively trifling cases of diarrhœa, but also in cholera and dysentery, in the diarrhœa consequent on certain forms of poisoning, frequently in the diarrhœa of phthisis, and occasionally in that of typhus.

When the patient has no control over the action of the bowels, and is unconscious of the passage of the feces, there is paralysis of the sphincters and of the lower portion of the rectum, which may be local, or dependent on the condition of the central nervous organs, or on the general state of the vital powers.

By observing the period of the day at which the looseness especially occurs, we may often obtain a useful diagnostic hint. When it comes on during the night and continues till morning, but then speedily stops, it usually depends on indigestion or on a chill. When it regularly comes on very early every morning, wakening the patient from his sleep, we are led to suspect that it depends on tuberculosis. When it appears soon after meal-times, it is indicative of great irritability of the intestines. The diarrhœas occurring in summer and in hot countries originate in chills, errors of diet, and epidemic relations; those occurring shortly after removal to another country (before a person has become acclimatised) are not serious, unless such diseases as dysentery, cholera, or intermittent fever happen to be simultaneously prevalent. No diagnostic inferences can be deduced from the number of the stools.

In reference to age, it may be observed, that diarrhœa is comparatively frequent in young children; and that if it continues any length of time, we must suspect that it depends on enteritis, and that it may lead to softening of the intestine, to serous exudations within the membranes of the brain and spinal cord, and to rapid general exhaustion. In older children, diarrhœa is often dependent merely on the presence of worms; if, however, it continue for any time, we must be apprehensive of ulceration and tuberculosis of the intestines. In young adults, diarrhœa is seldom a dangerous symptom, unless when typhus fever or cholera is prevalent. In aged persons it is rapidly fatal, soon prostrating their enfeebled



energies, and frequently giving rise to an œdematous condition of the lungs and brain. There are persons, otherwise apparently in good health, who have an extraordinary tendency to diarrhœa; a chill, any error in diet, any deviation from the ordinary mode of life, any anxiety or annoyance, even a long walk, is sufficient to induce it; it is almost their ordinary habit of body, and they appear not to suffer from it. In very debilitated persons, diarrhœa presents the same dangers as in aged persons.

In pregnancy it does not occur so frequently as constipation, and is less annoying than that condition of the bowels; if it be severe, we must fear that it will cause abortion. After delivery, severe diarrhœa is always a very dangerous symptom; it ordinarily assumes a dysenteric character, and is of a very chronic nature.

5. *The indications afforded by the character and properties of the dejections*, constitute the last group of direct symptoms.

A. Matters discharged from the intestinal canal by the mouth, or vomited matters, may consist of—

a. Food, other ingesta, and their remains.

β. Or altogether or in part, of substances which are produced or secreted in the intestinal canal. Thus *mucus* and *gastric juice* are almost always present. The mucus is distinguished by its viscosity, transparency, and mawkish taste (we are quite willing to take Wunderlich's authority for the last statement without repeating the experiment), and occurs in gastro-intestinal catarrh, in all cases in which digestion is very prolonged, and in incipient degeneration. *Free acids* are present in the vomited matter in cases of indigestion, in chronic gastritis, cardialgia, incipient degeneration, very frequently in the gastric derangement of drunkards, and in the softening of the stomach and other fatal gastric diseases of infants. In these cases the vomited matter has an acid odour and reaction, and the patient often complains of a sensation in the pharynx and mouth resembling that which would be caused by a caustic acid. *A thin fluid, containing flocculent matter* (cast off epithelium), is vomited in cholera, and sometimes in cases of poisoning. *Bile* occurs in all cases in which the act of vomiting is frequently repeated in a short time, provided that the bile finds its way freely into the duodenum. *Pancreatic juice* is probably not an unfrequent constituent of vomited matters, occurring principally in those chronic cases where very large quantities of a thin, lightish-brown, rather frothy fluid are ejected from the otherwise empty stomach. We have, however, no certain chemical test by which we can recognise the presence of this ingredient. We need say nothing of the dangerous import of *fecal matter*; it may be detected at once by its odour. When *blood* is present, it almost invariably proceeds from the stomach; in some very rare cases only from the œsophagus. It usually has a blackish tint, from the action of the acid occurring in the gastric juice; it may arise from a wound, from ulcers, scirrhus and encephaloid disintegration, from rupture of the vessels of the stomach, from serious alterations in the circulating fluid itself (as for instance, from yellow fever, severe typhus, or scurvy); and, finally, it may occur frequently, and in very considerable quantity, in cases in which the walls of the stomach are perfectly uninjured, and independently of any known change with blood itself,—in fact, as true capillary apoplexy. *Pus-corpuscles* may be found in cases of ulceration of the stomach, of the upper part of the small intestine, and of the œsophagus; or in cases of

abscess opening into any of these parts. Whether they occur in the matter vomited in simple chronic gastritis, is uncertain. The peculiar coffee-ground-like fluid which is sometimes ejected, is a mixture of cancerous discharge and blood, and is indicative of open cancer of the stomach or œsophagus.

γ. Parasitic formations are occasionally found in vomited matters, being ejected from the stomach, or occasionally coming from some neighbouring part, as, for instance, the liver. The common round-worm is the most frequent of these parasites; but the tape-worm and acephalocysts are occasionally vomited. Fermentation-cells and the sarcina (to which we shall specially allude in our next article), are also not unfrequent constituents of the matters ejected from the stomach.

β. The fæcal dejections have never yet been fully studied; and, except in the case of a very few diseases, our ideas regarding this somewhat uninviting subject, are very far from correct. The augmentation and diminution of the amount of the fæces, and deviations in their consistence, form, colour, and smell, are described at some length. Those cases where ingredients, which normally are absent, or present only in very small quantity, enter into the composition of the fæces, are then noticed. Under this head Wunderlich refers to foreign bodies which have found their way into the intestine;—undigested fragments of food;—water separated in large quantity from the walls of the intestinal canal;—epithelial scales;—mucus (thin mucus especially occurs in acute catarrh of the colon and small intestine; thick, stringy mucus occurs chiefly in chronic catarrh, and in affections of the rectum);—pus, a sign of severe inflammation of the intestinal mucous membrane, or of abscesses having opened into it; when the pus and fæcal discharge are intimately blended, we may assume the pus has its sources in the upper part of the intestine;—fluid albumen, occasionally occurring in the stools in various forms of diarrhœa;—coagulated albumen and fibrin, shreds of pseudo-membrane in inflammatory affections of the intestine, especially of its lower portions;—blood, in gastric hemorrhage, in ulceration of any part of the intestines, in cases of hardened fæces or sharp foreign bodies passing along the intestines, in dysentery, in hemorrhoidal hyperæmia, in cancerous deposits, in necræmia; if the blood comes from the stomach or small intestine, it is usually black, and, as it were, carbonised; when it proceeds from the lowermost portion of the gut, it is comparatively unmixed with the fæces;—a peculiar colouring matter (noticed in Germany by Vogel and Heller, and in this country by Parkes), which reddens on the addition of nitric acid; it is most abundant in cholera-stools, but occurs also in other forms of diarrhœa;—sugar has been stated to have been found by some chemists in the fæces of diabetic patients; its presence there is, however, extremely problematical;—oily and tallow-like fat, chiefly in diseases of the duodenum and pancreas; the recent investigations of Bernard (see *British and Foreign Medico-Chirurgical Review*, Vol. III, p. 527) throw much light on the cause of these fatty stools;—sloughy matter in very bad ulcers and in gangrene;—portions of intestine, occasionally amounting to several feet, in the natural cure of intus-susceptio;—fragments of encephaloid matter which may be recognised by the microscope;—urine in cases of rectovesical fistula;—large quantities of ammoniaco-magnesian phosphate, principally in typhus, but also in other affections;—carbonate of ammonia,

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always dread in these cases. A single venesection at the commencement of cholera or dysentery may, however, be very serviceable; and in other forms of intestinal disease, especially when the patient is of a plethoric habit, or there is any threatening of thoracic complication, an early bleeding may be permitted. Local bloodletting (by leeches) is very strongly recommended. "Although the want of connection between the blood-vessels of the intestine with those of the spot on the abdomen at which the leeches are applied, prevents a ready explanation of the beneficial result, the fact on that account is not the less certain." Amongst other local applications, he recommends warm cloths, and, more strongly, warm, moist cataplasms, as tending not only to diminish pain, but to obviate constipation. To sinapisms and blisters he seems much opposed. "They are of doubtful utility in relation to the intestine, and often appear to act unfavorably on the kidneys." This condemnation—in relation at least to sinapisms—is, we think, too general. Ice-bladders are in very rare cases useful, namely, in intensely acute affections. He speaks doubtfully regarding the advantages to be derived from the inunction of mercurial ointment in acute intestinal diseases.

Neutral salts in small doses as, for instance, sulphate of magnesia (about two drachms daily), the alkaline citrates, acetates, and chlorides, and especially sal ammoniac, in doses of a scruple daily, and tartar emetic in minute doses, appear to exert a slightly irritating action on the intestinal mucous membrane, and to increase the flow of the ordinary secretions into it. Emetics are next noticed. We have already had occasion to allude to the dangers which, according to the belief of Wunderlich and other German physicians, pertain to the administration of this class of remedies; and we have quoted somewhat fully our author's graphic and, as we thought, somewhat over-coloured sketch of the act of vomiting (see pp. 476-7). He grants, however, that they are serviceable in cases where injurious or noxious matters have been taken into the stomach, as also in cases when a large quantity of mucus or bile is suspected in the stomach, or when an intestinal catarrh has existed for some time; and that, if administered early enough, they may often cut short an acute affection of the intestinal canal. At length we discover (p. 827), that "the most common emetic is a combination of ipecacuanha and tartar emetic, a scruple to half a drachm of the former, and one to four grains of the latter;" and now we can fully understand, that "common emetics" in Germany may produce all the potent effects ascribed to them by our author, and that "it is seldom, except in very obstinate cases, necessary to repeat them more than once or twice in twenty-four hours."

Laxatives are regarded as of much more problematic use than emetics in acute intestinal affections. They are especially to be avoided, when there is a threatening of a typhous affection.

"Narcotics (he observes) find a frequent application in acute diseases of the intestinal canal, and (independently of their remote action on the brain and spinal cord) they have a double action on the organ itself; moderating the sensibility of the mucous membrane, to which end we give small doses, and commonly the milder medicines (hydrocyanic acid); and, further, controlling the motions of the muscular coat of the intestines, with which view we give stronger medicines in full doses (especially opium and morphia); this class of medicines is especially useful in obstinate vomiting, in profuse or persistent diarrhoea, and when perforation of the intestine is threatened." (p. 828.)

We may add, that after perforation has actually taken place—small though the chances of the patient's recovery be—opium is the only remedy in which we can place the slightest hope.

Passing over Wunderlich's general remarks on the treatment of chronic cases of intestinal disease, we proceed to notice his mode of treating certain individual symptoms. There are, as he very truly observes, few forms of disease in which we have more often to confine ourselves to the treatment of symptoms, than in those of the intestinal canal; and this is the case, partly because our diagnosis is so often uncertain, or even assuming it to be correct, because we cannot bring our means to act directly on the morbid process (as in typhus, cholera, intestinal tuberculosis, cancer, &c.); and again, partly because the diseases of the intestinal canal are especially liable to give rise to extremely painful, severe, and highly dangerous conditions, against which our leading efforts must often be temporarily directed. Passing over his directions regarding anorexia and nausea, we find him advising, that for the burning thirst which often accompanies gastric and intestinal disorders, ice in very small portions should be administered, or iced water, or slight acidulated drinks, mixed with a little syrup. As a remedy for those pains in the large intestine, which we recognise under the name of colic, he mentions a mixture of equal parts of the tinctures of *nux vomica* and *asafoetida*—a prescription most highly eulogised by Rademacher.

His remarks on tympanitis are good. In chronic cases, he has seen much benefit derived from the application of a firmly applied bandage to the abdomen. In extreme cases, when life is actually in danger, the intestine must be punctured through the abdominal walls.\*

The means of checking vomiting, enumerated by our author, are somewhat numerous, and some of them will, we conceive, be new to most of our readers.

"The simplest method of moderating vomiting is rest and the horizontal position. A warm and aromatic application to the epigastric region is also useful; repeated sinapisms, or a tartar emetic plaster on the region of the stomach, sometimes check an obstinate habitual vomiting; moreover, a bladder filled with ice, and similarly applied, will, in many cases, prove serviceable. The most important internal means are:—1. Carbonic acid in all the forms of its application; it is, however, useful only in the milder forms of vomiting; in the more severe cases, it seems even to excite the vomiting. 2. Ice, a palliative means in severe vomiting. 3. Narcotics, of which the best are hydrocyanic acid (the mildest), belladonna, opium (the most certain), *nux vomica* (in chronic cases); these are, however, mostly only palliatives in severe cases. 4. Vegetable and mineral acids (citric acid, acetic acid, sulphuric acid). 5. Small doses of salt (of common salt, sulphate of potash, sulphate of magnesia, muriate of ammonia, muriate of lime, and of salted meat). 6. Bitters, as quassia, gentian, wormwood, ox-gall, black unsugared coffee; these, however, are only available in the less severe forms. 7. Aromatic and ethereo-oleaginous remedies will often stop moderate vomiting, and in more severe cases, they will moderate it, and render it less painful; as infusion of chamomile, oil of peppermint, cinnamon, oil of cloves, oil of turpentine. 8. Alcohol and ether, especially in the chronic forms of vomiting, taken in the form of the finer wines, as Malaga, Tokay, or Marsala, or in the form of certain liqueurs, as spirits of wormwood or spirits of aniseed, or as sulphuric or acetic ether. 9. *Ipecacuanha*

\* This operation, according to Wunderlich, was first performed on the human subject by Levrat (*Nouv. Biblioth. Méd.*, 1823). Zeis (*Abhandl. aus dem Gebiete der Chirurgie*, 1845), and Groell (*Walther und Ammon's Journal*, 1847), have also advocated this treatment.

in very minute doses. 10. Creosote. 11. Certain metallic remedies, as the soluble salts of iron, calomel, bismuth, nitrate of silver, zinc, and lead in small doses. 12. Tincture of iodine (strongly recommended by Rademacher, and found useful by myself, when given in doses of four to ten drops in some mucilaginous vehicle); and many other remedies." (pp. 833-4.)

There is nothing very novel in our author's observations on the general treatment of constipation and diarrhœa. With the single remark, that in the treatment of the latter affection he very properly lays great stress on the importance of the patient always retaining the contents of the bowel as long as he can, and that *Trifolium fibrinum* and *Monesia* are included in his list of remedies, we proceed to the consideration of the *Special Diseases of the Digestive Canal*.

We shall continue to follow Wunderlich's arrangement, and shall divide the diseases of the intestinal canal into

- I. *Those unaccompanied by a corresponding anatomical change*; and
- II. *Those with corresponding anatomical changes*.

The former are subdivided into

- A. *Irritative forms of disease*; and
- B. *Paralytic forms of disease*.

The first of the irritative forms of disease to which we shall advert, is *Gastralgia*, an affection known as also as *Gastrodynia*, *Cardialgia*, *Neurosis of the stomach*, &c.

Although many writers of the eighteenth century paid much attention to this affection, it was not till M. Barras published his '*Traité des Gastralgies et des Entéralgies*,' in 1827, that the distinction between inflammatory diseases of the stomach and the gastric neuroses was fully recognised.

We have endeavoured to sum up the ordinary causes of gastralgia in the following paragraph.

It is a very common disease of middle life, and of incipient old age; it is rather more frequent in the female than the male sex; although it is most common amongst those whose diet is poor and insufficient (as, for instance, amongst the London sempstresses, whose principal food is bread and butter, and weak tea), it is not unfrequent among those who live freely. Wunderlich remarks that in his neighbourhood it is very common among the farmers during summer, when their occupations in the open air prevent them from taking their usual meals with regularity, and when they are in the habit of taking large draughts of cold water, cider, &c. Any chill to the mucous membrane of the stomach, or the sudden suppression of any ordinary secretion, will often induce it.

Moreover, it not unfrequently arises from primary affections of other organs, as, for instance, from strong psychical impressions and emotions, from cerebral disease, and more frequently from spinal irritation, which, indeed, is often detected by the anomalous character of the gastric pain. It sometimes alternates with other forms of neuralgia; it is induced by sexual excesses, by pregnancy, by dysmenorrhœa, leucorrhœa, and various other disorders of the female generative system; occasionally by affections of the rectum, as, for instance, hæmorrhoids or cancer; by certain anomalous conditions of the blood, as by plethora, and more frequently by anæmia, by the uric acid diathesis, by uræmia, and pyæmia. Most

writers on this affection regard it as hereditary; we are not aware, however, of any numerical data bearing on this point.

There is considerable obscurity regarding the pathology, or, as Wunderlich terms it, "the pathological physiology" of this disease. The most essential symptom is pain; and the question naturally arises, if gastralgia is a primary painful affection, which set of nervous filaments is principally implicated,—those of the pneumogastric or of the sympathetic nerve? We have as yet hardly sufficient data to answer this question. While some circumstances, as, for instance, the changes of appetite frequently observed in this disease, and the influence which psychical emotions exercise on gastralgia, would lead us to infer that the pneumogastric is the seat of the disease; others again, as, for instance, the spinal tenderness or irritation, which is of such frequent occurrence, and the various painful sensations which are usually present (and which we cannot ascribe to the pneumogastric nerve), seem to indicate that the seat of gastralgia must be sought for in the fibres of the sympathetic. There are, however, as far as we can see, no reasons why both sets of fibres should not sometimes be implicated.

Another question naturally suggests itself, whether in gastralgia the disease must be regarded as lying in the peripheral distribution of the nerves, or whether the pains do not radiate from a central point in which the disturbance is situated—that central point being either the coeliac plexus or the spinal cord? As far as affording a rational explanation of the symptoms is concerned, it is unimportant if the diseased spot occur in the course of sensitive nervous fibres, whether it be at their periphery or at any part of their trunk, or in the central organ itself. Without maintaining that gastralgia is *never* a central affection, we may safely assert, that in some cases, if not in all, it depends upon a disordered condition of the peripheral extremities. The question is, however, happily one of comparatively little practical importance, in reference either to the interpretation of the symptoms, or to the treatment of the disease.

In reviewing the symptoms of this affection, the first we shall notice is *the pain*. In a diagnostic point of view, it is of much importance to distinguish between *spontaneous pain* and *pain excited by pressure*. It is the former with which we almost invariably meet in this disease. The nature and character of the pain are most variously described by different patients—some speaking of a pain like that which a piece of red hot iron might be supposed to excite in the stomach; others complaining of a sensation of twisting, of violent constriction, &c. Valleix states, that in only three of the twenty-nine cases on which he grounds his remarks was the pain continuous, and that in these three there was an aggravation of the pain either immediately after taking food, or during digestion.

In all the other cases there were well-marked intermissions. Indeed, the paroxysmal character of the pain is one of its most marked peculiarities. The attack is sometimes almost of a momentary duration, but it usually lasts from some minutes to an hour or more. There is then an interval which may extend over several days, during which the patient may feel himself perfectly well. As, however, the disease progresses, the intervals become shorter, and the paroxysms recur several times daily. The attack sometimes comes on in the morning when the stomach is empty, and is checked or moderated by the ingestion of food; it sometimes supervenes

after the use of certain kinds of food and drink, especially those of an acid, cold, indigestible, and flatulent nature; and it is often induced by a sudden mental shock, by over-exertion, or by a chill. In chronic cases, an accession of pain most commonly follows all the principal meals. The pain is concentrated in the epigastric region, but in severe cases extends to a greater or less degree over the thorax, spinal region, and indeed to the shoulders, and along the arms. The old idea that the cardiac extremity of the stomach is the exclusive seat of this affection, seems utterly unfounded.

The following may be regarded as the direct and the most frequent consequences of gastralgia. When the painful sensations have attained a considerable degree of severity, the patient feels himself intensely depressed and miserable; he is unable to make any exertion, his voice becomes harsh and rough, and black specks (*muscæ volitantes*) are always floating before his eyes. In the most severe cases the hands are cold, the forehead is covered with perspiration, the pulse is irregular and small, there is vertigo, and occasionally even syncope. There is also great tenderness about the region of the seventh and eighth dorsal vertebræ, which, although most marked during the paroxysms, may be detected by pressure during the intermissions. Moreover, associated sensations and reflex motions are very frequent in gastralgia; amongst the former we may place the burning sensation in the throat (*pyrosis*), the feeling of constriction of the chest, the *besoin de respirer* of the French writers (*Athmungsmoth*), we have no corresponding word or phrase in our language), and the colic-like pains which occasionally occur, although less frequently than the previously mentioned sensations. Amongst the latter (reflex motions) we may mention yawning, hiccup, palpitation of the heart, cramp in the legs, and short spasmodic contractions of the abdominal walls.

None of our authors throw any light upon the connection between water-brash and gastralgia. Wunderlich observes that the cases in which water-brash occurs are neither those which are characterised by peculiar severity, nor by peculiar mildness; and that no plausible reason can be assigned why it occurs in one case and not in others.

Many writers have regarded vomiting as a very constant symptom of gastralgia—almost as constant as the pain. Valleix has shown that this idea is altogether erroneous; it only occurred in ten out of thirty-seven cases collected or observed by him. Moreover, when it occurs, the vomiting in gastralgia is not constant, as in cancer of the stomach and in chronic gastritis, but merely occasional.

The tongue presents no indication of importance.

The bowels are usually very constipated, and there is sometimes considerable intestinal flatulence. When the last-named symptom is very severe, we must apprehend the existence of incipient cancer of the stomach.

It is difficult to say whether gastralgia ever merges into other diseases. The cases on record in which it has been stated to have terminated in epilepsy, hysteria, and insanity, are very doubtful; and there can be little question that those in which it is said to have passed into inflammation, hæmatemesis, ulceration, cancer, contraction, and dilatation of the stomach, &c., are mere examples of erroneous diagnosis.

The cases in which gastralgia proves fatal, through the prolonged pain, continuous vomiting, and increasing debility, are extremely rare.

The diagnosis of gastralgia often presents very serious difficulties ; we may mistake the more acute forms of the disease for the slight gastric disturbance, which, although very common, has hardly a specific name in our nosological tables (*l'embarras gastrique* of the French writers), or for acute gastritis ; while the ordinary, more chronic forms may be mistaken for chronic gastritis, simple ulcer of the stomach,\* cancer of the stomach, or for epigastric pains dependent on intercostal neuralgia.

The following table, extracted from Valleix, points out the most important distinctive signs between these several affections and gastralgia :

I. GASTRALGIA—ACUTE FORM.	GASTRIC DISTURBANCE.
<i>Acute pain</i> in the region of the stomach.	<i>Discomfort</i> , rather than pain, in the epigastric region.
Appetite <i>preserved</i> .	Appetite <i>lost</i> , or even a distaste for food.
No <i>headache</i> , unless as a complication.	Frequently supra-orbital <i>headache</i> .
Nausea <i>only after meals, or in the morning</i> .	Frequently distressing nausea at <i>different periods of the day</i> .
II. GASTRALGIA—ACUTE FORM.	ACUTE GASTRITIS.
Appetite <i>more or less preserved</i> .	Appetite <i>lost</i> .
<i>Little or no pain</i> on pressure.	Pain on pressure, frequently <i>acute</i> .
Vomiting <i>rare</i> , and composed of <i>mucus or of food</i> .	Frequently repeated <i>bilious</i> vomiting.
No <i>fever</i> .	More or less decided <i>febrile disturbance</i> .
III. ORDINARY GASTRALGIA.	CHRONIC GASTRITIS.
Not <i>usually complicating</i> other affections.	Usually <i>complicating</i> another affection.
Comparatively <i>rare</i> vomiting of <i>food or mucus</i> .	Frequent vomiting of <i>bilious and alimentary matter</i> .
Spontaneous pains, <i>often very acute</i> .	Spontaneous pains, generally <i>less acute</i> .
Usually <i>no pain</i> on pressure.	Always <i>more or less acute</i> pain on pressure.
Progress of disease <i>very irregular</i> .	Progress <i>less irregular</i> .
Absence of <i>fever</i> , unless as a complication.	Fever <i>generally present</i> .
IV. ORDINARY GASTRALGIA.	SIMPLE ULCER OF THE STOMACH.
Appetite <i>more or less preserved</i> .	Loss of appetite.
Vomiting a <i>considerable period</i> after food has been taken.	Frequently vomiting <i>immediately</i> after food has been taken.
Never vomiting of <i>black matter</i> or of <i>pure blood</i> .	Sometimes vomiting of <i>black matter</i> or <i>pure blood</i> .
Slow progress of the disease.	More rapid progress of the disease.
V. ORDINARY GASTRALGIA.	CANCER OF THE STOMACH.
Often vomiting of <i>glairy matter</i> or of <i>food, comparatively soon</i> after it has been taken.	Vomiting of food at a <i>longer period</i> after it has been taken.
Destroys <i>less rapidly</i> .	Destroys <i>rapidly</i> .
No <i>signs</i> of the cancerous cachexia.	<i>Signs</i> of the cancerous cachexia.
Hypochondriasis <i>frequent</i> .	Hypochondriasis <i>less frequent</i> .
Progress of the disease <i>very irregular</i> .	The disease progresses <i>regularly</i> , increasing in intensity.

\* By "simple ulcer of the stomach," we mean the "chronic ulcer" of Cruveilhier, and the "perforating ulcer" of Rokitsansky.—R.V.

## VI. ORDINARY GASTRALGIA.

EPIGASTRIC PAINS, DEPENDENT ON  
INTERCOSTAL NEURALGIA.

*Usually no pain on pressure.*

No shooting pains towards the dorsal region.

*No other painful spots* in the course of the nerves.

*Well-marked disturbance* of the functions of the stomach.

Epigastric region *painful on pressure.*

Shooting pains along the course of the nerve.

*Other painful spots*, similar to the epigastric painful spot.

*No disturbance* of the functions of the stomach.

It is not very rare to find these neuralgic pains complicating ordinary gastralgia.

We cannot convey to the minds of our readers so clear an idea of the extreme difficulty that sometimes presents itself in the diagnosis of this disease, as they will obtain by referring to Andral's 'Clinique Médicale,' liv. i, sect. ii, chap. 4, obs. 5. It is the case of a woman, aged 38 years, in whom chronic gastritis was diagnosed with apparent certainty, and cancerous infiltration of the sub-mucous tissue suspected. In describing the examination of the stomach after death, Andral candidly observes:

"Quel fut notre étonnement de le rencontrer dans l'état le plus sain! La membrane était blanche dans toute son étendue; nulle part elle n'était modifiée ni dans son épaisseur, ni dans sa consistance."

Valleix devotes no less than twenty-five pages to the treatment of gastralgia; while Wunderlich condenses his corresponding section into two thirds of a page. The former commences by proving, from statistical evidence, that bloodletting in every form, emollients, and very low diet, should be entirely rejected in this disease; and that emetics and strong purgatives should be also discarded. In these points, we fully agree with him. Antispasmodics are then noticed. Amongst these, he assigns the first place to the subnitrate of bismuth, which may be advantageously united with henbane, extract of lettuce, magnesia, or ipecacuanha, according to circumstances. Wunderlich states, that he has cured nearly all his cases (amounting to about 500) with bismuth, in one- or two-grain doses. A combination of bismuth, asafoetida, and valerian has been highly recommended by Albers and other German physicians. Zinc (especially the cyanide) and nitrate of silver, in doses varying from 1-20th to 1-12th of a grain, are the remedies which Wunderlich places next in order of value to sub-nitrate of bismuth. Narcotics—especially opium, belladonna, and lactucarium—have been highly extolled; and even if they do not effect a cure, their administration, in severe cases, is so essential to the temporary comfort of the patient, that it cannot be dispensed with. The best method of applying narcotics is externally,—on the epigastric region, by the endermic method. A small blister must be raised,\* and dressed daily with a small quantity of muriate or acetate of morphia (from half a grain to two thirds) combined with a little savine or other irritant ointment. Excitant medicines have long held a high rank in the treatment of gastralgia. They form the basis of all the stomachic or cardiac mixtures, of visceral elixirs, &c., which have ever found favour with the multitude in these cases. We may espe-

\* The quickest method of raising a blister is by the application, prolonged for a few seconds, of the hot iron hammer, recommended by Dr. Day in the treatment of neuralgic affections.

cially mention chamomile, mint, orange-peel, cloves, wormwood, canella, mace, allspice, saffron, and even alcohol, as common ingredients of these mixtures. Indeed, alcohol is almost always the menstruum adopted to extract the excitant properties of the drugs in this class of medicines.

Various compound powders of a similar nature have been prescribed, the most important ingredients being canella, nutmeg, cubebs, and coriander, often combined with rhubarb. Valleix speaks strongly in favour of this class of remedies, and regrets the timidity with which many physicians employ them. Tonics, either alone or combined with excitants, are often of great efficacy, especially the preparations of cinchona and iron. The following was a favorite prescription of Schmidtman\* in these cases:

Of Inspissated Osgall,	} each, 3ijss.
„ Extract of Gentian,	
„ Rhubarb,	
„ Carbonate of Iron, 3j.	

To be divided into 2-grain pills, six or nine of which may be taken during the day, especially before meals.

Alkaline medicines have been especially prescribed in those forms of gastralgia which are accompanied with acid eructations and pyrosis. They are, however, only of service in counteracting one of the symptoms—the abnormal acidity of the *primæ viæ*. Carbonate of ammonia, carbonate of potash, magnesia, &c., are far less efficacious than the mineral waters of Vichy, Neris, Pougues, &c.

Amongst other remedies which have found strong advocates in gastralgia, we may mention cod-liver oil (Münzthaler), tincture of guaiacum (Elison), phosphoric ether (Löbstein), nux vomica (Schmidtman), electricity (Gourdon, Sarlandière), and hydropathy. The last-named remedy has been recommended by several eminent French physicians; amongst others, by Recamier and by Valleix, who observes that it is an undeniable fact that patients suffering from obstinate gastralgia have been more or less rapidly cured by the hydropathic treatment. “We must, however (he adds), not overlook the circumstance, that their diet and general mode of life are completely changed, and that their food is substantial, though extremely simple, and that these conditions may have a considerable share in promoting the cure.” Valleix adds that he has frequently checked the acid eructations by prescribing a glass or two of cold water in the morning and evening, and after meals.

In cases of this nature, more usually depends on the management of the diet, exercise, mode of life, &c., of the patient, than on the mere drugs prescribed. The physician who conceives that his duties commence and terminate with the writing of his prescription, has a very erroneous view of the duties of his calling. Narcotics may temporarily relieve pain, and tonics may tend to restore the tone of the stomach; but unless the physician can succeed in obtaining the full confidence of his patient, and in persuading him of the certainty of a speedy cure, the entire armoury of the Pharmacopœia will be of comparatively little service.

Under the title of ‘Neurosis of the Intestine,’ Wunderlich includes “endemic and epidemic colic,” “lead colic,” and “copper colic.” As there is nothing very new in any of our authors on these diseases, we proceed

\* Summa Observ. Med. ex praxi clinica xxx ann. deprimpt. 1829.



to 'Neuralgia of the Anus,' a rare but very painful affection, of which a few cases have been recorded by Campaignac,\* Piorry,† Hirsch,‡ and others. Wunderlich states that he has recently met with a case, occurring in a young countrywoman, who in other respects was perfectly healthy. The pains were extremely severe, and occurred in paroxysms, which sometimes but not always were simultaneous with defecation. A most careful examination showed that there was no tear or fissure in the perinæum or rectum, no cancerous deposition in the neighbouring parts, no uterine disease, and that there were no ascarides. Anatomically, the parts were in a condition of perfect integrity. Belladonna, in the form of ointment, and injections, diminished but did not entirely remove the pain.

Spasm of the œsophagus is the last of the affections included in Wunderlich's first sub-division of the nervous affections of the intestinal canal.

It is, he observes, frequently only a symptom of various diseases of the brain and upper part of the spinal cord—of hysteria, hypochondriasis, epilepsy, tetanus, hydrophobia, meningitis of the brain and spinal cord, or of the development of tumours at the base of the brain. Wunderlich once saw maniacal delirium alternating with spasm of the œsophagus in a powerful-looking man. The spasm sometimes appears to be induced by vivid psychical impressions—by violent passion, by terror, &c. Moreover, it appears to be often associated with diseases of the stomach, especially with cancer. Whether it ever occurs alone, that is to say, independently of any other morbid affection, is very doubtful.

There is nothing in the sections on the symptoms or treatment of this affection requiring notice, except that Wunderlich suggests that in serious cases of spasm the inhalation of ether or chloroform might prove serviceable.

The second sub-division of the nervous affections of the intestinal canal, namely the *Paralyses*, may be passed over with scarcely a remark. Although paralysis occurs with considerable frequency along the whole digestive tract, it is not so much an essential and independent disease, as a secondary affection, depending on some other form of disease, and generally one of its final consequences, or merely a partial manifestation of more general paralysis, dependent on the nervous centres.

We now arrive at the second, and by far the most important of the divisions adopted by Wunderlich; it includes *all the diseases of the intestinal canal, that are accompanied by corresponding anatomical changes*. It is subdivided in the following manner:—

- A. *Deviations of position and form.*
- B. *Spontaneous disturbances of connection, unnatural openings, and communications.*
- C. *Anæmia of the intestinal canal.*
- D. *Hyperæmia, hemorrhage, quantitative disturbances of nutrition and secretion, catarrh, and inflammation of the intestinal canal, subdivided into—*
  - a. *Affections limited to an individual part of the intestinal canal.*
  - β. *Affections simultaneously developed in various parts of the intestinal canal.*

\* Journ. Hebdom., II, 405.

† Mém. sur la Nature et le Traitement de Plusieurs Nevroses, obs. v.

‡ Spinalneurosen, p. 367.

- E. *Typhus.*
- F. *Tuberculosis of the intestinal canal.*
- G. *Cancer of the intestinal canal.*
- H. *Syphilitic affections of the intestinal canal.*
- I. *Processes connected with mortification—*

- a. *Ulcers.*
- β. *Softening.*
- γ. *Gangrene.*

K. *Abnormal contents in the intestinal canal—*

- a. *Gas.*
- β. *Foreign bodies, gall-stones, intestinal concretions.*
- γ. *Parasites.*

The subject of 'Deviations of Position' belongs rather to pathological anatomy than to practical medicine. We pass on, then, at once to 'Deviations of Form.' The only practically important deviations of this nature are those in which there is abnormal *dilatation* or *contraction*.

Abnormal *dilatation of the œsophagus* may occur as a uniform cylindrical enlargement of the whole canal, or it may be partial and saccular; or, finally, there may be several dilatations. Its causes are very obscure; it may arise from chronic inflammation and catarrh of the mucous membrane, and, according to Rokitsansky, from blows or concussions. An abnormal contraction almost always gives rise to a partial dilatation above it.

The symptoms are more or less dysphagia, occasionally rumination, and a fœtid odour from the mouth; in dilatation of the thoracic portion there are pains of the chest and heart. The patient usually sinks with all the symptoms of marasmus. A very singular case was recorded by the late Dr. Hannay, of Glasgow, in which sudden death from hyperæmia of the lungs was induced, apparently by the pressure of a dilated œsophagus, filled with food, on the pulmonary vessels.

*Dilatation of the stomach* rarely exists, except in association with, and as dependent on, contraction or partial occlusion of the pylorus from cancerous deposit or hypertrophy of the walls. Independently of these organic causes, we may mention considerable thinning (if we may be allowed to use the word) of the walls of the stomach (a frequent result of chronic gastritis), atrophy of its muscular fibres, paralysis of the stomach, (a condition assumed rather than known, and compared to paralysis of the bladder,) the habitual overloading of the organ with solids, and especially with fluids, and possibly the enormous accumulation of liquid secretions and gas in the stomach.

The symptoms are usually commixed with the symptoms of the primary affection (the contraction or partial occlusion of the pylorus, chronic gastritis, &c.), which has given rise to the dilatation. As far, however, as they can be separated from the latter, they are:—

A feeling of weight in the epigastric region, especially after a meal, or after the ingestion of a considerable quantity of fluid. This is a symptom of constant occurrence, and one of which patients usually first complain. A dull pain in that region is also frequently noticed. Vomiting invariably occurs, but it differs considerably in its character during the different

stages of the disease. In the early stage, while the stomach can still readily contract, it is frequent; in the latter stages, the stomach seems unable to expel its contents until the accumulated food of two or three days is collected in it. The vomited matters must be carefully examined. In mere dilatation they consist of partially digested food, drink, and mucus; when cancer is also present, they contain grumous matter and blood. The form of the abdomen gradually changes as the disease progresses. At a comparatively early stage we observe a decided arching forwards between the lower extremity of the ensiform cartilage and the umbilicus; soon afterwards, the abdominal walls project so strongly forwards, that ascites or pregnancy may be suspected; in some cases the outline of the great curvature may be clearly perceived through the abdominal walls, and the pulsations of the coronary artery may be clearly felt; finally, in very extreme cases, the stomach lies like an enormous bag, in quite the lower part of the abdomen. The extent and amount of the ventral protuberance are found to vary greatly, if we examine the patient both before and after he has vomited. By a careful manual examination, made when the stomach is distended, we may usually succeed in detecting that organ, as a slightly resistant and somewhat indefinite tumour, extending from the left hypochondrium towards the spine of the ilium of the same side, and from thence towards the right hypochondrium. When the stomach is partly filled with fluid, fluctuation may be detected. Percussion often yields us great assistance in determining the size and position of the stomach; and in this mode of investigation it is of great importance that the patient should be examined in different positions. If he is made to lie on his back, there is a large extent of surface yielding a clear sound, because the gas collects under the anterior wall of the abdomen; if he lies on the left side, the clear sound is chiefly to be heard in the right hypochondrium and right side generally, and the line of level of the fluid will be found running obliquely from above downwards, and from right to left; if he lies on the right side, the opposite result is of course obtained.

There is one more symptom—a subjective one—which deserves notice. Patients suffering from dilatation of the stomach, not unfrequently complain that, on taking fluids, especially very cold drinks, they feel them descend deeply, as if they were sinking into the pelvis.

With respect to the general condition of the system in cases of uncomplicated dilatation of the stomach, it may be observed that the tongue presents no constant peculiarities, that the appetite is usually increased, sometimes to an enormous degree, and that there is generally obstinate constipation, relieved, however, in some cases, by occasional copious dejections.

The anatomical lesions are very simple. The two orifices of the stomach lie close to one another, and the viscus hangs suspended like a bag from its two extremities. The small curvature often lies below the level of the umbilicus, while the great curvature occupies the iliac fossæ. The epiploon is frequently almost entirely effaced, and the compressed intestines are hidden by the enormous pouch of the stomach, whose walls are usually attenuated and transparent, and whose muscular fibres are less close than usual, and sometimes destroyed in the vicinity of the pylorus. In some cases, however, the walls are thickened. The cavity of the stomach usually contains a considerable quantity of partially digested food, liquids that have been

taken as drink, mucus, &c. Wunderlich refers to one case in which the stomach, after death, was found to contain *ninety* pints of fluid.

The treatment that has been found most successful in these cases is somewhat prolonged very rigid diet, followed by a life of extreme moderation, and a restricted diet of nutritious but not bulky food. M. Pézerat,\* was consulted by a woman thirty years of age, whose belly was of the same size as if she were completing her period of pregnancy. He readily ascertained that this was a case of dilatation of the stomach; and from the woman's own account he learned, that she daily ate enough food for two or three workmen, and that she would take more if she could get it. She was kept for a fortnight on a "demi-portion" daily, corresponding to the "low diet" of our English hospitals, and by that time the stomach had quite regained its original state,—at least so says M. Pézerat. Afterwards, as long as she continued to live abstemiously, she had no return of her complaint; but whenever she indulged her appetite too freely, there was a return of the original symptoms. The same author gives another somewhat similar case of cure.

Excitant and tonic medicines, aromatic and bitter infusions and tinctures, cinchona and iron, have been recommended in these cases to restore the tone of the stomach. Gentle aperients may be occasionally given; and when there is a great accumulation in the stomach, an emetic is decidedly serviceable. When there is great flaccidity of the belly, an elastic bandage often gives great relief.

We proceed to the consideration of abnormal contraction of the intestines. These may depend on various causes:

"1. Sometimes they are congenital.

"2. They are very often induced by a morbid process going on in the walls of the intestine, sometimes by simple infiltration with shrivelling or contraction (as in the œsophagus, pylorus, and rectum); most frequently by cancerous infiltration (especially in the œsophagus, cardia, pylorus, and rectum, and more rarely in the sigmoid flexure and in the colon); not unfrequently by excrescences of a cancerous or syphilitic nature (especially in the rectum); by tumours (as, for instance, hæmorrhoidal tumours and polypi); and by shrivelled cicatrices occurring in tuberculous, dysenteric, cancerous, or venereal ulcers.

"3. By a prolonged emptiness of the canal; from this condition we often have contraction of the stomach and colon.

"4. By foreign bodies which have entered the canal and got fast at any spot.

"5. By obstruction, twisting, invagination, &c. of the intestine, by which a certain portion usually becomes contracted or occluded.

"6. By pressure from without, which may be caused by tumours, encysted abscesses, aneurisms, the displacement of solid organs, or even by another portion of intestine." (Wunderlich, p. 865.)

In many cases, dilatations or contractions of certain portions of the intestinal canal may exist, without giving rise to any inconvenience. The injurious effects resulting from alterations in the form and position of the intestine, depend on two conditions, namely, on the difficulty in the passage of the fæces, and on the derangement of the capillary circulation in the intestine; and if either is once fairly established, the other follows as a natural consequence. Although in severe cases these two conditions are thus always present, yet their separate consequences may be distinctly laid down. The impediment to the free passage of the fæces induces emptiness

\* Journ. Complém., t. xxv, p. 162, &c.

and contraction of the lower portion of the intestine, distension of the intestine at the point of the obstruction and superiorly, and a reversed motion of its contents; whilst an impediment to the free capillary circulation in the intestines (or hyperæmia), if it continues for any time, gives rise, in very chronic cases, to sanguineo-serous, or, where the hyperæmia is very intense, to plastic infiltration of the coats of the intestines, rendering them soft and friable, to plastic exudations on the surface of the peritoneum, to paralysis of the muscular fibres, to purulent exudations and ulcers on the mucous membrane, and, finally, to softening and gangrene of the intestines, and to perforation.

Wunderlich arranges the disturbances arising from deviations of position and form, under three heads, according to their degree of severity and acuteness. The severer forms may either be developed from the slighter, or arise suddenly; while the slighter ones may either be gradually produced, or may be the *sequelæ* of more severe ones.

The slightest degree of disturbance consists in a difficult and prolonged progress of the contents of the intestine (according to the position of the impediment we have dysphagia, protracted gastric digestion, or habitual constipation); in the upward discharge of its contents, when the impediment is not far down, as shown in rumination and vomiting; and, further, in catarrh of the affected part, often extending along the whole intestine, and giving rise to considerable pain, which is dependent on slight infiltration of the tissue and peritoneal exudation. This form of disturbance may continue for years, with occasional exacerbations and remissions, and is commonly known as chronic colic, flatulence, hæmorrhoidal pain, &c., according to its supposed cause. It not unfrequently gives rise to hypochondriasis and other forms of mental derangement.

In the medium degree, there is a more severe exacerbation, induced, doubtless, by a more decided stasis of the blood, and by more abundant local exudations. If the seat of the disorder be in the œsophagus, we have a more decided dysphagia than in the first form; if in the stomach, a more severe catarrh, or gastritis with considerable vomiting; if in the abdomen, more severe colic pains; and if in the rectum, a more severe hæmorrhoidal attack. There may be dyspnœa, spasm of the diaphragm, or icterus, according to the position of the diseased spot; and there is almost always more or less constitutional derangement, as rigor and febrile disturbance. By a careful manual examination, and by percussion, we can often succeed in determining the affected spot; the nature of the morbid condition is, however, much more difficult to diagnose. Although these exacerbations may gradually disappear without doing any permanent damage, they most commonly give rise to more intense *stases* and exudations, and to peritoneal adhesions retaining the intestine in its abnormal position.

In the highest degree, the disturbance is more generally distributed than in the two preceding forms, although it is most severe at the point at which the morbid action commenced. It shows itself in the form of intense gastritis, of enteritis, and of peritonitis, with obstinate constipation, usually with vomiting, and often with ileus, and it runs a very acute and usually fatal course. It generally comes on suddenly and with great severity, although it is sometimes developed from the less intense forms, if they have not yielded to treatment, or if the treatment has been injudicious. In this highest stage of disturbance, it is only rarely that we can overcome

the impediment, allow a free passage to the intestinal contents, and allay the hyperæmia and inflammation that have been excited. Most commonly, death results from the intensity and extension of the inflammation giving rise either to paralysis and mortification, or to perforation.

As the treatment of these acute exacerbations is precisely the same as that of enteritis, we shall pass on to Wunderlich's second division on "Spontaneous disturbances of connection, and unnatural openings and communications."

*Ruptures*, independently of external violence, occasionally occur in the œsophagus and stomach, but so very rarely, that we shall at once proceed to the consideration of *perforations*.

Although individual instances of spontaneous perforation of the intestinal canal have been recorded from a comparatively early period, it is only during the last ten or fifteen years that the pathology of this class of affections has been correctly understood. In this department of medicine the Germans and French have been more successful workers than ourselves. The investigations of Rokitsky\* and Cruveilhier† on the "perforating" or "simple chronic ulcer of the stomach," and the memoirs of Louis,‡ Corbin,§ Forget,|| and Toulmouche,¶ on intestinal perforations generally, may be mentioned as especially deserving of the attention of pathologists.

Although perforations may occur at any part of the intestinal canal, they are more frequent at some spots than at others. The parts into which they most commonly occur, are the small intestines, the stomach, and the appendix vermiformis. The causes, moreover, which induce their occurrence at different parts, are in some respects different.

Wunderlich observes that, independently of traumatic perforations, there are two ways by which perforation may take place.

1. The ordinary form of perforation commences at the inner membrane, and penetrates the intestine from within outwards. In this first form we place :

*a.* Perforations from intestinal worms. These cases are very rare.

*b.* Perforations by simple, acute inflammation, terminating in disintegration of the tissue. Perforation of this nature occurs when foreign bodies become entangled in the intestine, occasionally from mere impaction of fæces, and in cases of stricture and incarceration. Diffuse inflammation sometimes precedes perforation, and in that case we have the ordinary symptoms of inflammation of the intestines; sometimes, on the other hand, the inflammation is so limited in its extent, that no general or local disturbance precedes the sudden and unexpected occurrence of the perforation. The appendix vermiformis is the most frequent seat of these sudden perforations.

*c.* Perforations by dysenteric affections. These occur chiefly in the colon.

*d.* Perforations by typhous infiltration and ulceration. These occur chiefly in the lower part of the ileum.

*e.* Perforations by tuberculous infiltration and ulceration.

\* Oesterr. Jahrb., 1830, xviii, p. 184; translated in the Arch. Gén. de Méd., Juin, 1840.

† Mémoire sur l'Ulc. simple chron. de l'Estomac (Revue Méd., Fevrier et Mars, 1838). See also his Anatomie Pathol. du Corps Humain.

‡ Arch. Gén., vol. i, p. 17.

§ Gaz. Méd., vol. v, p. 226.

¶ Arch. Gén., vol. xxv, p. 36.

¶ Gaz. Méd., vol. xiii, pp. 646, 661, 741.

*f.* Perforation by the disintegration of cancerous deposits, occurring chiefly on the stomach and rectum.

*g.* Perforation by simple chronic ulcer; most common in the stomach and duodenum.

*h.* Perforation by softening, occurring for the most part on the stomach, but sometimes in the small intestine and the œsophagus.

*i.* Perforation by gangrene, especially in cases of strangulation. This, however, according to Wunderlich, is of rarer occurrence than is generally supposed.

2. It is comparatively rarely (although, as Wunderlich somewhat quaintly observes, *quite often enough*) that the intestinal canal is perforated in the opposite direction, by morbid processes in adjacent parts extending to it and causing a destruction of its component membranes. Abscesses and hydatid tumours in the liver, and abscesses in the iliac fossa, sometimes burst in this way into the intestine; more rarely we meet with cases of pleuritic exudations opening into the intestinal tube after having perforated the diaphragm, and of aneurisms opening into the œsophagus and stomach, and in some very few cases, into other parts of the intestinal canal. Acute and chronic peritonitis with exudation, especially when very much concentrated at one spot, may give rise to perforation there; and septic inflammation of the vagina, and cancer of the female generative organs and of the bladder, may finally produce perforation of the rectum.

Wunderlich states, that he has met with three cases in which perforation arose from abundant peritoneal exudation, and therefore from without inwards, without any primary disease of the intestine at the spot, and without its being glued to any adjacent diseased organ, but simply by the disintegrating action of the peritoneal exudation.

We proceed to the pathological anatomy and physiology of intestinal perforations generally. When perforations arise from suppurative disintegration, from softening, or gangrene, or when they occur from disease external to the intestine, they ensue rapidly and unexpectedly; when, on the other hand, they arise from the gradual destruction of the different layers of the intestine, as in simple chronic ulcer, in cancer, and in tubercle, their progress is slow.

In reference to the number of perforations, we most commonly meet with only one, but sometimes with more. It is in cases of typhous, tuberculous, dysenteric, and gangrenous perforations, that more than one usually occur.

The most common result of the perforations of the first class, is the escape of a portion of the contents of the intestine into the peritoneum. The quantity that thus escapes must depend on the size of the opening; if it is small, and only a little matter escapes, the peritoneal inflammation that is excited may be moderate, and, by adhesion with an adjacent organ, the abnormal opening may be closed. In this way the immediate dangers of perforation are escaped, and at worst there is only a communication established between the intestine and some adjacent hollow organ, as the vagina or bladder. If, however, there is any great amount of motion in the part of the intestine which has been perforated, the recent and delicate adhesions are torn asunder, the contents of the intestine again escape, and fresh and more severe peritonitis is set up. Moreover, it may happen that, by the prolonged continuance of peritonitis, the plastic matter with

which the opening was closed may be again rendered fluid, and the communication between the peritoneal cavity and the intestine again established. If, however, either when the perforation first occurs, or subsequently, any considerable quantity of matter escapes into the peritoneum, the gluing up and closure of the opening is no longer possible. It sometimes happens, in cases of considerable severity, that there is formed in the peritoneum a sacculated cavern, containing pus, air, and intestinal contents, and that from this cavern further perforations extend, either into one of the abdominal or thoracic organs, or to the surface of the body. In the latter case, emphysema, and diffuse inflammation, and destruction of the sub-cutaneous areolar tissue, precede the perforation of the cutis. These cases of circumscribed peritonitis are, however, unfortunately very rare, as compared with the cases of rapid and diffused peritonitis, which prove fatal without there being sufficient time for the formation of secondary perforations into other organs, or externally.

The escape of the contents of the intestine into the abdominal cavity may, however, be prevented, especially in somewhat chronic cases, by the formation of solid plastic exudations gluing an adjacent organ (the liver, pancreas, omentum, intestine, bladder, &c.) to the spot, before the serous intestinal coat has been ruptured. Acute perforations are not only more seldom closed up in this manner than chronic perforations, but there is a greater probability of their re-opening by the destruction or injury of the adhesive exudation, which is softer and less resistant in the former than in the latter cases.

When the perforation takes place from without inwards, and large masses of blood, pus, or other fluid thus enter the intestine, the intestinal contents do not usually escape, or if they do, the circumstance is of little importance. In this case, there is either a persistent abnormal communication, or death rapidly supervenes.

The form of the opening, and the character of the edges, vary according to the mode in which the perforation has been effected. Generally all small perforations are roundish, and the edges are very thin when there has been great softening of the natural tissues preceding the perforation, and comparatively thick when there has been pre-existing infiltration of the intestinal coats.

Wunderlich gives the following as the different modes in which an intestinal perforation may *possibly* terminate:

1. Simple closure, without adhesion to an adjacent part. This is doubtless a rare termination.

2. A glueing up, with stoppage of the opening, by an adjacent part. A common termination in chronic perforation, but rare in acute cases. The immediate peril is thus over; but there is risk of the adhesions giving way, in consequence of the motion of the intestines. The most favorable case is when the gut becomes detached from the adhering organ while the opening remains cicatrized.

3. The establishment of an abnormal communication with other viscera, or between two portions of intestine, or between the intestinal canal and the surface of the body. Although the immediate peril is removed, death usually ensues, sooner or later, from the effects of the fistulous opening which is formed. In some cases, however, these abnormal communications heal spontaneously, or may be removed by surgical means.



4. The formation of a saccular peritoneal pouch, with which the intestine communicates by the opening.

5. General peritonitis, which is almost always fatal.

6. Diffuse ichorous inflammation of the neighbouring areolar tissue, always terminating fatally with the symptoms of adynamic or hectic fever.

7. Death by copious loss of blood on the opening of an aneurism or of a large vein into the œsophagus or intestinal tube.

Notwithstanding the various ways already enumerated, in which cases of intestinal perforation may prove fatal, it is well known to all who have had an enlarged pathological experience, that cases not very unfrequently occur in which the appearances on dissection are not sufficient to account for the proximate cause of death. The patient seems to die before there is time for such changes as are commonly regarded to be incompatible with life, to have developed themselves. Thus, in acute cases, the peritonitis (pathologically speaking) is comparatively slight when death supervenes, and there is nothing found in the other organs to account for this rapid dissolution. Wunderlich thinks it possible that paralysis of the intestine may sometimes contribute to the fatal result.

Before proceeding to the consideration of the symptoms of intestinal perforation, we may remark, that the cases are not very rare in which perforation seems to give rise to no symptoms whatever. In acute cases this only occurs when the perforation takes place during the very death-struggle from some other disease. Wunderlich mentions two cases of this nature. He found a recent perforation of the small intestine in a puerperal patient suffering from peritonitis and pneumonia, who, two days before her death, had an apoplectic seizure, and was thrown into a state of deep sopor. The other case was that of a tuberculous patient, who, during the last few days of his life, was attacked with typhus. In neither of these instances was there any suspicion of perforation, until its actual presence was revealed by dissection.

In other cases of acute perforation the symptoms are indistinct and obscure, being confined to subacute pain in the abdomen, to more or less severe rigors, a general appearance of distress, collapse, and coldness of the extremities. From the presence of some or all of these symptoms, we may suspect, although we cannot with certainty diagnose, perforation. It is in cases of perforation from gangrene, softening, typhous ulceration, and when the contents of the intestine do not freely escape into the peritoneal cavity, that we have these masked symptoms.

When the perforation occurs between the intestine and another viscus, or conversely, or between two portions of intestine, the immediate symptoms are very obscure, and it is commonly only by its secondary consequences that we are ever able to diagnose it; and sometimes, indeed, we fail totally in determining the true nature of the affection. The perforation is sometimes indicated by a sudden diminution or suspension of the pre-existing pain, by a peculiar change in the countenance, by smallness of the pulse, and coldness of the extremities, while the rest of the body is bathed with a cold and clammy sweat. These symptoms are especially observed in the perforation of incarcerated loops of gut, and in perforation from gangrene.

In most cases, however, in which the contents of the intestine escape freely into the peritoneal cavity, the symptoms are sufficiently obvious.

The exact moment at which the perforation occurs cannot always be determined; but the patient feels a severe, and more or less sudden pain, at a limited spot in the abdomen, which is extremely sensitive to pressure. The pain sometimes becomes more diffused. Nausea, vomiting, and rigors ensue, and there is great distress depicted on the countenance; the pulse is small, and often irregular and very frequent; there is constant shivering, and a cold, clammy sweat. The pain sometimes continues till the death of the patient; it more frequently, however, moderates, and in some cases of even free escape of the intestinal contents into the peritoneum, altogether disappears. When the patient has been suffering from a previous affection, the character of the disease is often suddenly changed. Typhous sopor and apathy are replaced by great excitement and intense anguish; perfect consciousness supplants delirium; and the countenance loses its torpid, inexpressive appearance, and assumes an expression indicative of great suffering. Death seldom occurs in less than twelve hours after perforation. The patient may suffer acute pain, and have an agonizing feeling of impending suffocation till the last, or when he has become much weakened, a state of quiet drowsiness may supervene; in either case, the abdomen becomes tympanitic. It is very seldom that, in cases of free effusion of the intestinal contents into the cavity of the peritoneum, the patient survives beyond three or four days.

When the perforation makes its way towards the general integuments, we observe a red, painful, doughy tumour in the abdomen; this tumour sometimes has a well-defined margin, and on percussion yields a tympanitic sound. Without this last symptom, we may readily be deceived regarding the nature of the tumour; and even when the tympanitic sound is present, it must be recollected that it may depend on the presence of a loop of intestine. The skin often assumes an erysipelatous redness, and there is considerable subcutaneous infiltration. By pressure we may then detect an emphysematous crackling, which renders our diagnosis certain. The patient usually sinks before the skin is perforated.

Wunderlich truly remarks, that in many cases of perforation therapeutic aid is unavailable, either from the rapidity with which death ensues, or from the symptoms not being sufficiently distinct to enable us to form a correct diagnosis. He lays down three conditions, in which medical aid may be of service.

1. In cases where there is reason for believing that there is impending danger of perforation, as in cases of simple ulcer of the stomach, of softening of the stomach, or of inflammation in the right iliac region (of the appendix vermiformis), in relapses after typhus, in hernias and other displacements of the intestine, accompanied with severe inflammatory symptoms, in septic inflammation and cancer of the female generative organs, in cancer of the rectum, and in all cases where there is an excessive distension of any particular part of the intestine. In all these cases, preservative measures must be especially regarded. If the *causal process* of the disease admit of actual treatment, it is essential that such treatment should be adopted. Next in order of importance, or where the conditions of the case render the first indication impossible, the three following points must be attended to; namely, the obtaining of the greatest possible rest for the patient generally, and more especially for his intestines; the limiting, as much as possible, of the local hyperæmia and inflammation;

and the removal, as far as is possible, of faecal masses, foreign bodies, accumulated secretions and exudations, &c., from the parts affected.

Practically there are many difficulties in carrying out these three indications; for instance, if we give a laxative to effect our third object, we increase the movement of the bowels in opposition to our first indication; or if we give opium to quiet the motion of the bowels, we are almost sure to induce constipation, and thus act in opposition to our last indication. It is difficult to say whether too active treatment or irresolute delay is the more dangerous to the patient. There are few cases in which the cautious skill and at the same time the firm decision of the physician can be more fully exemplified, than in those we are now considering.

2. The second condition in which therapeutic treatment is still available, is when decided symptoms have rendered it obvious that the contents of the intestine have escaped into the peritoneum. These are most perilous cases; but still the patient may often be saved. Laxatives, even enemata, are now decidedly prejudicial; the patient must be kept in a state of the greatest possible quiet, and his food, drink, and medicines must be reduced to the smallest possible bulk. Opium is our sheet-anchor in these cases, and it must be given with an unsparing hand; one or two grains every hour, or second hour. In this point British,\* French,† and German‡ physicians seem unanimous. If the pain continues very intense, a large number of leeches (30—40) may be applied, and the abdomen afterwards covered with a warm, light cataplasma.

3. The third and last case in which therapeutical treatment is available, is when the intestine communicates with a sacculated cavern in the peritoneum, or the perforation in some other way is tending to the surface of the body. We must here endeavour to check as much as possible the extensive destruction of the subcutaneous areolar tissue, by the application of cataplasms to the tumour, and by making an early opening when air can be detected under the skin. The patient must be kept on the smallest possible quantity of food, which must be easily digestible without leaving bulky faeces.

The length to which our remarks on perforations have extended, compels us to exclude, from the present article, the consideration of several affections which we had intended to notice.

Of fistulous communications between the different parts of the intestine with one another, or with other organs, we shall say nothing, as they pertain rather to pathology and surgery, than to practical medicine.

Neither shall we detain our readers with any remarks on Wunderlich's third class of anatomical disturbances, the anæmic conditions of the intestinal canal, because we know very little about them, and our knowledge has not been materially extended by the perusal of all that our authors have written on the subject.

In our next article we shall commence with the inflammatory and hemorrhagic diseases of the intestinal tube, and trust to be able to include all that we have to say on "the disorders of the digestive system."

\* Graves and Stokes, in vol. v, p. 126 of the *Dublin Hospital Reports*; Griffin, in vol. v, p. 372, of the *Dublin Journal*, &c.

† Petrequin, in *Gaz. Méd.*, tom. v., p. 436; Caseneuve, in *Gaz. Méd.*, tom. vi, p. 3; Riche, in *Revue Méd.*, 1843, vol. III, p. 536.

‡ See especially Volz, *Die Durchbohrung des Wurmfortsatzes*, &c., 1846.

## ART. XIV.

*Physician and Patient; or a Practical View of the Mutual Duties, Relations, and Interests of the Medical Profession and the Community.*

From the Text of WM. HOOKER, M.D. Edited by EDWARD BENTLEY, M.D., &c. &c.—London, 1850. 12mo, pp. 303.

THIS work consists of fourteen chapters, in each of which some topic of medical etiquette or ethics is considered. The uncertainty of medicine; medical skill; popular errors; quackery; good and bad practice; theory and observation; popular estimates of practitioners; the rules regulating the intercourse of physicians, which the community ought to understand; interference with physicians; influence of mind and body, and especially of hope, in the development and treatment of disease; the necessity of adherence to truth in professional intercourse with the sick; the moral influence of the profession; and the trials and pleasures of medical life, are all passed successively under consideration. The method of treating these various subjects is judicious; the sentiments are excellent; and the language concise, and often elegant. Dr. Bentley has embodied the American code of medical ethics with this work, and has altered his author wherever he thought it advisable. He has also omitted chapters on 'Insanity;' on 'The Means of Removing Quackery;' and on 'Thompsonism,' 'Homœopathy;' and 'Natural Bone-setting.' It would have been better if Dr. Bentley had added some details respecting the original work and its author; for we observe that not even the date of its composition or publication is given, nor is it so much as stated whether the work has been already published, or not. We are also left in ignorance of how much belongs to the original author, and how much to his editor; and that Dr. Hooker is, in fact, an American writer.

As we have already, and so recently, discussed various points in Medical Ethics, we cannot enter into that lengthened review of the work, which its importance demands. One or two points, however, may be briefly reverted to, inasmuch as they have had more than usual attention directed to them lately, and principally, indeed, because they have been mooted in our own pages. The question of Quackery, and the best mode of suppressing it, or of checking its ravages, is one of these. In reference to this, Dr. Hooker first devotes a chapter to popular errors, judging rightly that these constitute the true, or at least, most important source of quackery. A great and very common error of this kind—an error, indeed, not peculiar to the community at large, is "a false estimate of the importance of positive medication." Dr. Hooker adduces, as an example, the prevalent notion respecting the healing of wounds. He observes on this, amongst other instances:

"The cure is usually attributed to some healing property in the applications made to the wound. But the union of the divided parts is effected entirely by a natural process, and the only use of any application is to bring the lips of the wound in apposition, so that the process may be effectual in securing union." (p. 35.)

So, again, any changes that may occur during the course of treatment of a case, whether favorable or unfavorable, are attributed almost as a matter of course to the remedy that was administered immediately previous to the change; and when recovery takes place, friends are very apt to

inquire what it was that cured him; as if there were some one remedy that effected a cure. Consequently, there is always a disposition to demand of the practitioner, that he shall *do something* to overcome the disease; and the friends of patients deceased, often remark, as a matter for consolation, that no means of relief which was suggested was left untried; "not seeming to dream," as Dr. Hooker observes, "that it was possible to do too much." These and other instances of illusive experience and imperfect observation exemplify the nature of the various popular errors, from which quackery springs.

In showing how the homœopathic quackery has gained a hold on the community, Dr. Hooker observes, that—

"1st. This system of practice is especially calculated to produce a great effect upon the public mind. The very idea that there is a peculiar power imparted to the little globules by their preparation, acts upon the imagination of the patient. It gratifies the love of mystery, so common and so ready to respond to the appeals which are made to it. The minute examination of symptoms of which such display is made by the homœopathic physicians, adds to this influence upon the mind, by its imposing air of deep and patient research. 2d. A strict regard to diet and regimen. 3d. The influence of the curative power of nature, the efforts of which are not interfered with by homœopathy. This is the chief cause of all the cures which homœopathy claims to itself, as the undoubted results of its infinitesimal doses. The two influences first named, prepare the system for the operation of this curative power. 4th. A comparison between the results of homœopathic practice, and those of the practice of overdosing physicians. Such a comparison will generally tell in favour of homœopathy, because the plan of giving no medicine, and relying upon a favorable mental influence, and a strict regulation of diet and regimen, is much better than overdosing. 5th. The facility with which people are imposed upon in their attempts to estimate the comparative merits of modes of practice by their results, is another source of the popularity of homœopathy." (pp. 46-47.)

Dr. Hooker might have added a 6th, namely, the secret use of ordinary remedies in their ordinary doses. It is now pretty well ascertained by chemical analysis of certain pretended homœopathic medicines, that they are at least occasionally administered in full doses. The facility with which a fraud of this kind may be practised is obvious enough, and raises grave doubts as to the verity of homœopathic facts.

Dr. Hooker then notices the means by which the tendency to empiricism, so rife in the community, may be corrected; observing that the efforts made by the profession to this end have been made with wrong means, and in a wrong direction. Popular lectures and treatises on anatomy, physiology, dietetics, &c., have been of much less service, Dr. Hooker maintains, than we are apt to think. On the contrary, a popular knowledge of these subjects sometimes evidently increases this tendency to empiricism, by giving its possessor an excellent idea of his medical acumen. Such an one is much disposed to adopt opinions and theories on slight or plausible grounds, and is thus constantly led into error. The chief means which he thinks available to the removal of quackery, is to be found in an exposure of the fallacy of the conclusions already adverted to, as those on which quackery is based. Dr. Hooker's remarks on this point are so much in accordance with our own, and withal so practical, that we subjoin them at length. The italics are our own.

"It may be said, that this has often been attempted, and with so little success, that there is little encouragement for repeating such attempts; and that it is best to

let the community find out their errors by their own experience, such as it sometimes is. Those who take this ground assume that the efforts which have been made for this object have been of a proper character. Has this been so? Or has there not been too much of ridicule and sarcasm? These are means which are appropriate, to a certain extent, as auxiliaries to sober argument, but never should be relied upon, as the only or chief instruments in combating error. *There has been too much calling of hard names.* On the part of physicians there has not been enough of calm, candid, and patient discussion with the well-informed. To the medical man quackery appears so nonsensical, that he has no patience with those who embrace it. He does not remember that many of his own profession have, in their reasoning about cause and effect, committed some of the very same errors which have engendered that quackery. Perhaps, if he looks back upon his own course, he may find that he himself has, at some time, fallen into an error, which might have led him into empiricism, if he had been out of the profession. . . . *He should, therefore, avoid being betrayed, by the absurdities of quackery, into the utterance of hard expressions, or a too free use of sarcasm.* On the contrary, *he should endeavour to show any intelligent friend who has chanced, in some way, to be deluded by empiricism, that he has been deceived,* and point out to him just how it has been done. He should show to him what the mistakes are, which he has made in relation to the connection between cause and effect; and endeavour to impress upon his mind the truth that there is more necessity for cautious discrimination, in forming conclusions on this, than on any other subject in the wide range of science; and how common a thing it is in medicine to attribute results to causes, which have had no agency in producing them." (p. 52.)

We are satisfied, with Dr. Hooker, that the class of persons to whom he alludes, "men of strong sense and good judgment," are not to be turned from empiricism by "a mere occasional effort,—some address, some short article in a public journal, some sting of biting sarcasm, or some sally of wit;"—remedies of a more searching character, Dr. Bentley observes, are required in their case, and a treatment more thorough, patient, and persevering. We also cordially agree with him as to the desirableness of medical men exercising their individual influence; because the class of persons to whom allusion is made, and who may be successfully reached by it, are the chief pillars of empiricism.

"It is a curious fact, but one worthy of remark, that those who are most shrewd and judicious in all worldly matters, and who entertain a high opinion of education, often put the quack of a day on a level with the accredited physician, laden with the carefully-gathered experience of years, or, perhaps, even above him; and welcome with open arms the advocate of some new system, for the moment in high favour, with scarcely any regard to the inquiry, whether he has been educated in a proper manner for the responsible post into which he has thrust himself." (p. 55.)

There is one circumstance of importance connected with this question, which Dr. Hooker has failed to notice; namely, that the unskilful method of warring with quackery, which he denounces, has not only proved to be totally inoperative to the desired end, but has inflicted serious injury on the profession. The true-hearted practitioner cannot reflect upon some of the late proceedings in our courts of law, without deep regret that its dignity should have been so slighted and its honour insulted. One eminent physician, not many weeks ago, when asked his *opinion* only, by counsel, in the case of Robert Page, was sharply reprov'd by the presiding judge (because, according to his oath, he answered the question so put), with the insulting remark, that he (the judge) would not allow medical men to take upon themselves the functions of judge and jury. During the late circuits of the judges, a singularly lenient construction of the law, as bearing on

fatal quackery, seems to have been adopted by the bench; while one member of the bar, at least, has not spared obloquy on the profession, when defending the quack prisoner on his trial for manslaughter. We need not multiply or specify instances, as they must be fresh in the memories of our readers.

The spirit towards the profession which seems to actuate a portion, at least, of the bench, may be found in the popular press, and its origin traced. We noticed this point previously; and we now find that 'The Nonconformist,' 'Spectator,' 'Examiner,' 'Economist,' and 'Morning Post,' all abusing the profession, are duly marshalled by Dr. Epps in support of homœopathy in his lately published work.\* It appears that a Mr. Pearce was found guilty, by a coroner's inquest, of the manslaughter of his brother, in consequence of treating him homœopathically (by starvation) when attacked by cholera. Mr. H. M. Wakley, the son of the editor of 'The Lancet,' presided as deputy-coroner, and told the jury that the homœopathic treatment was looked upon by the medical profession as a species of quackery and humbug. This assertion was seized upon by the journals mentioned, as an occasion for an attack upon Mr. H. M. Wakley in particular, and upon the medical profession in general; although we are quite at a loss to discover why the fault of the former (a non-medical man) should be visited upon the latter. Certain it is, that the only result of Mr. H. M. Wakley's indiscreet zeal, has been "a heavy blow and great discouragement" to the profession which he took upon himself to represent. He spoke the truth, but in a sadly wrong place. As an illustration of the opinions of the popular press on the matter, we subjoin a quotation from a leading article in the 'Economist' (a very influential London Journal), of Dec. 15th, 1849, headed, "Medical Science, and Coroner's Law."

"The question of science involved concerns our health, as that of law concerns our liberties. Of the causes of such diseases as cholera, medical men are as ignorant nearly as the rest of the world; of the means of cure, and of the operation of remedies, generally, on the human body, their knowledge is little better than conjectural; and whether they be homœopathists or allopathists, it is unbecoming to be dogmatic and positive in their assertions. The best medical man we are acquainted with,—one of the most distinguished surgeons of the day,—is at the same time the most cautious in prescribing, and the least positive in his anticipations. An ordinary apothecary is ready for every emergency, and prescribes for every case that he is summoned to off-hand, as if he were Esculapius himself, knew at a glance the whole history and nature of the disease, and was as certain of the operation of his drugs, as a smith is of the effect of his forge-fire on a piece of iron. Amongst such men, looking out for practice as a means of living, we meet with truculent disputants about the merits of their own and the quackery of others' theories of physic. Less ready to inquire and observe than to gather pelf, they hunt after fees and neglect knowledge. They see no other way to reach eminence and wealth, than to press down a rival. Science gets crushed or distorted between their personal quarrels; and the healing art, founded on ill-understood principles, divested of truth and beauty, neither deserves nor commands the confidence of mankind.

"Considering the complex nature of the animal functions, the merely empirical and conjectural knowledge which the most enlightened physicians have of the expected operations of medicines on any given patient, and considering the importance to all of the preservation of life and health, there is no science which ought to be

\* Homœopathy and its Principles explained. By John Epps, M.D. Published by the English Homœopathic Association. 8vo. 1850.

prosecuted with more care than therapeutics. It is, therefore, offensive to the public, and disgraceful to medical men, to impede investigation by hard names, and rabidly attack individuals, instead of coolly examining the discoveries they allege they have made. We are no advocates of homœopathy; but we can safely say, that allopathy has been, in so many cases, so little successful, that it ought to welcome instruction and assistance, from whatever quarter it may come."

Mr. Pearce was a medical student of four years' standing; and the deceased, who was the subject of the inquest, was his own brother. The 'Economist' puts the special circumstances of the matter in a strong light.

"A gentleman, perfectly innocent in intention, scrupulously careful in his professional practice—even if erroneous—who had to mourn the loss of his brother, and was himself laid up with cholera, that gentleman was, in the name and by the instrumentality of the law, which is made and obeyed only for the common benefit, wrongfully dragged off to goal, kept there for several days," &c., &c.

Justice Maule stopped the trial, after hearing and cross-examining Mr. Harris, the surgeon in previous attendance, who had given up the case to Mr. Pearce, in deference "to his feelings as a brother."

We need hardly say, that sentiments and proceedings of the kind quoted and described, are eminently calculated to place the profession in an odious light before the public, and to excite a spirit of hostility towards it. They emphatically warn us to adopt a different course of conduct in our controversy with quackery. As regards the bar and the bench, we must try to convince them of the doubtful validity, in all cases, of the legal dictum, "that if a person, whether duly qualified or not, acted honestly and *bonâ fide* in the treatment of a disease, and death ensued from any operation he might perform, it would not be manslaughter." As regards the public, we must show ourselves less dogmatic, less technical, and more willing to avail ourselves of every means of cure whatever. The public *will* ask, in spite of our protestations, whether we are so well provided with effective and successful methods of treatment, that we can afford to despise and reject all experience, save that which is strictly professional;—if it be wise to eschew active diaphoresis, because it is the principle of the hydropathic quackery;—or to ignore the advantages of the cold douche and wet sheet, because they have been abused by a set of men who know or can apply no other methods of treatment? In reply to our protest against homœopathic empiricism, the public *will* answer, in spite of us, "profit by that experience, and if diet and regimen alone will cure certain diseases, then cure them by diet and regimen; if you will not, others will." The profession must bear in mind, that laymen have little or no apprehension of professional feelings; and where they have, will condemn them when dogmatically or offensively expressed.

We were not aware when we wrote our article on the Relation of True Medicine to Empirical Systems, to how great an extent the public mind had been stirred by an injudicious abuse of quackery; and how greatly "men of strong sense" had been irritated by the offensive dogmatism of some of those, who profess to represent the sentiments of the profession. The warning we then gave has been amply affirmed by the events of the last few weeks; and we beg here to express a most earnest hope, that the leading men amongst us will exercise their individual influence in the *mode* recommended by Dr. Hooker, to allay this irritation; and while they calmly set forth the follies and dangers of homœopathy, and the other forms of quackery, indignantly repudiate any sanction by the profession



of that low and vulgar method of attack which amounts only to a "rabid" abuse, and degrades the profession alone. Our words may not reach the bench or the bar; but we do demand this from the popular press, that the conduct of individuals shall not be attributed to the large body of the profession. The enlightened general practitioner is not "a truculent disputant," nor does he, as "an ordinary apothecary," "impede investigation by hard names." He indignantly denies the charge, and emphatically disowns the "truculent disputants" of the organs, or so-called organs, of the medical world.

Perhaps we ought here to reply to a series of attacks that have been made upon ourselves in a weekly medical journal, under the pretext of controverting our views on this point. Under ordinary circumstances we should have felt it our duty, and an act of courtesy to a contemporary, to meet them; but as the whole matter springs from commercial rivalry, and as the charges made against us are so utterly unfounded, so absolutely opposed to the facts, and so truly absurd, we feel assured that we should only insult the common sense of our readers by a formal reply. How is it possible indeed, for a journal whose pages are indebted to the pens of some of the most practical and learned physicians and surgeons of the United Kingdom, and which is circulated exclusively within the profession, —how is it possible for that journal seriously to discuss whether or no it has deliberately become the organ of every form of quackery, and of whole classes of men whose sole occupation is to vilify the profession, and fraudulently trifle with the lives and feelings of the people? Our great regret is for the hurt and damage done to the character and dignity of the medical body, by the style of controversy to which we refer. If a "rabid" abuse of quacks and empirics by the organs of the profession inflict a heavy blow and great discouragement on it, how much heavier will that blow be, if those same organs turn in "rabid" abuse upon each other? To the imputations, then, of the vilest motives and most degraded course of conduct, that constitute the attacks upon us to which we refer, we are only at liberty to give an emphatic denial; the rest we leave with our readers.

There are one or two points, however, which it may be well to notice, because our friends have either mistaken our meaning, or courteously expressed a difference of opinion. We observed, in our April Number, that "a practitioner is not justified in abandoning his patient," because an empiric is called in; and that should he apprehend that the proposed empirical treatment will shorten his patient's life, or be otherwise injurious to him, the practitioner is *bound* by ethical considerations to continue his supervision. Now it has been erroneously thought that we intended this supervision to be carried out in co-operation and connection with the empiric. This was, however, in no degree our meaning; such supervision and watchful care should be effected without any intercourse with the empiric (if one be in attendance) whatever, and without giving the slightest sanction to the means used. The objects to be gained should be kept steadily in view, and separated as much as possible from any undignified feelings of professional jealousy. All experience shows, that to leave a patient exclusively in the hands of a quack, is often to abandon him to prolonged suffering, and to certain death. Is the practitioner, who sees such results imminent, justified in abandoning his patient? Or, in other words, must he waive his duties as a man, for his duties as a practitioner?

These questions we put, on the supposition that the two classes of duties are irreconcilable; and we think the answer can only be in the negative. Professional pride or wounded feelings,—wounded by the ingratitude of the patient perhaps, or by his unmannerly stupidity,—may incite us to abandon the offender to his fate; but assuredly such conduct, in the *ordinary* concerns of life, would not approve itself to our consciences. If we saw the individual who had hurt our dignity or wounded our feelings, amidst the jostlings of social intercourse, walking unconsciously on the edge of a precipice, or battling to the death with evils over which we had an easy and direct control, would we neglect the golden rule of christian morals, "Love thine enemy," without compunction, and leave the man to his fate without remorse? Or, to put the question in another point of view, would the practitioner so abandon a near and dear *relation*? And if he would not, is it consistent with Christian Ethics to do less for the stranger yet brother-man, towards whom, in the order of God's providence, the practitioner is entrusted with the holy and beneficent mission of saving life or alleviating anguish?—But, in truth, this godlike watch over the patient walking in darkness amidst the danger of quackery, is in no degree inconsistent with professional dignity; on the contrary, it would add infinitely to the moral grandeur of our body. The "men of strong sense and good judgment" in the community, would appreciate the calm repose of soul which quells the whisperings of offended professional pride and dignity, and rebukes the insolence of the ignorant, or the offensive selfishness of the timid, by a steady unflinching adherence to the great end of professional life—the well-being of man. While the practitioner is diligently watching the progress of a case in the hands of a quack, he may protect life, prevent suffering, expose a cruel deception, detect a thievish fraud, and add to his experience in the natural history and treatment of disease. All this is practicable, without the slightest compromise of professional dignity or etiquette. An interesting instance of this kind, in which the physician protected his patient from the arts of a swindling mesmerist, was related in a recent number of the 'Medical Times;' and we are satisfied that some of the fatal results of empirical treatment lately recorded might have been prevented, if the practitioner had been true to his mission.

Nor, if the practitioner, when following this line of conduct, fail in attaining his principal object, should he be discouraged; as he may still do good service indirectly to humanity. In the case of one of our friends, referred to in our April Number, as "snoring his life away" in the hands of a homœopathic physician,—in which the adoption of the energetic means that had been successful in recovering him from previous attacks of apoplexy, was forbidden by his relatives,—the medical practitioner in attendance did not desert his patient, and never ceased to warn these relatives as to the probable result of their neglect, until there was no longer any hope of saving him. In this case, steps were taken to have a judicial inquiry instituted; and although, in *deference to legal advice*, they were not followed up, the protests of the practitioner and of the profession, and the subsequent inquiries, were not without their beneficial influence on public opinion. So that, although the effort to protect the patient failed, a check was given to future malapraxis. And in this and other ways, if we cannot suppress quackery, we may always do something towards rendering it harmless.

We have to repeat, also, our decided opinion, that the professedly Homœopathic, although duly qualified, practitioner, must be treated exactly as other empirics; and consequently cannot be met in consultation, or co-operated with, in the treatment of a case. Every man duly authorised, has the undoubted moral right to practise his art to the best of his judgment; but he has no right to assume therewith the privilege of publicly depreciating and vilifying the profession at large. If he carry his quarrel into the court of public opinion, and appeal to lay judges to decide questions upon which they are incompetent to express an opinion, he is excommunicated by his own act. He ignores the professional tribunal; the profession justly ignores him. And although he may not be a member of the "English Homœopathic Association," nor identify himself *openly* with any similar body or any other supporters of the system, yet, if he so act as to give his indirect sanction to it, he cannot be held blameless. Professional honour and etiquette may not be left dependent on a sophism or a quibble.

Lastly, we cannot neglect this opportunity of expressing our gratification in learning that the British Code of Medical Ethics, which we desiderated in our April Number, is likely to be drawn up with the authority of the Provincial Medical Association. This question of appeal to a lay public will, we doubt not, attract the serious attention of the framers; and we trust that there will be a provision against the growing system of advertising and reviewing medical works and essays in the Daily Journals. It is, we fear, a cunning system of charlatanic puffing. We would also venture to suggest that they weigh well what is practicable and attainable; and in doing this, investigate carefully the etiquette and practice of the two kindred professions—the church and the bar. For the rest, we have no fear that so great a body as the Medical Profession will maintain its dignity by achieving the great ends of its existence. Without a steady eye upon these, no efforts can be successful; no laws effectual. A deep sense of individual insufficiency will constitute the best basis of unity, order, discipline. Isolated in their efforts, medical practitioners will, as a body, compass no great thing; united, there is hardly anything so great that they may not compass. But the union must be wholly interpenetrated with the great principle of Christian love. On a former occasion we adverted to this, and our readers will bear with us, we hope, if we here repeat what we then wrote. "*We would commend the comment of St. Paul on this new law of love, revealed by Christ, to be found in his letter to the Christians of Corinth, AS THE BEST CODE OF MEDICAL ETIQUETTE; and as comprising all that is necessary for soothing or preventing those bickerings, jealousies, rivalries, and deadly enmities, felt, and too much indulged in, by some professional men.* Love, St. Paul says, is forbearing, obliging; love is not envious; love is not arrogant, is not proud, is not rude or selfish, nor irritable, nor slanderous. It has pleasure in truth and not in falsehood. It is content with all, confides in all, trusts to all, bears with all. The highest rank, the greatest skill, the profoundest learning, are without this, nothing; the greatest performances and accomplishments in literature and science, without it are vain as the jingling cymbal."

## PART SECOND.

**Bibliographical Notices.**

- ART. 1.—*Pathology of the Human Eye.* By JOHN DALRYMPLE, F.R.C.S.  
Fasciculus V.—4to. Four Plates.
2. *Surgical Anatomy.* By JOSEPH MACLISE, Surgeon. Fasciculus VI.  
—Folio. Four Plates.
3. *Portraits of Diseases of the Skin.* By ERASMUS WILSON, F.R.S.  
Fasciculus VII.—Folio. Four Plates.

WE have a pleasure in noticing the progress of these beautiful works, not only because it is far more agreeable to praise than to blame, but because we can freely bestow the commendations due to all concerned in their production, without even being suspected of partiality; the whole Medical Press being, for once, unanimous in bearing testimony to their excellence.

I. The fasciculus of Mr. Dalrymple's 'Pathology of the Human Eye' now before us, is perhaps the most interesting that has yet appeared, as regards alike the subjects treated, and the mode in which they are handled, both by the author, and the artists who have executed his illustrations. The first figure, illustrating a somewhat rare disease—inflammation of the anterior hemisphere of the eye attended by increased secretion of the aqueous humour, and depression of the iris into a concave form—is peculiarly beautiful and characteristic. The remaining figures of the first plate illustrate the disease, which it is now the fashion to term, *Aquocapsulitis*; this, as Mr. Dalrymple justly remarks, though sometimes occurring as the result of extension of inflammation from the cornea proper, or as an early symptom of idiopathic iritis, "is unquestionably often a distinct and primary disease." We cannot but think, however, that he assumes too much, when he says of the supposed "membrane of the aqueous humour," that "though it may be difficult to demonstrate anatomically, I myself have no doubt of its existence." He places the question of its presence on the anterior capsule of the lens, on the same footing with that of the question of the existence of a distinct synovial membrane on the articular cartilages; but it is a little curious that all exact microscopical evidence has of late gone to show that no such membrane is traceable in the joints after a very early period of life. This is a question, however, rather of speculative than of practical value; the diagnosis and treatment of the disease are the important matters for the practitioner; and whilst the figures and descriptions of Mr. Dalrymple will materially assist him in the former, he will find the directions given for the latter of great value.

In Plate II, the subject of *Iritis* is introduced; "the disease of the eye above all others interesting to the pathologist." "Whether," continues

Mr. Dalrymple, "we regard it simply as affecting the iris, and endangering the vision from closure of the pupil; as the type of an inflammation of a serous membrane; or as illustrating in a far more perfect manner than any other human malady, the *modus operandi* of our remedies, it must be considered a subject well worthy the most attentive consideration, and the profoundest observation." We know, indeed, no cases more encouraging to those whose faith in the curative power of remedies is wavering, than those of such forms of iritis as are appropriate for mercurial treatment. A patient comes to us with intense vascularity of the conjunctiva and sclerotic, dullness of the cornea, cloudiness of the aqueous humour, contracted and irregular pupil, opaque fibrinous deposits upon the iris and within the pupillary aperture, and intense pain not only in the eye itself but in the orbit and temples. We know from sad experience to what termination this disease is almost sure to proceed; adhesion of the contracted pupil to the capsule of the lens, and obliteration of the pupillary aperture by an opaque false membrane, and almost total blindness, are the almost inevitable consequences; and worse results arising from the extension of the inflammatory action to other tissues, especially the choroid and retina, are not unfrequent. But if satisfied that the case is one in which mercury may be safely administered, the well-instructed practitioner places his chief (almost his whole) reliance upon that remedy alone; and is rewarded, in the vast majority of well-selected cases, by complete success. "As soon as any evidence of its action in the system is apparent, the vascularity declines, the eye becomes brighter, the aqueous membrane clears, and the deposits of fibrin are absorbed; and when the patient is fully under its influence, the inflammation speedily vanishes, neuralgic pains cease, and the iris, once more bright and brilliant, obeys the command of belladonna, and the pupil dilates." Though laying great stress on the utility of the mercurial plan of treatment, Mr. Dalrymple does not forget to put forth due cautions against its indiscriminate employment. In strumous cases, he recommends the use of the milder forms of mercury, in combination with iron or quinine; and as an example of the value of this method, he cites the case of "a very delicate young woman, suspected of tubercular disease of the lungs, and in whom the anterior chamber was filled by numerous deposits of fibrin, in all stages of organization;" yet in whom "three grains of the Hydr. cum creta, with five of the sesqui-oxide of iron, taken three times a day, sufficed to restore the healthy action of the eye in less than a fortnight, without ever having made the mouth perceptibly sore." This is certainly a most encouraging result of the treatment of a case, in which many practitioners would have shrunk from giving mercury in any form.

In the Third Plate, we have a very interesting representation of the results of unchecked iritis; the preparation from which the drawings were taken having been obtained from the body of a woman, who died at an advanced age in the Marylebone Hospital, after having been for many years blind of one eye. The usual changes had taken place in the pupil, which was contracted, irregular, everywhere adherent to the margin of the lens, and blocked up by a deposit of opaque lymph. The special interest of the preparation and delineations, however, lies in this; that although there was no appearance of the extension of the inflammation and disorganizing action to the choroid and retina, yet the retina of the affected eye was found to be wasted and converted into a nearly diaphanous membrane,

contrasting strongly with the pulpy opaque condition of the healthy retina; and the optic nerve of that eye was not above half the size of the healthy nerve on the other side, being, moreover, of a reddish colour, and much softer than natural. This atrophy proceeded no further than the chiasma. Mr. Dalrymple regards the change in the condition of the nervous textures as resulting entirely from their functional inactivity; and in this explanation we have no doubt that he is quite correct. He mentions a very interesting case, illustrative of the power of a renewal of activity to re-excite the nutritive processes, and consequently to bring about a restoration of the functional power, even after a very long interval. The subject of it lost the sight of one eye, when a child, in consequence of traumatic iritis; at the age of 60, he was unfortunate enough to meet with a second accident, which completely destroyed the sight of the hitherto sound eye; and in the hope of affording to him some useful vision with the eye originally injured, Mr. Dalrymple performed a series of operations upon it, so as to bring about the absorption of the opaque lens, and to make an artificial pupil of good size, and clear. These were *at first* attended, however, with little improvement to the sight; the patient being only able to distinguish light and shade, and not appreciating the form of any object, even under the most favorable circumstances. A gradual improvement took place, however; and at the end of a year, he had recovered so much visual power as to be able to walk about safely, and even to read large and bold type.

The Fourth Plate, and its accompanying text, are devoted to the illustration of a peculiar disease to which no place has yet been assigned in Ophthalmological Nosology.

"The seat of this peculiar inflammation is, I believe, primarily in the sclerotica; but in its progress it involves the cornea and iris, and the ciliary body, the conjunctiva being but slightly and secondarily affected. The complicated character of the disease renders it doubtful under what head we should class it; for while attacking, as it does, the fibrous tissues, the pain which constitutes so marked a feature in the early part of its course leads to the belief that it is of a rheumatic type; yet the effusions that result from a persistence of the inflammation, appear more allied to strumous tubercle, than to those bolder organizable deposits of fibrin which we see in genuine iritis."

In the course of this disease, an infiltration of matter of a tubercular character takes place into the sub-conjunctival cellular tissue, as well as an exudation between the sclerotic and choroid, especially about the corpus ciliare.

"When this effusion has taken place, we see the pupil displaced, drawn usually towards the seat of the disease, and if it be not irregular, it is at least eccentric; the iris, also, sometimes participates partially in the inflammation, as is evidenced by its change of colour. Still later in the disease, the cornea itself becomes opaque in the vicinity of the sub-conjunctival swelling, and its tissue seems to become softened and swelled, and the opacity assumes a pale buff or flesh colour, without, however, distinct vessels pervading it; this degeneration of the tissue is apt to spread, and in some cases almost surrounds the cornea. If not interfered with by local irritants, it seldom ulcerates; and on the disappearance of the original disease, it again clears to a great extent, if not wholly; the iris also resumes its natural colour, and, if staphyloma does not result, its proper central condition. The vision is much less disturbed, for a considerable time, than might have been expected; and where opacity of the cornea does not exist, is, indeed, little troubled, showing how cir-

cumscribed, at least, is the internal implication of the choroid, and how limited is the pressure of the effusion. Pain is a variable symptom: sometimes in the first activity of the disease it is sufficiently acute, chiefly of a neuralgic character, and radiating, as in rheumatic scleritis, to the frontal and sub-orbital nerves. Sometimes it is of a less severe kind, and gives rise to symptoms of distension of the globe, and of stiffness in the movements of the organ. There is some amount of intolerance of light, and the constitutional symptoms are more or less indicative of a strumous cachexia, with little or no febrile excitement. This malady is usually seen in young adults of a scrofulous diathesis, and in girls is often associated with disturbance of the catamenial functions."

The disease has been recognised and described by M. Sichel, whose account of it closely corresponds with that of Mr. Dalrymple, save that he considers the inflammatory action as commencing in the choroid, and regards the swelling and elevation of the sub-mucous tissue as secondary. Both observers agree in regard to the tedious and intractable nature of the complaint. Mr. Dalrymple states that he has found mercurials much inferior to turpentine, in checking the inflammatory action; and that after the subsidence of the more active symptoms, the course most certainly useful has been a combination of liq. potassæ, bark, and colchicum, with occasional blisters behind the ears, and a mild and sustaining form of diet. Of this disease, of which Mr. Dalrymple states that he has seen several cases during the last three or four years, six beautiful figures are given, illustrating the various stages of its progress.

II. Of Mr. MacLise's Plates, the first contains a series of diagrams designed to illustrate "the nature of congenital and infantile inguinal herniæ, and of hydrocele;" and the second comprises a similar series, demonstrating "the origin and progress of inguinal herniæ in general." These are very judiciously contrived to convey a kind of knowledge to the student, which must greatly facilitate his comprehension of this somewhat difficult subject. We have derived great satisfaction from the examination of them; and are satisfied that Mr. MacLise has adopted the best method of elucidating the topic, in fixing attention specially on the mode in which the testis descends from the abdomen to the scrotum, carrying before it prolongations of the various layers of the abdominal parietes.

There is nothing new to modern anatomy in this mode of looking at the subject; but the surgeon who writes upon the "parts of hernia," too frequently dwells upon questions of minute detail, the solution of which is easily and simply found in a little attention to the mode in which the peculiar arrangement of the several textures was first brought about. The following passage will illustrate the advantage derivable from surveying the subject from this point of view.

"Much difference of opinion prevails as to the true relation which the cord (and consequently the oblique hernia) bears to the lower margins of the oblique and transverse muscles, and their cremasteric prolongation. Mr. Guthrie has shown that the fibres of the transversalis, as well as those of the internal oblique, are penetrated by the cord. Albinus, Haller, Cloquet, Camper, and Scarpa, record opinions from which it may be gathered that this disposition of the parts is (with some exceptions) general. Sir Astley Cooper describes the lower edge of the transversalis as curved all round the internal ring and cord. From my own observations, coupled with these, I am inclined to the belief that, instead of viewing these facts as isolated and meaningless particulars, we should now fuse them into the

one idea expressed by the philosophic Carus, and adopted by Cloquet, that the cremaster is a production of the abdominal muscles, formed mechanically by the testicle, which in its descent dilates, penetrates, and elongates their fibres."

We are most happy to congratulate Mr. MacLise on the improvement of his powers of composition. His language is now free from all semblance of affectation; and his anatomical descriptions are quite to the point, including just so much of comparative and philosophical anatomy as serves to give an interest to the subject, and sometimes really to assist in its comprehension. Thus, it helps the student to appreciate the nature and purposes of the separation of the sac of the tunica vaginalis from that of the peritoneum, and to understand how the communication sometimes remains pervious, when he learns that in Quadrupeds generally (with the exception of the Chimpanzee, which in this particular agrees with man), the serous spermatic tube remains open, their naturally prone position rendering them secure against hydrocele or hernial protrusion.

In the other plates of this fasciculus, Mr. MacLise illustrates the subject of Femoral Hernia. The third plate contains four admirable drawings of dissections, in Mr. MacLise's best style; and the fourth gives a series of diagrammatic representations of "the origin and progress of femoral hernia, its diagnosis, the taxis, and the operation." These diagrams constitute a very valuable feature in Mr. MacLise's illustrations of Hernia, and we doubt not will be duly appreciated by those for whom they are especially designed.

III. Although the "Portraits of Diseases of the Skin," are said to be by Mr. Erasmus Wilson, yet it is but right to remark, that they are not by Mr. E. Wilson, in the same sense as the "Surgical Anatomy," is by Mr. MacLise, the latter gentleman being his own draughtsman and lithographer, whilst Mr. Wilson has availed himself, both for the original drawings and for the lithography, of the services of Mr. Bagg. We make this remark, not the least in disparagement of Mr. Wilson, whose production of such a series is most creditable to his taste and liberality, as well as to his love of science; but simply in justice to the talented artist by whom the "Portraits" are actually taken.—The first Plate illustrates *Scabies*, and is an excellent delineation of the purulent form of that disagreeable affection. As we have remarked, in another part of the present Number, (p. 353,) upon the difference of opinion in regard to the *Acarus Scabiei*, it may be as well to mention that Mr. Wilson considers the presence of the *Acarus* to be the pathognomonic sign of true *Scabies*; thus regarding as *Scabies* all the eruptions in connection with which the *acarus* is to be found; whilst those in which no *acarus* can be detected are to be unhesitatingly pronounced *not* to be *Scabies*. This is one way of solving the problem; but it is one which can be scarcely admitted in pathological inquiry.—The subject of the second Plate is *Lichen syphiliticus tuberculatus*, of which a highly characteristic example is figured, the syphilitic character of which can be scarcely questioned. The history of the case is interesting, as showing that, if the patient's account is to be credited, the syphilitic taint was derived from an infection nearly thirty years previously, and had remained dormant in his system until called into activity by other agencies unfavorable to health; or else, that it had been recently contracted from an impure connection, without the occur-



rence of any primary symptoms. The third plate represents a case of *Lupus non exedens*; in the treatment of which very troublesome disease, Mr. Wilson states that he has derived great advantage from the liquor hydriodatis hydrargyri et arsenici, which, in the case here described, (a lady, æt. 37, who had suffered from the disease, more or less violently, since childhood), was taken (in doses of ten drops three times a day), with occasional intermissions, for two years, the result being that the progress of the disease was entirely checked, and all the tubercles removed. —The subject of the fourth Plate is a case of the *Erythema marginatum* of Bateman, the *Lichen marginatus* of some other authors, the *Lichen annulatus serpiginosus* of Mr. Wilson. It will be sufficient to say of all these "Portraits," that they fully keep up to the standard of excellence displayed in the previous Fasciculi; higher praise we cannot give.

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ART. II.—*The Life and Correspondence of Andrew Combe, M.D. F.R.C.P. Ed., One of the Physicians in Ordinary, in Scotland, to the Queen, &c. &c.* By GEORGE COMBE.—Edinburgh, 1850. 8vo, pp. 563. With a Portrait.

Few medical men have done more service to mankind, in their day and generation, than Dr. Andrew Combe. He made no great discovery; he enunciated no original truths; but he called the attention of an intelligent section of the public to scientific principles of the highest importance to human welfare, a knowledge of which had been previously confined to a few; and he expounded these with a combination of earnestness, simplicity, and philosophical discrimination, which diffused his teachings over a very extensive range, made them easily comprehended even by those least prepared to receive them, and generally left a deep and operative impression of their truthfulness and value. Notwithstanding the number of books of the same class, which have issued from the press since the first appearance of Dr. A. Combe's 'Principles of Physiology applied to Health and Education,' it has not been surpassed by any of them, either in the soundness of its teaching, or in its adaptation to the public wants; of which the sale of 28,000 copies in this country, besides numerous large editions in the United States, affords ample proof. And his works on 'Digestion and Dietetics,' and on the 'Physiological and Moral Management of Infancy,' have been scarcely less extensively useful.

Dr. Combe's executors were led to desire that a Memoir of his Life and a selection from his Letters should be published, by the hope (as we are informed in the Preface), "that they might prove acceptable to medical students and young medical practitioners, as supplying them with hints calculated to assist the former in their studies, and the latter in the discharge of their practical duties; to patients, as containing his opinions on a variety of cases of chronic maladies to which general rules are applicable; and to his numerous friends and acquaintances, and that portion of the public which knew him through the medium of his books, as exhibiting him in his private capacity as a son, a brother, and a member of domestic society; and, above all, as a patient, acting out in his own person, in trying circumstances, and during a long course of years, those principles of hygiene which he taught in his writings, and recommended to general adoption." The preparation of such a life was undertaken by his brother,

Mr. George Combe, than whom there was probably no other person who was fitter for the task, especially as, by the copiousness of the materials left, Dr. A. Combe could be rendered, to a great extent, his own biographer.

From his desire to embrace every important topic which was likely to be of interest to Dr. Combe's admirers, his biographer has extended the memoir to dimensions, which many readers not personally interested in the subject of it will doubtless consider to be scarcely required by the importance of the materials; yet, in the attempt to give a complete portraiture of Dr. Combe's life and character, we cannot see that any large part of them could have been advantageously passed over; and an omission that might have rendered the work more palatable to some, would have destroyed its interest for others. This is somewhat amusingly exemplified in the collection of opinions which Mr. G. Combe obtained from various sources, when the work was half through the press; some regretting that so much space was bestowed on the details of Dr. Combe's own malady, and of his hygienic treatment of it, whilst others regard this as the most valuable portion of the book; some considering the large dose of phrenology incorporated with the Life as a great obstacle to its extended utility, whilst others consider it essential that the strength of Dr. Combe's convictions of its truth and importance should be prominently expressed. For ourselves we can only regret, that a due regard to the wishes of all parties could not have been found compatible with a greater compression of the materials; since we are satisfied that the bulk of the volume, rather than any particular part of its contents, will prove the great obstacle to its extensive circulation, especially amongst the medical profession.

To such as have leisure for its perusal, we can cordially recommend it as a volume full of interest and instruction. And for the sake of those who have not, we could much wish that Mr. G. Combe would publish a selection from his brother's papers, of such as bear upon the hygienic treatment of chronic disease, a subject very imperfectly understood in the profession, but one upon which Dr. A. Combe was perhaps better qualified to speak, in regard alike to principles and to practice, than any one whom he has left behind him. Of these a small volume might be made, which would meet, we doubt not, with extensive acceptance, both with the medical and the general public.

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ART. III.—*Tic Douloureux and other Painful Affections of the Nerves; with Suggestions for their Treatment by means of the Aneuralgicon.* By C. TOOGOOD DOWNING, M.D. M.B.C.S.—London, 1849, 12mo, pp. 73.

THE purpose of this *brochure* is to bring into notice a simple apparatus for applying warmth and sedative vapour to any part of the surface of the body; by which means the author has succeeded in affording great relief in many cases of obstinate neuralgia. He by no means holds out this method as a specific; on the contrary, his view of the general treatment of the complaint is sensible and judicious; and his narrative of cases certainly leads to the conclusion, that the Aneuralgicon may afford great relief in this very distressing malady. But the local application of the vapour of chloroform would probably serve the purpose as well, and without any apparatus or trouble.

ART. IV.—*Me ghorar el nagah fi a'amal el garrah*.

*The Book of Success in Surgical Operations.* By MOHAMMED ALEE-EL-BAGLEE, Surgeon to the Hospital Kassar el Ayn, and Professor of Surgery in the Medical School of Cairo. 3 vols., 8vo.

PROBABLY very few of our readers will ever see a copy of the above Arabic work; and as our own knowledge of the language is not very great, we have merely to state, that the first volume contains the minor surgical operations, with directions for dressing and bandaging; the second, the subject of operations in general; and the third, special operations, or those on particular regions of the body. The mere publication of such a work is a fact of no little significance; as it is, we believe, the first medical work written in Arabic since the decline of Eastern learning after the fall of the Caliphate. When Clot Bey succeeded in establishing Medical Institutions in Egypt, the author, then a youth, with eleven companions, was sent to Paris to study. They remained several years, and on their return became the teachers of Medicine and Surgery in Cairo. Only five of this party remain, and by them have been produced elementary works on Anatomy, Physiology, and Medicine, which, with the work before us, are the text books of the Cairo school. The author has been indebted to Rhazes and other medical authors of the classical period of Arabic literature for his phraseology; only introducing modern terms when absolutely necessary, and in that case adapting them to his native pronunciation by slight orthographical changes.

The work is one of the many evidences daily brought before the Eastern traveller, of what Mohammed Alee's educational system effected; and one which leads us to deplore the lamentable manner in which the present ruler of Egypt has neglected the brilliant example of his predecessor.

ART. V.—*An Introduction to Conchology; or Elements of the Natural History of Molluscous Animals.* By GEORGE JOHNSTON, M.D. LL.D. F.R.C.S. ED.—London, 1850. 8vo, pp. 614.

THIS work was suggested by Kirby and Spence's well-known 'Introduction to Entomology,' and forms a worthy and appropriate companion to that highly esteemed treatise. "My object," says the author, "is to present the conchologist with a view of the economical, physiological, and systematical relations of molluscous animals to each other, and to other created beings. I am not aware of any other introduction, in the English language, in which this has been attempted; and therefore my little ambition to occupy the vacant niche in the literature of a favorite department of natural science may be the more excused." No excuse could be necessary from one who ranks so high among the working naturalists of this country, as the author of the 'British Zoophytes,' for there are few who unite as fully as he does the literary qualifications, and the knowledge of his subject, requisite for the production of such a work.

In former times, an 'Introduction to Conchology' was a mere technical account of the general characters of shells, the terms used in describing them, and the principles of the Linnæan arrangement. Such a work could now only satisfy the mere shell-collector, who fills his cabinets with

objects of whose nature he is content to remain profoundly ignorant, and only cares for systematic arrangement as a means of disposing his treasures in some kind of order. The notions of classification which such a man entertains, are very little higher than those of a collector who should arrange his shells according to their size, their colour, or the character of their surface. To him, a shell is either "univalve," "bivalve," or "multivalve;" and, in adopting these characters as the groundwork of his classification, he associates Cephalopods and Pteropods, and even a few Acephala, with the Gasteropodous "univalves;" breaks down altogether the grand distinction between the Brachiopodous and Lamellibranchiate "bivalves;" and makes up his group of "multivalves" by associating the Gasteropodous *Chitons* with the Lamellibranchiate *Pholades*, and some other equally dissimilar forms. It is now universally admitted, however, by all who have the least pretensions to a scientific acquaintance with conchology, that all classification must be based upon the characters of the animals by which the shells are formed; and that a knowledge of these must therefore constitute the foundation of every attempt to work out a systematic arrangement, that shall indicate the real affinities of the shells now furnished, in a number and variety scarcely dreamed of by the older collectors, not only from the remotest parts of the existing surface of the globe, both terrestrial and marine, but from almost every portion of its crust yet explored. And not only is such knowledge absolutely essential to the scientific conchologist; but a new interest is imparted to the contemplation of the treasures brought together by the collector, when each shell is regarded, not only for and by itself, but as the *exuvium* of an animal, in whose structure, physiology, or habits there is certain to be something worthy of attention. It has been the aim of Dr. Johnston, to endeavour to awaken such an interest; and the plan and execution of his work are admirably adapted for his purpose. The following enumeration of the titles of some of the chapters will give an idea of the nature of its contents.—Inducements to the Study of Conchology.—The Mollusca considered as hurtful Animals.—The Mollusca considered as edible Animals.—The Mollusca considered in their relations to Man.—The Shell considered in its relation to other Animals.—The Mollusca considered in their relations to Inorganic or Dead Matter.—On the Locomotion of the Mollusca.—The Burrowing and Stationary Mollusca.—The Boring Mollusca, and Nest-builders.—Nervous System and Senses of the Mollusca.—Their Circulating System.—Their Secretions.—Their Respiration.—Their Food and Digestive Organs.—Their Reproductive Functions.—Their Diseases and Parasites.—Structure and Formation of Shell.—Nomenclature.—History of Conchology, including a View of the Different Systems of Classification.

Many of these subjects had been previously treated of by the author, in a series of letters which he contributed to the 'Magazine of Natural History,' between the years 1829 and 1834; and a considerable part of the work is made up of a reprint of these letters, with such additions as the advance of science rendered necessary. We everywhere find evidence that the author has kept himself quite *au courant* with the present state of knowledge; and have only to wish, that in introducing his new matter, he had incorporated it a little more artistically with the old, so as to give to his readers a clear idea of what they are to receive as true. For it happens more than once, that there is a complete contradiction between

the older and newer authorities cited; if the latter be correct, it would have been much better to *substitute* their descriptions for those of the former, instead of merely *adding* them; and if they be not trustworthy, they had better not have been introduced at all. Thus, in the chapter on the Circulation, the vascular system in the different groups is first described according to the statements of Cuvier; and an account is then given of the more recent researches of Milne-Edwards, who has proved the almost complete deficiency of proper systemic veins in the Mollusca,—the blood meandering back through mere *lacunæ*. And, to add to the confusion of the unenlightened reader, the objections of Messrs. Hancock and Embleton to some parts of Milne-Edwards's views are made to form the conclusion of the chapter. So, again, in the chapter on the Structure and Formation of the Shell, a summary is given of the researches of Dr. Carpenter and Mr. Bowerbank, which prove its organic origin; yet, in the reprint of an anterior Memoir, by Mr. J. E. Gray, which is appended to this chapter, the Shell is treated as a mere inorganic exudation,—the peculiarities of arrangement, which have been shown to be due to the disposition of the organic basis, being still set down to crystalline action.

Although we have thought it right to mention these defects, for the sake of putting our readers a little on their guard in their perusal of the work, yet we can most conscientiously recommend it as, on the whole, a most admirable treatise, containing a large amount of ancient learning, pleasantly blended with a comprehensive summary of the most approved modern knowledge, and setting forth its subject in a pleasing and attractive style. To all, therefore, who take a pleasure in shell-collecting, the work may be strongly recommended; and it has the peculiar claim on the friendly regard of our readers, of being the production of the “learned leisure” of a member of our own profession, who has not the less actively or successfully discharged his duties as a practitioner, from having recreated his mind by the study of the Zoophytes and Mollusca of the coast on which he resides.

When we add, that the work is published by Mr. Van Voorst, in the same form with his series of treatises on British Zoology, and that it contains numerous wood-cut illustrations, executed in the best style, we shall have said enough to recommend it on the score of its “getting-up,” as well as of its contents.

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ART. VI.—*Health, Disease, and Remedy familiarly and practically considered, in a few of their Relations to the Blood.* By GEORGE MOORE, M.D., &c. &c.—London, 1850. 12mo, pp. 372.

2. *On the Principles of Health and Disease. An Inaugural Dissertation of the University of Edinburgh.* By DAVID NELSON, M.D., Physician to the Queen's Hospital, and Professor of Clinical Medicine in Queen's College, Birmingham.

ALTHOUGH the first of these books is intended rather for the non-professional than for the medical reader, whilst the second, as an academical thesis, must be considered as addressed to professional judges, yet there is a considerable similarity between them, both as to their character and their tendency. Dr. Moore is well known as a pleasing writer on subjects

occupying the border ground between the ordinary range of popular inquiry and that of professional learning; and his present treatise may be regarded as a well-meant, and on the whole well-executed attempt to inform the public as to some of the principles by which the enlightened practitioner of "orthodox" physic is guided. We are quite of opinion that in medicine, more perhaps than in any other department of art, "*a little knowledge is a dangerous thing*;" tending, as it too frequently does, to make the patient believe that he knows better than his doctor, and weakening his confidence in the treatment pursued (if, indeed, he does not abandon it altogether), so soon as he finds it running counter to his own notions. But the public *will have this* "little knowledge;" and the question is, whether we shall leave it to be supplied to them by the books of homœopathic, hydropathic, hygeist, and mesmeric quacks, or whether we shall endeavour so far to enlighten them with regard to our own procedures, as to make them see that we have true science as well as experience on our side. Dr. Moore's treatise, as described by himself, "is neither a popular compendium of physiology, a hand-book of physic, an art of healing made easy, a medical guide-book, a domestic medicine, a digest of odd scraps on digestion, nor a dry reduction of a better book, but rather a running comment on a few prominent truths in medical science, viewed according to the writer's own experience, and on the principles of common sense. The object has been to assist the unprofessional reader to form a sober estimate of physic, and enable him to second the physician's efforts to promote health; and throughout there has been a desire that even the professed medical student might, if he looked, find in the volume a few hints of some value to himself." Having carefully examined the work, we feel justified in saying in its commendation, that while we question the judgment of introducing some of the topics which are discussed, and dissent from some of the views expressed by the author, we heartily accord in its general spirit, and can conscientiously recommend it to those who are entering upon the study of medicine, as being well fitted to enlighten them as to its legitimate objects, and the mode in which they are to be attained.

Whilst Dr. Moore comes before us with the experience of a practitioner of thirty years' standing, Dr. Nelson makes his *début* fresh from the schools, in which he would seem, from the appointments with which he has been honoured, to have been a distinguished pupil. In addressing the profession rather than the public, we might naturally expect to find him presenting some new idea, if no new facts; but we must confess that his thesis does not seem to us to contain either, and that we are rather at a loss to understand the motives which induced him to publish it, unless he intended that it should be considered as his "profession of faith." Looked upon in this light, it may be characterised as a clear and sensible summary of many important doctrines on which intelligent physiologists, pathologists, and practitioners, are now agreed, in regard to the nature, causes, and treatment of the principal forms of disease; and we are glad to think that the Birmingham Medical School should have the services of a gentleman, who will enunciate and dwell on these doctrines in his clinical teaching.

ART. VII.—*Every-day Wonders: or Facts in Physiology, which all should know. Illustrated with Wood-cuts.*—London, 1850. Fcap. 8vo, pp. 137.

WE cannot impart to our readers a better notion of the character of this admirable little book, than that which its Preface will afford them.

"The object of the writer of this book has been to present a few of the truths of that science, which treats of the structure of the human body, and of the adaptation of the external world to it, in such a form, as that they shall be readily apprehended by children and young people. The lively interest which intelligent children would take in the works of God, within and around them, is frequently checked by the manner in which they are introduced to the study of science. Portions of elementary scientific books are required to be learnt as task-work, and the learner inwardly condemns the subject as 'dry,' and abandons all farther pursuit of it when at liberty to do so.

"Where a teacher communicates orally such a thorough explanation of the terms employed, and such familiar illustrations as may enable the child, not only *to remember words*, but *to apprehend truths*, the child's wonder and delight are excited, and such a beginning is likely to awaken sufficient energy to lead to sound and valuable acquisitions.

"The writer disclaims any wish to represent the attainment of science as an easy work. But for the very reason that the path is toilsome, it is surely unwise to scare the learner from the entrance.

"It is attempted in this little book to supply the explanations and illustrations by which a teacher would win the attention of a child.

"For this purpose it has been thought better to *dwell* on a few *facts*, than to crowd the book with many. Pains have been taken that all the information imparted should be accurate."

We cannot give the book higher praise, than to say, that it most successfully realises the purpose thus judiciously set forth; and that we can speak from experience of its complete adaptation to the mind of an intelligent child. We should like to see some more scientific introductions from the same pen.

ART. VIII.—*Instinct and Reason: deduced from Electro-Biology.* By ALFRED SMEE, F.R.S.—London, 1850. 8vo, pp. 320. With Lithographic Plates and Wood-Engravings.

HAVING not long since (Vol. IV, p. 371) devoted no fewer than ten of our pages to an inquiry into the merits of Mr. Smee's system of philosophy, we do not think it requisite to bestow any lengthened space upon this last treatise, which is to be regarded as one of its offsets, produced during a period of repose from the author's severer labours, which we presume to be directed towards "the construction of a living, moving, feeling, thinking, moral, and religious man, by a combination of voltaic circuits," which will assuredly cause Mr. Alfred Smee to rank as the greatest genius of this or any other age. The author informs us, that the composition of this work was undertaken for amusement rather than labour; and truly we think that he must have found a congenial relaxation from his severer studies, in the free indulgence of his propensity to vanity and egotism, which this treatise displays, to the great amusement of his readers. We do not recollect ever to have looked into a book, in which the smallest trifle affecting its author was exaggerated into so much importance. Of this we

shall cite a few examples for our readers' edification, taking them just as they come. The following is Mr. Smee's estimate of the merits of his own researches :

"It would be vain to conceal, and useless to deny, that the system of Electro-Biology, upon which this work is based, required intense labour in the laboratory and dissecting-room, to learn the facts; and even then, the subsequent labour required for their arrangement was so great, that it cost me many years of anxious toil for the development of the system." (Preface, p. vi.)

Mr. Smee makes his Description of Plates a vehicle for advertising for a lost paper.

"The figures in this plate represent the distribution of the blood-vessels in the brain and spinal cord. These beautiful injections are exceedingly difficult to prepare; but they are made by using a solution of carmine in ammonia mixed with size; a process which I communicated to the Microscopical Society, but the society has contrived to lose my paper. (*Note.* If any gentleman now has it, I should feel extremely obliged if he would return it to me, as its loss has caused me much inconvenience)." (p. xxiv.)

Mr. Smee scolds his puppy-dog for carrying off a chicken from his breakfast-table; and the puppy, having learned wisdom by experience, has not done so again (p. 10). On the contrary,—

"My dog, when I return home, shows unequivocal signs of memory by jumping about and wagging his tail. He also, by little artifices, seeks to attract my attention, and induce me to caress him." (p. 53.)

We almost wonder that Mr. Smee did not introduce a picture of this highly interesting occurrence among his illustrations; it would have been just as appropriate as a great many of the subjects he has figured, such as the Nineveh Sculptures, or the Old Wooden Exchequer Tally, of which last he has given not only two figures, but a description two pages long, just because he finds that no gentleman in the Bank of England recollects the mode of reading them! What either has to do with "*Instinct and Reason*," deduced from Electro-Biology," we must own ourselves at a loss to discover.

Under the head "*Operations of Man superior to those of Animals*," we find all Mr. Alfred Smee's inventions in Electro-Metallurgy, &c. cited in illustration; and, curiously enough, under the head "*Perverted Reason*," we have no fewer than *twelve pages* devoted to the history of the "*Aphis vastator*," and the laws of its action on the potato, introduced for the purpose of showing "*the extreme difficulty which the mind finds in estimating the cause of any event*,"—Mr. Smee being obviously quite unaware that this speculation of his was generally regarded by competent judges as a most excellent example of the "*perverted reason*" which he is exposing.

We had marked a number of amusing passages for citation; but it would be scarcely fair to our author to separate too many of these innocent frailties from the connection in which they occur. The following, however, is a choice example of a "*stage aside*," which is told to the whole audience, but which the other party to the conversation is supposed not to hear. Speaking of a certain wasp's nest, which has been lent to him by Mr. Bowerbank, Mr. Smee tells his readers :

"When Mr. Bowerbank asks for this interesting specimen, I intend to use every persuasion to induce him to allow me to keep it, by offering him a substitute in some other department of natural history in which he more delights." (p. 161.)



Our author happens to be at the Opera when 'Roberto il Diavolo' is performed, and wishes to see behind the curtain how the scenic effects are produced. Behold Mr. Alfred Smee, F.R.S., "behind the scenes."

"At first I thought that it was hardly consistent with my dignity to visit such a spot; but I reconciled myself by the thought that I should be sure to see nobody whom I knew. However, to my astonishment and amusement, I found several persons who, at one time or another, had come before me in my official capacities, and who were engaged in various departments of the opera." (p. 234.)

The book is, in fact, a congeries of all sorts of odds and ends, brought together out of the treasures of Mr. Alfred Smee's reading and personal experience, with the slenderest possible connecting thread. In fact, if we were asked the subject of the book, we should say that it was at least as much an exposition of Mr. A. S.'S doings and thoughts *de omnibus rebus et quibusdam aliis*, as a Treatise on Instinct and Reason. We began to reckon up how many of the 604 paragraphs of which the body of the work is composed, contain some special mention of the author, as *I, we, my, or mine*; but gave up the enumeration, when we found that after going through about one third of the volume, those paragraphs in which Mr. Smee's personality is thus brought prominently before the reader, certainly constitute the majority. One thing he takes every opportunity of impressing upon the minds of his readers,—namely, that Electro-Biology is to effect the great *moral* revolution by which the whole world is to be renovated; when, of course, Mr. Alfred Smee will take rank as a sort of second Messiah, whose work is to complete that which was left imperfect by the first.

"I have much cause to rejoice that the system of Electro-Biology, which I have developed, after many tedious hours of investigation with the midnight oil, and after many years of anxious thought, should now be found to be a system deduced by reason, and in perfect accordance with that religion which I hold by faith. I have, indeed, even more cause to rejoice that reason [*i. e.* electro-biology] and faith mutually re-act, each supporting the other, and causing it to be more firmly fixed in the mind. I have, however, most cause to rejoice, because this union of faith and reason cannot fail, when universally known, to have the best influence in supporting pure religion, in suppressing infidelity, bigotry, and irreligion, and in extending the inestimable blessings of true Christianity throughout the different countries of the globe." (p. 227.)

And all this is to come out of Smee's galvanic battery! Among all the arrogant assumptions which it has been our lot to meet with, we do not recollect to have met with one that beats this; yet it is by no means an unfair sample of what the reader of this strange book will not unfrequently encounter.

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ART. IX. — *Proteus; or the Law of Nature.* By CHARLES BLAND RADCLIFFE, M.B., Lecturer on Vegetable Physiology and Botany at the Westminster Hospital, &c.—London, 1850. 8vo, pp. 150.

"MANY thoughtful and philosophical men," as the Author of this treatise rightly observes, "have believed in the existence of a common idea or principle in the works of Nature, by which all things are linked together in one; and the joint dictates of experience and tradition are in harmony with this belief." This common principle he endeavours to trace out, first in the material organisms of Plants and Animals, and in

the analogies which connect unorganised substances with living structures ; and secondly, in the mutual affinities of the forces or agencies which are operative in animated and inanimate nature.

Under the former head he considers, in the first place, the Unity in the Organs of living bodies, Plants and Animals ; then the Unity of the entire Organism, in the Plant and in the Animal ; next the Unity between Plants and Animals ; and lastly, the Unity of the Forms of Organic and Inorganic Nature. In his treatment of this part of his subject, the author displays considerable knowledge both of Zoology and Botany ; and shows a remarkable ingenuity in the detection of analogies between objects apparently the most remote. We cannot but think, however, that he has allowed a fertile fancy sometimes to lead him astray ; and that his analogies are occasionally far-fetched and superficial ; as for instance, where he likens the eye of a bird to "a polype, the development of which has been arrested immediately before the completion of the perfect state." (p. 44.) And this love of tracing *resemblances* often leads him to overlook the *differences* which are far more weighty. Thus he attempts to show a unity between the annulose and the vertebral type of conformation, on the ground that certain aberrant *vertebræ* and *annellæ* show a certain approximation to each other ; overlooking the fact that the articulated skeleton is essentially *dermal*, the muscles being attached to its interior ; whilst the typical vertebral skeleton is *internal*, and is clothed by the muscles which move it. If he were to study the embryology of these two groups respectively, he would find that the first clear indication of segmentation is shown in the articulated animal on the *surface* of the germinal mass, whilst in the vertebrated it is in the centre, along the 'primitive trace.' Still this part of the volume may be read with profit, for the suggestions it contains, even by those who have deeply thought on the subject of it ; and we have remarked upon some of what we believe to be its errors, only for the sake of putting our readers on their guard against too ready a reception of Dr. Radcliffe's statements on points of detail, his general doctrines being, in our apprehension, perfectly sound, and capable of being demonstrated in the most satisfactory manner, especially by the data supplied by embryological research.

The second part of the work "Of Unity in Force," commences with a general account of Professor Grove's views on the 'Correlation of the Physical Forces ;' on which the author founds his views of "Unity in the Force of Organic and Inorganic Nature." These views we believe to be correct in the main. So far as we can judge of them, they correspond with those which we ourselves put forth some time since, (Vol. I, p. 235), and towards which several intelligent thinkers (as we have recently learned) have been for some time converging. Still we must confess that Dr. Radcliffe's enunciation of them is so far from clear, that we cannot always satisfy ourselves that we apprehend his meaning ; especially in the closing portion of his treatise, in which he mixes up his physiological views with theological speculations, whose connection with them he has by no means made evident.

Our readers will perceive, therefore, that the recommendation of Dr. Radcliffe's work, which we should be most glad to give on account of the valuable matter it contains, and the obviously high qualifications of its author, must be accepted with considerable reservation ; and that it is on

the whole better adapted for conveying suggestions to those who are already sufficiently informed on the subject to sift its truth from its error, than for introducing the beginner to the contemplation of that interesting aspect of Nature, in which the combination of Unity and Diversity is so wonderfully displayed. We should add, too, that the want of illustrations would render the book almost useless to the beginner, to whom the mere names of the Plants and Animals referred can convey no definite ideas.

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ART. X.—*The Accommodation of the Eye to Distances*. By WILLIAM CLAY WALLACE, M.D.—*New York*, 1850. 8vo, pp. 36.

THE substance of this essay has already appeared in various journals, medical and scientific; it is now republished, however, in a somewhat enlarged form. The author's theory on the difficult subject of which he treats is, that the adjustment is chiefly effected by the ciliary body, which, when its vessels are turgid with blood, will draw forward (he affirms) the crystalline lens.

"If, when the eye is adjusted to a remote object, we direct it by the external muscles to one which is near, an indistinct image of the latter is formed on the retina; the impression is communicated to the sensorium by the optic nerve; a reflex affection of the third, from which the ciliary nerves in part proceed, causes the ciliary muscle to contract, the processes to become erect, and the crystalline body to be drawn forward, until a distinct image of the object is formed upon the retina." (p. 24.)

According to our author, the eye, in a passive state, is adjusted for the discernment of distant objects; and the adjustment for near objects can only be made by a special effort, which, when long continued, becomes painful. The correctness of this statement, however, entirely depends upon the sense in which the words "near" and "distant" are to be understood. If the ordinary "reading distance" of twelve or fourteen inches is to be regarded as "near," we decidedly dissent from the proposition; since we are satisfied that a person of ordinary visual powers can read at that distance with as little fatigue as he would incur from looking for the same length of time at distant objects. The fact is (as any one who understands optics theoretically, or the working of optical instruments, well knows,) that the adjustment required for the change from a distant object to one moderately near is very trifling; but that the great alteration is required when the object is brought within a distance of ten or twelve inches; and *here* we fully recognise the justice of Dr. Wallace's statement, that an effort is required. We quite believe that the ciliary muscle is largely concerned in this effort, and that it advances the lens towards the cornea; the structure and attachments of this muscle in the eye of the bird being such, as necessarily to produce this effect. But it is by no means so clear to us, that, in man and the mammalia, the ciliary muscle produces the same effect, by compressing the veins of the ciliary body, and thus causing its erection; and we should like to see this statement put to an experimental test, which we should think it might be, with the aid of injections.

## PART THIRD.

## Periscope.

## ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

*On the Development of the Retina and Optic Nerve; and of the Membranous Labyrinth and Auditory Nerve.* By HENRY GRAY, M.R.C.S.

THIS paper contains some very interesting additions to what had been made out by previous observers, in regard to the development of the nervous apparatus of the eye and ear; Mr. Gray's observations having been made on the embryo chick.

*Development of the Eye.*—The first rudiment of the eye is seen at the thirty-third hour, as a lateral projection from the anterior cerebral cell, to which projection Mr. Gray gives the name of optic vesicle. At first the cavities of the two freely communicate; but the optic vesicle is gradually detached from that of which it was an offset; and finally remains connected with it by a pedicle, which becomes the optic nerve. In this account of the origin of the eye, Mr. Gray's observations accord with those of Baer, and are in opposition to those of Wagner and Huschke. Whilst presenting a pyriform shape, there is still a fissure connecting the cavity of the optic vesicle with that of the cerebral cell; and it is probably this fissure which remains permanently open in the retina and optic nerve of fishes, and in the nerve though not in the retina of reptiles, whilst in birds it gives admission to the pecten. The retina at first consists of a single layer, continuous with, and derived from that which lines the cerebral cell. The lens is first seen as an ill-defined granular mass, in the cavity of the vesicle itself, containing in its centre a nucleus; but between the fourth and fifth days, the granules become darker and more aggregated towards the centre, and the distinction between lens and capsule begins to appear. According to Huschke, the crystalline lens is formed (so to speak) outside the retinal layer, and doubles it in, by pressing against its anterior surface, so as to form two layers; this account, however, is decidedly stated by Mr. Gray to be erroneous:—"In no position that I put the embryo, in each of the several examinations that I made, could I ever detect but a single layer." The spherical end of the protrusion is gradually absorbed; and by the time that the retinal layer is completed, it becomes attached to the margin of the lens, in front of which it originally passed. At the 8th day of incubation, the choroidal surface of the retinal layer consists of a closely aggregated mass of globular nuclei, about the size of the red corpuscles of the blood, highly refractive, and of a slight yellow tinge; these apparently correspond to the "agglomerated granules" mentioned by Mr. Bowman, as forming a considerable portion of the membrane in the normal state. Its deep surface consists of some fine granular matter, and of a mass of pale and exceedingly delicate nucleated cells, precisely similar to those included in the meshes of the fibrous lamina in the normal structure of the membrane. The first appearance of Jacob's membrane, as a delicate epithelial layer on the choroidal surface of the retina, is to be found on the 15th day; the epithelial cells gradually elongating into the staff-like bodies. The first trace of the fibrous layer is to be detected about the same time; and this is at first composed of a very fine pale granular lamina, marked by numerous faint longitudinal striæ, which, by the 18th day, is found to consist of numerous fibrillated

bundles, forming numerous meshes by their interlacement, in which are enclosed the gray nucleated vesicles just described as visible on the 8th day.

[These observations fully confirm the views of those who have taught that the cells of the vesicular layer of the retina are not, as affirmed by Henle, of an epithelial nature, but are true ganglionic corpuscles. Mr. Gray is in error, however, in specially ascribing this doctrine to Mr. Bowman, as it had been taught by Valentin and other physiologists for some years prior to Mr. Bowman's first publication on the subject. There is a difficulty attending Mr. Gray's views of the origin of the optic vesicle from the anterior cerebral cell alone, which he does not explain. The optic nerve, which is the pedicle of that vesicle, is undoubtedly connected, when fully formed, with the optic lobes which are derived from the second vesicle, as well as with the optic thalami which are developed from the first. When and how is this connection formed, if it does not result from the original relations of the parts?]

*Development of the Ear.*—In his general account of this process, Mr. Gray again coincides with the accurate Baer; stating that the auditory vesicle is formed by a protrusion of the central part of the wall of the cavity of the medulla oblongata (subsequently to become the fourth ventricle); this protrusion, like the optic vesicle, gradually becomes a pyriform sac, the cavity of which is to form the vestibule, whilst the pedicle is to close up into the auditory nerve. In this stage of development, a remarkable analogy shows itself between the auditory apparatus of the bird, and the organ of hearing as it normally exists in the higher invertebrata. The membranous lining of the semicircular canals is formed by the contraction, folding-inwards, and subsequent union, of the walls of the terminal portion of the vesicle itself; a process which begins during the fourth day. The cartilaginous nidus, in which the petrous portion of the temporal bone originates, does not make its appearance until about the 12th day, by which time the development of the membranous semicircular canals is nearly completed. "It is interesting to remark, that the membranous labyrinth between the 8th and 13th days, has a structure almost precisely similar to that of the retinal expansion of the same period, consisting, like it, of a distinct but very delicate fibrous mesh, in the spaces between which are deposited a quantity of granular matter and numerous nucleated cells, its exterior surface being composed of a dense mass of nuclei, almost precisely analogous to the agglomerated granules which form so large a portion of the entire substance of the retina."—*Philosophical Transactions*, 1850, Part i.

[Mr. Gray's observations on the development of the auditory apparatus are particularly interesting, as confirming the views of philosophical anatomists as to the fundamental analogies between the visual and auditory apparatus, by the surest of all proofs, those derived from the history of development. It appears, too, that in the earliest condition of the auditory apparatus in the bird, a very close analogy may be traced between its character and position, and that which it permanently presents in the Gasteropod Mollusca, where it is imbedded in the suboesophageal ganglia, which constitute the homologues of the medulla oblongata in the vertebrata.

Although Mr. Gray's paper wants completeness on some points, yet it is a highly valuable contribution to our previous knowledge, and promises well for the future eminence of an observer, who is, we believe, only just out of his pupilage.]

#### *On the Development of the great Anterior Veins in Man and Mammalia.*

By JOHN MARSHALL, F.R.C.S., &c. &c.

THE principal object of this paper is to state the result of observations on the metamorphosis of certain of the great veins in man and mammalia, and on the relation between the primitive and final condition of these vessels, both when they pass through their changes in the usual order, and in cases of deviation from it. Our previous knowledge on this subject is principally due to the researches of Rathke, which it has been Mr. Marshall's object to extend and complete. The

paper is one of great research, but involves so many details, that it will be scarcely possible to give anything more than the most general account of its contents.

The knowledge of the common type of formation of the veins of vertebrata, and of their metamorphosis in some species by partial occlusion, suggested to Rathke the explanation of the occasional occurrence of double vena cava superior in the human subject, as the result of an arrest of development. He did not, however, indicate the details of this metamorphosis; nor had any persistent remains of the fetal structure been recognised in the adult, until Mr. Marshall took up the inquiry.

The metamorphosis of the primitive lateral and symmetrical venous trunks in the higher mammalia, and in man, may be said to consist of two fundamental changes, viz. *a*, the formation of the cross branch, or communication between the two jugular veins in the neck; and, *b*, the occlusion of a greater or less portion of the left primitive venous trunk. Besides these, however; there are, during embryonic life, certain concurrent and subsequent alterations in the size, position, and direction of the venous trunks, which finally remain pervious. Mr. Marshall has shown, that even in the normal condition of the venous system in the adult, the vestiges of its embryonic state are still traceable,—particularly what he terms the “vestigial fold” of the pericardium, which marks the course of the lower part of the primitive venous trunk on the left side; and the small oblique articular vein, which has been described by some of the earlier anatomists as a branch of the great coronary vein, but which originally formed part of the pervious portion of the left ductus Cuvieri.

Mr. Marshall then goes on to show, that the normal varieties in the distribution of the great anterior veins in man and the mammalia, are all explicable by studying the various modifications to which the primitive venous system is subjected in the course of its development, and that the abnormal arrangements which sometimes present themselves in man, may be equally well reduced to an original uniformity. —*Philosophical Transactions*, 1850, Part i.

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*On the Structure of the Liver, a Description of Preparations of the Liver of Different Animals.* By Prof. RETZIUS.

IN the main points Retzius found Kiernan's researches on the structure of the liver to be correct. He admits the lobular structure of the liver, but thinks that the lobuli or acini (with him synonymous) may coalesce, and by that means the lobular appearance would be lost; but that, under certain circumstances, it may return to the former condition. The preparations of the human liver were made from that of a child six months old. These preparations clearly showed that six months after birth the portal system, which during uterine life formed part of the umbilical venous system, still maintains a state of development far in advance of that of the hepatic veins. The injections in Glisson's capsule were very interesting, and confirmative of a fact touched upon by Kiernan, viz., that the hepatic ducts form a network in the walls of this membrane, which is often in continuity with the lobular network of the liver. In two specimens from livers of children two years old, fine hexagonal lobes were observed. In the liver of the dog, Retzius could not so distinctly see the vaginal biliary network lying in regular cylindrical or prismatic planes, as it does in man and other animals. Preparations of livers of the cat, rabbit, hedgehog, pig, and calf, are also described, with very interesting details of the peculiarities of structure of this organ in these animals. The general results of these researches are:—

1. That the liver is in the main lobular; but that the lobular form appears in many stages of development and retrogression, sometimes proceeding to fusion of the lobuli with one another, and combined with a more or less regular development of the ramifications of the hepatic veins. That which most denotes the presence of the lobular type, is the constant occurrence of the alveolar network of biliary ducts.
2. That the biliary ducts are tubes furnished with their own peculiar walls (basement membrane). Retzius was enabled to convince himself of the presence of this

membrane, and found it such as Schröder Van der Kolk has described it,—a simple membrane surrounding the angular and round liver-cells. Retzius procures this membrane by cutting thin slices from a liver (which has been first macerated in ether and then dried), then soaking them in water until they become transparent, when they show the peculiar membrane of the fine network of biliary ducts containing the liver-cells. 3. Retzius found no arterial network in the partitions formed by Glisson's capsule.—*Müller's Archiv.*, 1850.

*Observations on the Growth of the Hair and Nails.* By Dr. BERTHOLD.

To determine the time required by the nail to grow to a certain length, the writer first made some experiments on himself, and found that the nail of the middle finger grew 11 millimètres in four months. Continuing his experiments, he found a great difference in the growth of the nail, according to the age of the person, and the season of the year. For instance, he found that the same nail, which would take 152 days in winter to attain a certain length, would grow to the same length in 116 days in summer. The growth also differs on different fingers, as also on the right and left hand. On the right hand the growth is quicker than on the left.

The hair of individuals, from 16 to 24 years old, grew in two years 12 to 16 inches, or 7 lines a month.

The growth of the hair is accelerated by frequently cutting it.

During the day, reproduction of hair goes on more rapidly than at night. In warm weather, the reproduction is greater than in cold weather.

The quantitative formation of nail and hair coincides with the peripheric secretions, perspiration, &c. in this—that it increases in summer, and decreases in winter; whereas, the development and nutrition of the body is decreased in summer, and increased in winter; so that the weight of a man is greater in winter than in summer.

The growth of hair decreases in the night, which coincides with the decrease of the secretions, perspiration, formation of carbonic acid gas, urine, milk, bile, &c.—*Müller's Archiv.*, 1850.

*On the Existence of Iodine in all Fresh-Water Plants.* By PROFESSOR CHATIN.

MÜLLER some time since discovered iodine in the water-cress, and the confirmation of this, in respect to the plantations of this plant near Paris, convinced M. Chatin that this body is not, as usually thought, confined to the zone of saline waters or mineral springs. He first examined various other species of the cruciferae, under the idea that most of these might contain it, as they do sulphur and nitrogen; but the only one in which he detected it was another aquatic, the *nasturtium amphibium*. He now turned his attention to the fresh-water aquatics, and with some modifications in its mode of existence, found it present in an immense number, a long catalogue of these accompanying this communication. Some of the results of the investigation are thus stated:—1. Plants living in running waters, or in large masses of water capable of agitation by the winds, contain more iodine than do those of stagnant waters. 2. It is found also, though in small quantity, in those which are only imperfectly immersed in water, or during only a portion of their lives. 3. The same plants which contain iodine when growing in water, do not do so if developed out of it.

Examinations of the juices of these plants, and of their parenchyma, proved that the iodine existed exclusively in the former, and therefore as a soluble iodide. In answer to the questions why land plants are destitute of it, and why it varies so greatly in quantity in the fresh-water ones, it may be observed, that iodine can scarcely exist in appreciable quantities in the small quantity of water that gains access to terrestrial plants; and the different position of the aquatic ones, in respect to soils, &c., which may contain it, may account, in some measure, for the variations. Plants agitated by running water contain much, in consequence of the frequent renewal of the water.—*Journal de Pharmacie*, Third Series, tom. 17, p. 418.

*On the Diet of the Belgian Miners.* By M. CHARPENTIER.

IN our last number we extracted, from a communication made by M. Gasparin to the *Académie des Sciences*, an account of the surprising effects which he attributed to a vegetable regimen and coffee, in imparting strength to the miners of Charleroi. Dr. Charpentier, of Valenciennes, struck with the contradiction this statement offered to facts of which he was cognisant, obtained official information upon the subject, and forwarded it to the *Académie*. From this it seems that the miners of Charleroi, like persons engaged in other occupations, receive very different wages, and by no means follow any identical regimen. As a general rule, when not at the works, they drink a portion of corn-brandy and nearly a quart of small-beer daily, eat of pork, with vegetables, two or three times a week; while on the Sunday they take meat three times in the day, and drink large quantities of beer. M. Gasparin's statement of their diet is quite correct while they are actually engaged at work in the mine, because they find that the digestion of any more substantial food is difficult in the painful positions of the body they are obliged to assume.

As to the health of these persons, it is very indifferent compared with that of the population not engaged in mining occupations. While certain parts of the body become abnormally developed, others are wasted. They are prematurely incapacitated for laborious exertion, and usually die soon after fifty. Still they live longer than the miners in some of the neighbouring districts; but that does not depend on diet, which is very similar in all, but upon the fact that, while at Liege, Mons, and other places, the workmen have to ascend and descend the mines with great labour, at Charleroi they are transported to the place of work by machinery. —*Gazette Médicale*, No. 26, p. 497.

*Specific Gravity of Animal Substances.*

C. SCHMIDT has determined the specific gravity of various animal substances, all of which yield an ash when burnt. The sp. gr., as experimentally determined, is given under A, and as calculated after the removal of ash under B.

	A.	B.
Blood corpuscles . . . .	1250.7	1239
Fibrin of muscle . . . .	1283.3	1276
Albumen of hen's eggs . . .	1314.4	1286
Tendons . . . . .	1301.1	1299

*Ann. Ch. Pharm.*, t. lxi, p. 156; and *Liebig's Report*, vol. ii, p. 131.

*Influence of Animal Charcoal in removing the active principles of Plants from their Solutions in Water.*

It is customary to clarify the impure solutions of vegetable active principles by filtering them through a deep stratum of animal charcoal. The researches of Lebourdais, however, show that this cannot be done without losing a large quantity of the valuable active principle. Lebourdais has even proposed to take advantage of the absorbent powers of animal charcoal, as a means of extracting these principles. He takes the aqueous solution of the plant, and boils it with bone-black, previously freed from phosphates by hydrochloric acid, until the liquor has become colourless, and is no longer of a bitter taste; the charcoal is then washed with water, and, after drying, extracted with boiling alcohol. The latter extracts the bitter principle, or the organic base, and it remains in a pure state after the evaporation of the alcohol. Colouring matters may be previously separated from the juice by means of a lead salt.

Lebourdais states that he has in this way prepared colourless syrupy ilicin, scillitin, arnicin, and colocynthin; some other proximate principles, as digitalin, columbin, &c., were obtained in a crystallised state. —*Ann. Ch. Phys.*, xxiv, p. 58.; *Liebig's Report*, vol. ii, p. 109.



*Composition of the Blood of New-born Animals.*

POGGIALE has investigated the composition of the blood in new-born animals. In man he found the same amount of solid matter in the blood of the placenta as in that of the fœtus; and the composition of the former was :

Water	.	.	.	.	.	.	.	.	744.2
Corpuscles	.	.	.	.	.	.	.	.	172.2
Fibrin	.	.	.	.	.	.	.	.	1.9
Albumen	.	.	.	.	.	.	.	.	69.3
Fatty matters	.	.	.	.	.	.	.	.	8.1
Extractive and salts	.	.	.	.	.	.	.	.	10.3

*Compt. Rendus*, xxv, p. 198; and *Liebig's Report*, vol. ii, p. 154.

*Influence of a Salt Diet on the Composition of the Blood.*

POGGIALE has, moreover, examined the blood of man, both at the time that the usual diet was taken, and whilst 154 grs. of salt were consumed daily. The following are the results :

	During usual diet.	During salt diet.
Water	779.9	767.6
Blood corpuscles	130.1	143.0
Albumen	77.4	74.0
Fibrin	2.1	2.3
Fatty matters	1.1	1.3
Extractive and salts	9.3	11.8

From which it is evident that the proportion of solid constituents are increased; this occurs chiefly in the blood corpuscles and extractive, the amount of albumen being slightly diminished.—*Compt. Rendus*, xxv.

Boussingault has also extended his observations concerning the influence of salt on the fattening of cattle. His earlier experiments had shown, that salt does not exert that beneficial influence on the growth of cattle, and the production of flesh, which is usually ascribed to it. His present experiments have been extended over a period of thirteen months, and have been made on a number of steers, some of which had their rations salted, while the others had not; in other respects they were treated in a precisely similar manner. The results have shown, that the increase in the proportion of flesh does not pay for the salt employed. Boussingault, however, remarks, that a saline diet exerts a beneficial effect on the appearance and condition of the animals; for the steers which were deprived of salt for eleven months, appeared sluggish, and of a languid temperament; their coats were rough, devoid of gloss, and partially bare; while those which had been fed with salt were lively, had a fine glossy coat, and were sure to obtain a considerable higher price at market.—*Ann. Ch. Phys.*, xx and xxiii; and *Liebig's Report*, vol. ii, p. 340.

*Condition of Carbonic Acid in the Blood.*

LIEBIG remarks, that while water only takes up its own bulk of carbonic acid gas, serum has the power of absorbing twice its bulk of it. Now as this cannot be dependent on the presence of neutral carbonates in the blood, Liebig endeavours to show that it is owing to the existence of basic phosphate of soda. This chemist finds that a solution of one part of dry phosphate of soda ( $2\text{NaO},\text{HO},\text{PO}^3$ ) in 100 parts of water, absorbs, likewise, a double volume of carbonic acid. By shaking up with air, or by diminishing the atmospheric pressure, two thirds of the carbonic acid taken up are evolved at the ordinary temperature; the entire amount of carbonic acid gas is given off during simple evaporation in the atmosphere. When the blood absorbs carbonic acid, the soda of the former is appropriated partly by the carbonic acid, and partly by the phosphoric acid; but the phosphoric acid

which has been expelled, remains and tries to reunite itself with all the soda; consequently the phenomena are different from what they would be if the blood really contained carbonate of soda as such.—*Liebig's Report*, vol. ii, p. 158.

Liebig states, that serum strongly concentrated by evaporation, does not evolve a trace of carbonic acid, on the addition of acids. Lehmann, on the contrary, asserts that blood contains a large quantity of alkaline carbonate. He has communicated the results of experiments, in which the free carbonic acid was expelled by hydrogen introduced into the blood, and the combined acid by means of acetic acid in a rarified space. According to his determinations, 1000 grains of fresh ox-blood yields on an average 0.132 grains or 0.28 cubic inches of free carbonic acid, and 0.676 grains or 1.42 of combined. He mixed blood with an equal quantity of water, coagulated the albuminous constituents by heat, and evaporated the filtered fluid to dryness. The residue was incinerated at the lowest possible temperature; in 100 parts of ash there were found from 4.1 to 4.5 of sulphate of soda; 3.7 of phosphate of soda ( $3 \text{ NaO}, \text{PO}^3$ ); from 15.8 to 18.1 carbonate of soda; and from 74.0 to 75.0 of alkaline chlorides.—*Liebig's Report*, vol. ii, p. 157.

*On the Salts contained in Cholera Evacuations.* By Dr. GÜTERBOCK.

THE following are the author's conclusions, drawn from the chemical examination of the stools in cholera.

1. A most remarkable circumstance is the large proportion of water, amounting, on an average, to 98.119; while, according to Berzelius, normal stools contain 75.3. The specific gravity, too, is remarkably low, varying from 1006 to 1008; the urine itself, in but few diseases, manifesting so low a one. 2. The solid constituents amount on an average to but 1.581. 3. Among these, the inorganic salts constitute by far the largest portion—upon an average 4.5ths, the organic matter being only 1.5th:—a proportion that is the reverse of the normal, in which the organic parts form 19.20ths according to Berzelius, and 9.10ths according to Rose. 4. Among these salts the most remarkable is the *chloride of sodium*, constituting, upon an average, nearly  $\frac{1}{2}$ , and, in some cases, 2-3ds, of the whole. The cholera stools contain nearly  $\frac{1}{2}$  per cent. on an average, while, according to Berzelius, the normal proportion is but 0.28; and according to the latest analysis of solid excrement, by Rose and Fleitmann, this proportion is even unusually large, as in 100 parts of the entire salts, only 1.58th were chloride of sodium. Thus the cholera stools contain a hundred times as much salt as the normal; and, indeed, so considerable is the quantity, that after the evaporation of the fluid, the unassisted eye recognises the characteristic crystals. 5. Next to this the *carbonate soda* is most abundant, and besides these, are some phosphate of magnesia and lime, and a trace of the sulphate. The absence of *potass* in the stools is remarkable, forming, as it does, a chief saline constituent in the normal solid stools. According to Rose and Fleitmann, these contain 22.49 per cent., while in the cholera stool only a trace is detectible. 6. The organic matters of cholera stools consist in great part of mucus and the remains of epithelium, and it has generally been believed that they are rich in albumen. The experiments of the author and of Corenwinder contradict this, for they either found it entirely absent, or mere traces present. Masselot (*Gaz. Méd.*, No. 14, 1849,) states that he found a considerable quantity; but as the stools were compared to soup in colour, they probably contained blood—an important point to be borne in mind during investigations for albumen. In almost all cases a small quantity of casein is present, especially in stools removed after death. 7. The abdominal discharges were always alkaline, but the *vomits* neutral, or slightly acid. These latter, too, contained a still larger proportion of water, viz., 99 per cent., and were little above the specific gravity of water. Upon an average, the salts constituted about a half of the solid matter, but the proportion between these and the organic constituents was not so constant as in the alvine evacuations. So also the proportion of chloride of sodium varied, but it never exceeded that of the other salts. No albumen was present. 8. The contents of the bowel thus appear to become changed in

their passage, not only by the presence of broken epithelial scales, but also in chemical composition, being richer in organic constituents than when they have passed the anus. 9. It is to be expected that the increase of chloride of sodium in the stools is contemporary with the diminution of the substance in the blood noticed by O'Shaughnessy, Rayer, and Mulder; but the cessation of the epidemic prevented Dr. Güterbock examining this point for himself. Supposing future researches to affirm it, it is only one step towards explaining an increase, which, compared with Rose's analysis of normal excrement, is a hundredfold. According to this chemist, a large portion of the chloride of sodium is excreted in the urine, almost 600 times as much as in the stools; and the arrest of the secretion of urine during cholera may explain the enormous increase in the stools.—*Poggendorf's Annalen*, Band lxxix, p. 323.

*A Case of Partial Arrest of Muscular Development.* By Dr. HUNTINGTON.

THIS case occurred in a young man, æt. 22, of good habits, in whom, without any obvious cause, the muscles of the thighs, pelvis, and arms have ceased being developed during eight years, although those of the leg, forearm, foot, hand, and back are fully developed. The gastrocnemii are very large, while the muscles of the thigh and the glutei are flaccid and shrunk, as in the last stage of phthisis. The contrast between the arm and forearm is not so striking, but is still apparent. He complains of no pain or inconvenience, other than the weakness attendant upon such a state of the muscles; and both sides seem affected alike. When he rises from a sitting posture, he does so by the assistance of the upper extremities, with a sort of springing movement; and he cannot mount a stair without a great effort, while he is quite unable to raise himself from a stooping posture without aid.—*Philad. Med. Exam.*, N. S., vol. vi, p. 454.

*Elementary Composition of Epithelium and Epidermis.*

M. SCHREER formerly determined the composition of the last-named body, and Gorup-Besanez has lately ascertained the composition of the epithelium of the whale. This substance was obtained by scraping the mucous membrane of this animal. Under the microscope it appeared to be composed of well-developed tessellated epithelial-cells, which do not differ from those of man, either in structure or chemical deportment. After repeated treatment with water, alcohol, and ether, drying and pulverizing, it formed a yellowish powder, which was with difficulty soluble in potassa. The alkaline solution gives with acetic acid a precipitate soluble in excess of acid; the latter solution was precipitated by ferrocyanide of potassium. Boiling hydrochloric acid produced a blue solution similar to that of albuminoid substances. Analysis afforded the following results:

	Epithelium (Gorup-Besanez).	Epiderm (Schreer).
Carbon . . . .	51.53	50.34
Hydrogen . . . .	7.03	6.81
Nitrogen . . . .	16.64	17.22
Oxygen . . . .	22.32	25.63
Sulphur . . . .	2.48	undetermined.

Gorup-Besanez does not consider the discrepancy sufficient to establish an essential difference between the two substances.—*Ann. Ch. Pharm.*, t. xl and lxi; and *Liebig's Report*, vol. ii, p. 132.

## PATHOLOGY AND PRACTICE OF MEDICINE.

*A Case of Hæmorrhagic Diathesis.* By Dr. DUNLAP.

A young lady, æt. 22, who had for some time suffered from symptoms of hepatic disease, and subsequently from suppressed catamenia, found a **profuse discharge** of blood issue from her mouth six weeks after this **last event, which continued** unchecked by remedies for five days. For **four or five monthly periods** in succession, she had like attacks before the reporter saw her, her health continuing tolerable in the intervals. Called to her at the commencement of another attack, he found blood oozing from the gums, both in front of and behind the teeth, which reappeared as soon as it was wiped away. As on former occasions, it continued to increase for five or six days, and then spontaneously subsided, it being calculated that about six quarts were discharged. (?) Renewed endeavours were now made to correct the hepatic derangement, and to restore the menstrual secretion; but the bleeding returning in a month, continued longer than usual, and prostrated the patient more than ever. On account of the pain in the side, she was cupped, in the reporter's absence, and the bleeding from the gums entirely ceased. It was found, however, impossible to arrest the bleeding from the scarifications, and in less than six hours she died. At the autopsy, the only disease discovered consisted in some enlargement of the ovaries, and the presence of some calculi in the gall-bladder.—*New York Journ. of Med.*, N. S., vol. iv, p. 316.

*On Oxæna.* By M. MAX-SIMON.

M. MAX-SIMON, in relating a case of this disease, makes several interesting remarks. Sometimes, he observes, it arises from ulceration of the Schneiderian membrane; at others, a deformity of the organ, whether congenital or acquired, prevents the free discharge of the mucosities of the nose, while in some cases there are signs of simple chronic inflammation. Another class of cases, of which this is an example, arises from a morbid condition of the economy at large, inducing modification in the condition of the secretion: just as we meet with intolerable stinking secretion from the feet, in persons perfectly clean; a peculiar odour from the axilla, occurring in others besides the red haired, in whom it is normal; and an insupportable condition of the breath in certain menstruating women. In such cases as these, the most careful examination exhibits no marks of inflammation or ulceration. The ulceration, in fact, which is met with in syphilis or other diseases, is not the cause of this peculiar stench, which is different from the smell they produce. A delicate young lady, æt. 14, had been under M. Simon's care for several years, for well marked *oxæna*. Repeated examinations exhibited nothing abnormal, the mucous membrane being pale and not thickened, and the mucus itself normal in appearance, though sometimes increased in quantity. All medical treatment, whether local or general, proved quite unavailing, so that at last all was abandoned, but extreme cleanliness, and attention to maintain the general health in as good a condition as possible. However, menstruation came on, under the influence of which her system became much stronger, and her health far better, and the fætid odour diminished, to entirely disappear sixteen or eighteen months after the menses had become established. Another proof of the constitutional character of the disease in this case, and of the inutilty of local measures, is found in the fact that a blister having been applied to the girl's arm, the serum furnished precisely the same smell. Her mother, too, suffered from the same infirmity, although by excessive cleanliness she kept it somewhat concealed. Her nose was a flat one, but that of her daughter was well formed.

These well-marked cases must not be confounded with a mere temporary unusual odour of the nasal mucus. In some persons this is liable to become accumulated

or suppressed for a longer or shorter time. In the case of a young man, in whom there is no congestion of the mucous membrane, thickened mucosities are blown out from time to time, having a distinct spermatie smell. A young girl, at times, exhales mucus having the characteristic smell of ozæna, and then, without the slightest care on her part, this disappears.

When the disease depends upon a mere perverted action of the mucous membrane, the nitrate of silver injections are very useful; but when it is a constitutional affection, all local applications are mere palliatives. The greatest cleanliness is essential, and scented waters should be frequently injected into the nose. Sauvages found advantage in some cases, from the setting up a substitutive action by the use of snuff.—*Bulletin de Thérapeutique*, tom. xxxix, p. 8.

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*An extraordinary Case of the Duration of Insanity, with Return of Reason prior to Death.* By M. BRIERRE DE BOISMONT.

THIS patient, formerly a gardener, æt. 74, and having always previously had good health, was suddenly terrified, during a masquerade, in 1799, by the supposed presence of a bear. He became motionless and speechless, and from that moment maintained the most enduring mutism, never being heard to speak, and avoiding all persons with fear, taking his repasts in a corner by himself, and making sounds, supposed to be imitative of a bear, whenever any one approached him. He ate well, became stout, and was never ill. In January, 1844, he was attacked with diarrhoea and loss of appetite, and eventually with œdema of the legs. In proportion as this last made progress, his dislike to society seemed to diminish, and on the 1st February, he replied to the interrogatories respecting his health, which, during fifty-two years, had been so repeatedly put to him in vain: he having, during that long interval, spoken to no one, or even looked in a book. He now, however, perfectly understood the questions, and replied to them rationally, though only in monosyllables; but nothing could be got out of him in respect to past transactions. He thus remained fully conscious until he died on the 16th February. No autopsy was made.—*Annales Méd. Psych.* N.S. vol. ii, p. 531.

In the same number of this journal a case is related, that occurred to M. Foville, which is interesting by the fact of the temporary return of consciousness in a case of confirmed *general paralysis*. A professor, by a reckless course of dissipation and extravagance, had ruined the once favorable prospects of himself and those dependent upon him, and at last became the subject of paralysis with dementia. For three months he had continued in a state of the completest lethargy, when, two days prior to his death, entire consciousness returned; and he manifested the most feeling and rational repentance for the mischief his misconduct had caused his relatives. M. Foville, who had repeatedly observed him during the progress of the disease, declares, that during these last two days, it was impossible to detect the slightest discrepancy in his observations.

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*On Chorea.* By Dr. LEE.

AN analysis of various published cases of this disease, and of forty-two observed by himself at the *Hôpital des Enfants*, leads Dr. Lee to the conclusion that there are four principal varieties of it.

1. One which has been called *sympathetic*, coincides with the local lesions of the various viscera of organic life, and especially with disease of the gastro-intestinal system and of the heart.

2. A second, which is very common, depends upon a *general* disease, and especially *rheumatism*. So frequent is this variety, that it constituted seventeen out of the author's forty-two cases, and thirty of seventy-four he has collected. Rheumatism indeed may not only give rise to chorea, but to a variety of other nervous disturbances, as simple convulsions, contractions, tetanic convulsions, pseudo-meningitis, pseudo-myelitis, &c.; and, in fact, there is no symptom usually referred

to lesions of nervous substance, which may not be dependent upon a rheumatic affection of the joints or heart, such affection being almost always marked by the nervous derangement, and giving rise to only very slight local suffering and febrile action, especially in a chronic neurosis like chorea. When, however, the febrile reaction is intense, the neurosis is usually only developed when the inflammatory fever has undergone some remission; and a reproduction of the febrile action always induces an improvement in the nervous symptoms, except in some cases in which the disease proves quickly fatal.

3. Another form of chorea quite independent of cerebral alteration, is the so-called *essential* chorea, in which no appreciable change of structure is recognisable either in the organic viscera or the nervous system; this, like rheumatic chorea, is a very common form.

4. The last form depends upon *cerebral or spinal lesion*, and is but the symptom of various cerebral and spinal affections.—*Bull. de l'Acad.* tom. xv, p. 343.

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*On the General Paralysis of the Insane.* By M. MOREAU.

Two questions engage M. Moreau's attention in this paper. First, has the number of insane affected with general paralysis been on the increase of late years; and secondly, is general paralysis ever met with unconnected with insanity?

1. *Increase of General Paralysis among the Insane.* An answer to the first question can only be obtained from figures. M. Moreau examines it thus in different classes of society; viz., the mass of the people, the *bourgeoisie*, and the aristocracy. In respect to the *first* of these, the registers of the *Bicêtre* have been very carefully kept for several years, and the result of their examination during 21 years (1828—1849) is adduced. From these it appears that these cases have *progressively increased*, from 7 per 100 in 1828, to 37 per cent. in 1849. The disease has been too well understood since the treatises of Bayle and Calmeil, to admit of this increase being explained by its being better recognised now than heretofore. The figures, too, have been furnished in succession by such eminent observers as Ferrus, Voisin Leuret, Delassiauve, and the author. *Charenton*, especially, receives the insane from the *bourgeoisie* class, and according to the successive reports of Calmeil, Bayle, and Esquirol, the proportion there has increased from  $6\frac{1}{4}$  per cent. in 1820-1825, to 20 per cent. in 1826, and 25 per cent. in 1826-8. More recent numbers than these last furnished by Esquirol are not obtainable; but all persons connected with the establishment state that the number has much increased since then. The *aristocratic classes* constitute the chief number of those who have inhabited the establishment founded by Esquirol; and the numbers, for a long series of years, have been furnished by M. Mitivié. According to these, no progressive increase is to be observed here as in the two other classes, the most extreme variations occurring even quite recently (as 5 per cent. in 1847 to 36 per cent. in 1849), as formerly; so that the safest conclusion is to consider the numbers as stationary, as regards the higher classes.

From this account, the author, believing paralytic insanity to constitute the highest degree of psycho-cerebral disorder, draws the important inference, that in proportion as civilization extends from the upper to the lower ranks of society, anxiety, restlessness, and intellectual excitement, descend with it, inducing cerebral excitement, simple insanity, and finally the most serious of all cerebral disorders, insanity with paralysis.

With respect to the question whether *general paralysis* is ever met with *unconnected with insanity*, M. Moreau arrives at the following conclusions:—1. That the disease generally known under the name of "general paralysis of the insane," really constitutes a morbid individuality, having no analogue in the nosological chart. 2. That the lesion of the motor and of the intellectual powers are pathological elements of equal value, whatever may be the comparative epochs at which they respectively manifest themselves. 3. That the one necessarily implies the other, if the disease is regarded in its entire and complete development, and not in its isolated phases.

4. That the general paralysis and the insanity are pathological phenomena united together, as necessary and constant effects of the same primordial cause. 5. That the two phenomena never manifest themselves completely and absolutely isolated from each other at the onset or in the course of the disease. 6. That if the contrary has been maintained, it has arisen from the erroneous ideas which have generally been held, of the nature of the disturbances of the understanding,—the philosophical sense of the word insanity having been confounded with its physiological and medical sense.—*Gazette Médicale*, No. 19.

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*On the Treatment and Prevention of Habitual Constipation.* By Dr. J. C. WARREN.

THE treatment of chronic costiveness, an ailment so common in persons who lead sedentary lives, may be either medicinal or alimentary. Of medicines, Dr. Warren employs but three, aloes, magnesia, and senna wine. He considers that the bitter principle of *aloes* renders it in some measure a substitute for the deficient bile, and a few grains of the powder or gum forms a mild and effectual aperient. The watery extract (5 to 10 gr.) combined with an aromatic oil, is one of the mildest of laxatives, taken fasting. Aperients should not be taken at night, when they become buried in the alimentary mass and ineffectual, or in their action disturb the repose of patients of an irritable temperament; but when the stomach is empty, either before dinner or breakfast. *Magnesia*, however, is so mild an aperient, that even if taken at night, it does not disturb sleep, and, from its combining with the acids which have been generated during the day, is then even more likely to be useful. When the stomach is not acid, half an orange aids its action. *Senna-wine*, prepared according to Dr. Lane's process, is a very satisfactory aperient, the dose being from  $\frac{1}{4}$  oz. to 2 oz., and as it is slightly stimulant, it may be taken for an oppressed stomach at night. If taken in the morning, it should be diluted with equal parts of water.

Among the *alimentary* substances which tend to relieve constipation in some persons, fruit is the most agreeable; but for this end it should be taken before meals, when the stomach is empty—its employment after a full meal often giving rise to acetous fermentation and consequent disorders. Laxative vegetables are not usually easily digested by persons of weak stomachs, in whom they cause distressing flatulence. In other subjects they may act very beneficially. The costiveness which occurs after weaning is removed by a slightly-sweetened infusion of cranberries. Some persons find advantage in drinking a glass of cold water, others a cup of strong coffee, early in the morning. Animal food is rather laxative than not, especially the fatty portions, which, however, delicate stomachs cannot manage. Some people take much wine, believing it acts on the bowels. If it does so, it is from the mere weight of the fluid, or perhaps from its causing a toxic effect, when the bowels take on a conservative eliminatory action. Most wines and spirits, taken in moderate quantities, adstrict the bowels.

Fine flour is highly nutritious; and if exclusively used, like other nutritious substances, as jelly, arrowroot, milk, &c., causes costiveness. The artificial separation of the covering of the corn is counteractive of the intentions of nature, this stimulating the intestine to expulsive action. After employing, with great advantage, bread containing the *bran* in his own family, and recommending it to numbers of others, the author was induced to try a still coarser preparation. Wheat was ground in a coffee-mill, and then boiled with a succession of water and a little salt for three or four hours. This Dr. Warren has found incalculably *the best and pleasantest remedy for constipation*, effecting quite a revolution in the economy and health, when taken in sufficient quantities (12 oz.), either as a part or whole of the breakfast, or instead of pudding and vegetables at dinner. When the stomach will bear sweet substances, honey, molasses, &c., may be added with advantage. A moderate degree of fluidity, i. e., less than that of boiled rice or hominy, increases the laxative power. The wheat acts in part by its mere bulk, and probably in part by reason of the

stimulating effect of the sharp edges of the particles of bran.—*Amer. Journ. Med. Sc.*, N.S., No. 38, pp. 291-8.

[Although the fine bread so much consumed in towns is doubtless frequently a cause of constipation, yet brown bread, containing the bran, cannot be always indiscriminately substituted. Upon this point, M. Bouchardat has recently published (*Journal de Pharm. et de Chimie*, vol. xvii, p. 277, 1850) some interesting observations, in reply to an inquiry by the Paris municipality as to the propriety of substituting fine bread for the coarse brown supplied to the indigent. He observes that it is quite true, as stated by M. Millon, that by rejecting the outer covering of the corn, we reject a substance rich in gluten and fatty matter, and one which, except 10 per cent. of ligneous matter, is assimilable. Physiology also teaches us that a residue is requisite for the constitution of the alimentary bolus which is to traverse the canal. A natural condiment is prematurely removed when we reject the bran. But for the system to utilise this portion of the corn, the stomach must be capable of digesting it. An ox or a cow can assimilate it completely: and a country peasant, working freely exposed to air and light, and employed in fatiguing labour, digests his coarse bread easily, and utilises the bran completely. But if you give this same bread to poor and aged persons, the bran will traverse the canal unchanged, the soluble matters it contains being defended from solution by their strong cohesion, and by the covering of ligneous matter. For such persons white bread is alone suitable, and the same observation applies to other persons having weak digestive powers, who indeed, Dr. Warren says, are not able to take the boiled wheat in sufficient quantities.]

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*On the Appearances of the Gums in Phthisis.* By M. FREDERICQ.

M. FREDERICQ published some observations in 1847, in which he stated, that in phthisis a red streak is constantly to be found on the gums opposite the lower incisors, and sometimes the upper ones also. Since then he has paid great attention to the subject, and now says, that in the latter period of all chronic diseases, some time before death, a blue or red streak will always be found; but that in phthisis it is one of the earliest signs. In all the cases of *phthisis*, without exception, that have come under his notice, he has met with this brick-red, or blue streak. The *brick-red* colour is especially found in inflammatory phthisis, or at least when much bronchial irritation and nocturnal fever are present; while the *blue* colour denotes a less active form of disease, and is oftenest found in those who suffer from abundant pneumonorrhagia, although by no means always indicating the probability of the occurrence of this. In a few cases, a *white* line, as observed by M. Vanoye, contrasts with the general colour of the mucous membrane, and those who exhibit it are usually of a marked scrofulous and cachectic habit. These streaks are of great importance in announcing early phthisis, in those cases in which doubt prevails as to whether a bronchitis or catarrh is connected with tubercle. The author has never known persons suffering from bronchitis, in whom this mark was absent, afterward become the subjects of phthisis; while those in whom it was distinctly present have all afterwards furnished signs of tubercle. The deeper the colour, the more rapidly does the disease proceed, while it is a good sign to find it becoming paler. Patients, in whom the process of softening has become temporarily arrested, and who believe themselves cured, continue to offer the blue mark, but paler than before. When the disease re-commences its march, the mark becomes plainer.

Dr. Bonarden states that he has observed a blue line in *intermittent fever*. M. Fredericq has generally found it wanting, and when present it has been blue, not red, and seems to be connected with engorgement of the spleen. In *chronic abdominal* affections, there is a dirty, livid-looking streak, along the whole extent of the gums, and much broader than in phthisis. So in the *menstrual molimen* there is sometimes a blue line along the free edge of the gums, which disappears when the menses come on. It is frequently, but not constantly, observed in amenorrhœa.



The following are the author's conclusions: 1. The red, blue, or white streak, along the incisor gums, is only of real semeiological importance when manifested early, since it is always seen in the latter periods of chronic disease. 2. It is observable, as soon as the cough occurs in phthisis. 3. In aged persons, where there is retraction of the gums, it is not present. 4. Where the brick-red streak exists, it is a sure indication of tubercle; the blue one, which is common to other diseases, is also frequently so. 5. Chronic abdominal affections give rise to a livid streak along the entire gums.—*Rév. Med.-Chir.* vol. vii, pp. 139-141.

*Epidemics of Mumps at Geneva and Montpellier.* By MM. RILLIET and RESSIGUIER.

M. RILLIET, one of the authors of the well-known classical work upon the 'Diseases of Children,' now settled at Geneva, has published a very interesting account of an epidemic visitation of mumps which prevailed there from March 1848 to May 1849, Dr. Lombard having assisted him in the acquisition of many of the facts. Still the exact number of cases could not be ascertained.

In the great majority of cases there were no premonitory symptoms, those of a local character being the first intimation of illness the patient received. We need not detail these. The tumefaction of the parotidean and mastoidean regions continued increasing from 4 to 6 days, and, after remaining stationary, then rapidly diminished, so as to have disappeared by from the 7th to the 10th day—the submaxillary gland continuing hard and swollen, in several cases, long after the resolution of the parotidean tumour. The most frequent cases were those in which the tumefaction was of medium size only; but in some it was enormous, extending almost to the external extremity of the clavicle. In such, and in subjects predisposed to neuralgia, the pain was severe and even violent, but in the other cases moderate—it being usually spontaneous, but also augmented by pressure and every movement of the jaw, which, indeed, was sometimes almost as immovable as in tetanus. In some cases, when the mouth could be sufficiently opened to allow the tongue to pass, the patients were unable to protrude it. In no case did Dr. Rilliet or any of his colleagues witness salivation, nor, when examination of the mouth and fauces was practicable, could any exanthema be observed. The swelling was usually double, but rarely so at first, commencing usually on the left side, and then proceeding, in a period varying from 12 hours to 3 or 4 days, to the opposite side, one gland being almost always more swollen than the other.

The accompanying constitutional irritation was not prolonged beyond 48 hours, save in very bad cases. A feeling of great lassitude and debility, accompanied or followed the appearance of the swelling, several patients not recovering their ordinary strength for two or three weeks. The cure was always more prompt and complete in children. The duration of the disease, in slight cases, and in children, was but for 4 or 5 days; but, in other cases, usually 8 or 10; then 6 or 7, and, lastly, 7 to 8 days. In some it continued even to the 15th day. Authors speak of this disease terminating by *suppuration*; but no example of this occurred. It is very rare for the disease to attack children under two years of age, and no example occurred under one. So, too, after 40, cases were very rare. The maximum number occurred between 5 and 15. The two sexes were affected nearly alike.

M. RILLIET believes the disease to be analogous in its nature to *eruptive fevers*, and to be *contagious*. The period of incubation is as difficult of determination as in other fevers; but both the author and M. Lombard believe that it is most often found to be from 20 to 22 days, and then from 14 to 18 days. Still, in one of the best marked cases it did not exceed 8 days. In families the disease usually appeared successively, not simultaneously. The experience of the author and his colleagues is favorable to the non-recurrence of the disease, attacks at former periods of life exerting a marked preservative power. Like other febrile diseases, too, this has occurred as an epidemic—so considerable a one not having been observed at Geneva in the memory of man, the number affected being, as in measles, proportioned to the length of interval between the visitations. It prevailed equally in

warm and cold weather, during its continuance. During its prevalence, various other diseases were observed; but roseola was especially so during its height, not the simple summer roseola, but an eruption very analogous to rubeola.

The treatment of the disease was very simple, frictions with anodyne substances being especially resorted to when the pain was great. Bloodletting was never required, but stimulating pediluvia were employed as revulsives. In children, in whom the digestive organs were usually disordered, emetics were found useful, and almost always slight aperients were given during convalescence. In some cases tonics were eventually required, as were anodynes when the pains were urgent.

The only complication which the author has had frequent opportunity of observing, has been *orchitis* or *engorgement of the testis*. He met with but one case of tumefaction of the labia. The orchitis did not usually commence with severe pains, and the patients have been surprised at discovering the amount of swelling. This usually reached its height in from 4 to 6 days, diminished on the 6th or 7th, and had disappeared in from 8 to 15. At the commencement the scrotum was not cedematous, and the testis rather than the epididymis was attacked with the swelling, which never acquired the hardness of blenorhagic orchitis. In slight cases the testis was half as large again as natural, but in severer cases its volume became doubled or even quadrupled. When the epididymis was affected, it was to a less extent than the testis, and was much indurated only in one case. The cord was almost always normal, but sometimes a little enlarged. Sometimes, on from the 3d to the 12th day, the scrotum also became swollen, with or without redness, the swelling, sometimes considerable, being cedematous; and when the scrotum was implicated, both the swelling and pain were much greater than when the testis alone was affected. It was in these cases that general febrile symptoms were most marked. When the orchitis was cured, the testis in general returned to its normal condition; becoming, however, in some cases atrophied.

The orchitis usually appeared the 6th or 8th day, very rarely from the 3d to the 5th, and the parotid swelling had almost always much diminished; but no case of *metastasis*, properly so called, was observed, nor any example in which the orchitis suddenly disappeared, and the parotitis re-appeared. While the parotitis was oftener double, the orchitis was oftener unilateral; and orchitis on the right side was observed in 13 out of 23 cases, and in 4 it was double. In 2 cases there was orchitis without parotitis. The greatest number of persons attacked by orchitis were between 23 and 38 years of age—the youngest being 14, the eldest 45. Most examples occurred in the lower class of patients, and the complication especially prevailed in January—twice as many cases then occurring, as in all the other months together. The treatment of the orchitis was very simple, as by linseed poultices, fomentations, goulard water when the scrotum was cedematous, &c. Sometimes an emetic was given, and in three cases blood was abstracted.

M. Ressiguier supplies a much less exact account of the epidemic which prevailed at *Montpellier*. It was first observed in February, 1848, and by April its epidemic character had become evident. In this month, the metastatic symptoms were first observed. It became less frequent in June, and sporadic cases were alone observed in July. The swelling usually took on the characters of active cedema rather than inflammation, but in several cases it required leeching. When both sides were affected, they were almost always so successively, the left usually taking the precedence. The accompanying fever usually abated when the tumour was completely developed; but, in other cases, continued during the entire duration of the case, and was attended by exacerbations. In most cases the cedematous engorgement was dissipated in 5 or 6 days, but in others required double the time. It never terminated by suppuration.

In some cases, as the parotid tumour diminished, orchitis showed itself, and between March and June none of the patients were secure from this. In the present epidemic, while the parotitis was as yet unaccompanied by orchitis, a young man became the subject of this without having had any parotid affection whatever; and Groffier, who has left an account of a prior epidemic in this town,

in 1806, relates the case of a man in whom, after the disappearance of an orchitis, the parotitis became developed. A similar case also occurred on the person of a soldier during the present epidemic; and in other patients the two affections appeared simultaneously. In the majority of cases orchitis showed itself on the opposite side to the parotitis, and only in one case was it double, though this was of frequent occurrence in the parotitis. The present epidemic confirms the statement of Laghi, that the so-called metastasis is never seen before puberty, and very rarely in the aged.

The present epidemic, like former ones in 1757 and 1799, especially attacked the soldiers in garrison.—*Gaz. Méd.*, 1850. *Rev. Méd.-Chir.*, vol. vii, p. 204.

## S U R G E R Y.

### *On the Treatment of Sprains of the Ankle.* By M. BAUDENS.

M. BAUDENS observes, that judging by the frequency of the occurrence of this accident, its treatment ought to be well understood and successfully practised; but that this is in fact far from being the case, and he is therefore desirous of making his own plan of treating it, by the cold-bath and gum bandage, more extensively known.

The indications are, first, to prevent or remove inflammation, and then to secure immovability to the distended or lacerated parts, until they have recovered their power, the patient being at the same time allowed the use of the limb. For the purpose of subduing inflammation, numbers of leeches are usually applied, and then an emollient cataplasm; and M. Baudens feels convinced that it is in consequence of such treatment that degenerated sprains so often augment the number of amputations in hospitals. By free leeching of a joint, the seat of sprain, two mischievous effects are produced. In the first place, the pain, which is the first of the series of symptoms of inflammation after sprain, is increased by the leech-bites, in place of being mitigated; and, in the next, the increased afflux of blood towards the part is encouraged instead of being repelled. M. Baudens, on these grounds, strictly forbids the application of leeches in all surgical maladies attended with acute inflammation, while he often derives most excellent aid from their employment in chronic inflammations; thus, by the induction of a temporary congestion, giving a fillip to the too languid action of the part. When blood need be taken in sprain, he abstracts it by venesection, although probably both the profession and the public, from the force of habit, would tax with ignorance any one who neglected the use of leeches. As to emollient cataplasms, they favour in place of opposing the afflux of fluids to the part, while the long maceration the joint has been thus submitted to, deprives it of its elasticity, gives rise to a pasty *engorgement*, and predisposes to the formation of white swelling.

M. Baudens has pursued his own plan of treatment now for twenty years, and under it his patients have been enabled to resume their trying military duties in a very short time. He is not the first who has employed cold water in the treatment of sprain; but his originality consists in trusting to it alone, and continuing its application for so long a period. His plan of employing it, contrasted with that of his predecessors, may be thus summed up:—1. *Period of the Application.* Cold has usually been thought desirable only when it could be resorted to very shortly after the accident; but he applies it not only immediately, but also several hours or days after the occurrence, or even in chronic sprain—whenever, in fact, there is a morbid degree of heat to abstract. 2. The local bath has never been ordered by others for longer than five or six hours, although some practitioners, since his first publication on the subject, have ventured to extend it to twenty-four. In certain of his cases, however, immersion has been continued for eight or ten days, and, in one example, for fourteen days; while in no case has it been less than for two.

3. *Mode of Application.* The vessel containing the water is brought to the bedside of the patient, so that he can conveniently place his leg in it, having the heel resting on a sponge at the bottom, the leg and thigh being supported by cushions, so that the position may be maintained for as many days as required. In the vessels used at the Val-de-Grâce the water reaches as high as the middle of the leg, and is changed about every three hours in order to keep it sufficiently cool. Spring-water is usually employed, and if the inflammation is intense, ice is added. A purgative is given, and, if indicated, one or two bleedings are resorted to.

4. *Effects.*—One of the first of these is the cessation of pain, which sometimes occurs at once, and at others in an hour or two. From the moment the foot is placed in the bath, the swelling becomes stationary, and soon after, with the heat and redness, decreases. About the fourth or fifth day the part becomes wrinkled like the hands of a washerwoman; and usually about the third or fourth day, the patient finds the water too cold, and then the limb is removed from it—the period for doing this being regulated by the patient, he being told to keep it in only as long as he derives comfort from so doing. Few of the patients suffer from any general reaction. *Gangrene* has been said to have resulted from this application, but the author has never met with such a case. The patient sometimes persists in keeping the limb in water after the dispersion of the heat and pain, and the consequence is the production of engorgement of the joint, a tense state and dark colour of the skin, together sometimes with darkish lines—precursory signs of congelation in fact—on seeing which the joint should be enveloped in a fomentation of elder-flowers and poppy-heads at the temperature of the atmosphere. The objections which have been urged, from the fear of producing *repercussion*, are quite theoretical and unfounded. It is, in fact, only the *excess of morbid caloric* that is abstracted.

*Gum-bandage.*—When the inflammation has been subdued, all the depressions in the vicinity of the joint are filled with wadding, and a bandage carefully and equably applied. This is well moistened, by means of a brush with very thick gum, which in a short time imparts to it almost the hardness of wood. After this has been worn for twenty-five or thirty days, it is removed, and the joint slowly and gradually exercised; for want of which precaution many patients (especially those treated by leeches and poultices) suffer all the symptoms of a sub-inflammation of the white tissues of the joints, even for years.—*Gaz. des Hôp.*, 1850, Nos. 5 and 6.

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*Case in which a large Iron Bar passed through the Head.* By Professor BIGELOW.

THIS truly extraordinary case occurred in the practice of Dr. Harlow, of Cavenish, Vermont, who has already published some account of it. Prof. Bigelow, having since then had an opportunity of examining the patient, now furnishes us with some additional particulars, and with what he very naturally deems requisite, some evidence of the authenticity of the narration.

Phineas Gage, æt. 25, a shrewd intelligent man, of middle stature and good health, was employed as a superior kind of workman in the construction of a line of railway. On the 13th September, 1848, he was engaged in charging a hole in a rock with powder, for the purpose of blasting it, and erroneously believing it to be covered with sand, he let the end of an iron bar fall upon the powder for the purpose of ramming it down, he being at the moment standing leaning over the hole, but with his face momentarily somewhat averted. An explosion followed, and the bar was forced directly upwards, passed *through* his head high into the air, and was found at some rods distance, smeared with blood and brains. It weighs  $13\frac{1}{4}$  lbs., is 3 feet 7 inches in length, and  $1\frac{1}{4}$  in diameter; the end by which it entered having a taper of 7 inches long, the diameter at the point being  $\frac{1}{4}$  of an inch. It entered the cranium opposite the left angle of the lower jaw, behind the zygoma, and emerged on the left side of the centre of the frontal bone, near the sagittal suture. Thrown down at first, the man spoke after a few minutes, and was conveyed to a cart, from which, after sitting erect in it during the three-quarters of a mile ride,

he descended of his own accord, and then, with but little help, mounted a long flight of stairs leading to a piazza, where he sat down, perfectly conscious, until aid could be procured. A surgeon arrived in about half an hour after the accident, and soon afterwards Dr. Harlow joined him. They found a large fracture at the top of the head, which, from the uplifted position of the broken bones, and the protrusion of portions of the brain, gave the idea of the operation of some force from below. A slit-like wound was also observed at the angle of the jaw, and into both wounds the finger could be deeply inserted. The small fragments of the cranium were removed, the larger ones adjusted, and the entire wound simply dressed with adhesive plaster, portions of brain having first come away. The patient during all this time was perfectly conscious and conversible, until he became faint from the great hemorrhage which took place both externally and internally, the latter giving rise to frequent vomiting. This, however, gradually subsided after the wound was dressed. We need not pursue the details of the treatment of the case. It suffices to say, that although during its progress temporary and occasional delirium and coma did occur, the patient eventually was completely restored to health both of mind and body, with the exception of the loss of sight of the left eye.

Dr. Bigelow, and numerous other medical men, at first incredulous respecting the case, have, after investigating the particulars for themselves and carefully examining the patient, fully accorded their testimony to the reality of the nature of the accident assigned. A linear cicatrix, about an inch long, is found near the angle of the jaw, and some degree of thickening of the soft tissues about the malar bone. The left eye is much more prominent than the other, complete ptosis exists, and the eye cannot be moved outwards or upwards. A portion of the cranium behind the forehead, the size of the palm, is elevated, having a deep sulcus behind it, beneath which the pulsations of the brain are perceptible.

Dr. Bigelow passed the bar through the cranium of a subject in the same direction it seems to have taken in the patient, and observes, "That, while a portion of the lateral substance of the brain may have remained intact, the whole central part of the left anterior lobe, and the front of the sphenoidal or middle lobe, must have been lacerated and destroyed. This loss of substance would also lay open the anterior extremity of the left ventricle; and the iron, in emerging from above, must have largely impinged upon the right cerebral lobe, lacerating the falx and the longitudinal sinus."—*Ann. Journ. Méd. Sc.*, N. S., No. 39, pp. 13-39.

[Dr. Bigelow subjoins the evidence of some of the bystanders, which seems to us of very little additional value; but we think, taking all the circumstances and medical testimony into consideration, the case must be received into the archives of medical science for what it professes to be, and therefore as one of the most extraordinary on record.]

*On Necrosis of the Jaw, in Lucifer-Match Manufactories.* By Dr. BAUR.

IN Dr. Baur's opinion, the phosphoric vapours are the immediate cause of the necrosis, and these consist of phosphorous, but chiefly of phosphoric acid. Although the phosphate of lime as it exists in the bones is insoluble in water, by the arrival of an additional quantity of phosphoric acid, which is absorbed, and penetrates into the bones, the basic phosphate of the bones is transformed into an acid phosphate, which is very soluble in water, and deliquescent in the air. In this manner the bone loses its consistence, becomes inflamed, suppurates, and falls into a state of necrosis. As the disease always commences where there are carious teeth, the transformation takes place directly; the vapours coming in contact with these teeth, first affect them, and then the jaw with which they are in connection. This view of the disease is confirmed by Von Bibra's analyses of the bones in this disease, from which it results, that there is a predominance of organic matter, with a deficiency of the salts of lime, especially the phosphate.

Consequent upon this theory, the prophylactic measures are the prevention of

the vapours from gaining access to the teeth by means of a mask, or better still, by a sponge soaked in some feeble alkaline fluid. In the workshops there should be shallow vessels, filled with alkaline fluid, in order to attract the phosphoric vapours. All cavities of the teeth should be filled with a mastic stopping, and the teeth themselves, as well as the face, frequently washed with liquor calcis. As a curative indication, Dr. Baur suggests the restoring the phosphate to its basic condition, by saturating the economy with saline bases, and especially lime.—*Gazette Médicale*, No. 34.

*On Primary and Secondary Amputation.* By Professor RESTELLI.

Dr. RESTELLI, one of the Surgeons-in-Chief of the Sardinian army, in relating the surgical history of the late campaign, confirms the opinion now generally entertained by military surgeons, of the preferability of primary amputation, by the following statistical statement, which, although on a small scale, is very conclusive. He says that whenever an operation becomes recognised as one of undoubted necessity, he believes it the best practice to perform it at once (that is, within thirty-six hours after the injury) in place of waiting the development of fever, inflammation, and suppuration, with the consequent greater liability to phlebitis, purulent infection, tetanus, visceral inflammations, &c., dangers with which the simple wound from amputation is scarcely to be compared. Acting upon these principles, he obtained the following results:

IMMEDIATE AMPUTATIONS.			CONSECUTIVE AMPUTATIONS.		
	Recovered.	Died.		Recovered.	Died.
1 At the hip-joint .	1	0	6 Middle of the thigh .	1	5
7 Middle of the thigh .	5	2	1 At the upper third .	0	1
4 Upper third of thigh .	2	2	1 Upper third of leg .	0	1
3 Upper third of leg .	2	1	7 Upper third of arm .	2	5
10 Upper third of arm .	9	1	2 Shoulder-joint .	1	1
1 Shoulder-joint .	1	0	3 Forearm .	2	1
<hr/> 26	<hr/> 20	<hr/> 6	<hr/> 20	<hr/> 6	<hr/> 14

Thus in the immediate amputations there was a mortality of about 24 per cent. and in the consecutive ones of 70 per cent.; the fatality of these last especially referring to the lower extremities.—*Annali Omedei*, vol. cxxx, p. 242.

*On Hospital Gangrene.* By Professor RESTELLI.

BETWEEN September 1848 and February 1849, hospital gangrene prevailed in the overcrowded Military Hospital of Alessandria to a terrible extent as a complication of wounds, and especially of gun-shot wounds; so that above 400 cases came under Dr. Restelli's care.

Owing to the crowded state of the hospital, no classification of patients could be observed; but this gave the author the opportunity of observing, that individuals suffering from contagious diseases (as syphilis), were more easily attacked by the gangrene; and that it rapidly produced in them dreadful ravages. No general symptoms usually preceded the alteration of character in the wound; and in some cases the disease continued local in its manifestations, but in several others, remittent fever was present, increasing with the progress of the disease. In some cases there were daily paroxysms of fever and profuse sweating, simulating an inter-mittent fever, with which various nervous symptoms, especially those of a convulsive character, were conjoined, and were of very bad omen.

In order to prove the contagious character of the disease, the author, most unjustifiably as we think, inoculated various wounds occurring in different temperaments, with some of the matter, and also inserted it subcutaneously in healthy parts. Other proofs were found in its being communicated from bed to bed during the overcrowding of the hospital, and on its having been propagated in the town by

means of a dirty knife used in an operation. The author, indeed, regards the propagation of the disease as entirely effected by contact, although he allows that the bad state of the air of a crowded hospital increases the virulence of the disease. He does not believe, however, that any contagious principle capable of propagating the disease is absorbed into the system; and thus, when in consequence of the ravages of the disease, operations or amputations were required, the new surface did not take on gangrenous action, providing care were taken to prevent gangrenous matter obtaining access to it. Taking this view of the disease, it is to local treatment he looks for a curative agent, and this may be had recourse to at any stage, if the part retains sufficient reactive power. After trying experiments with numerous caustic substances, as the strong acids, &c., he has come to the conclusion, that the best application is a *solution of caustic potass*, which he considers to act not as a mere caustic, but also by neutralizing the virulence of the poison itself. On the first day, however, he applies pieces of caustic potass in substance to the wound, endeavouring to penetrate into all its sinuosities. Next day the wound is dressed with a solution of  $\mathfrak{z}$ j ad  $\mathfrak{z}$ j of water, and every day the strength of the solution is diminished by four or five grains, to the fifth day, when the wound is simply dressed. The author declares that even the worst cases went on well after this plan was put into practice. In the subsequent treatment of the wounds, absorbent powders, as charcoal, and especially carbonate of magnesia, were of very great service.—*Omedei Annali*, vol. cxxx, p. 266.

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On Dry Gangrene. By Dr. DE MARTINI.

DR. DE MARTINI observes, that the seat and cause of progress of spontaneous dry gangrene have not been sought for by pathological anatomists in the primary elements composing the structure of the affected parts, but in some distant points from the centre of the alterations. He is of opinion, however, that all the tissues of a part suffering from it are the seats of a *minute capillary injection, and a total and primary coagulation of the blood*. The most delicate capillaries are injected, they become obstructed with fibrinous coagula, and a portion of the colouring matter is effused around the anastomotic points. The papillary body loses its moistness and rose-white colour, and becomes a collection of black and hard asperities, rising from a dermis that looks as if it were carbonised. Observed through a microscope, these minute vessels seem as if injected by a substance which has hardened within them; and, in fact, so obstructed are they, that when divided, they do not empty themselves any more than as if they were filled with so much wax. In the adipose layer, muscles, and, indeed, all parts that the gangrene implicates, the same minute capillary injection and coagulation is observable.

In reference to the causes of this complete coagulation in the capillary circulation, the author expresses his opinion, that insufficient attention has been paid to *primary lesion of the capillary innervation*. The acute, burning pain preceding the development of the gangrene, the greater disposition of a paralysed extremity to become attacked, the commencement of the process by a complete arrest and coagulation of the blood in the capillaries, (the circulation in which is under the influence of the ramusculi of the sympathetic,) and the nervous disorders which frequently precede or accompany the development of the affection, lead to the belief that a primary paralysis of the capillary innervation may frequently play an important part in the induction of this gangrene.

In a *chemical* point of view, the metamorphoses which accompany dry gangrene are quite special. In gangrene, the consequence of inflammation, there is rupture of capillaries and effusion of blood, and thence a more intimate combination of its oxygen with the elements of the tissues. The effused blood coagulates, the globules and darkened hematose dissolve, and the whole organic materials of the fluid pass into a state of decomposition, which is communicated to the surrounding tissues. In dry gangrene, coagulation takes place without prior inflammation, and at the various anastomotic points, effusions forming also coagula are likewise observed. No

solution of the blood, no decomposition or disruption of tissues, takes place. The structure of these undergoes little or no alteration. The physical characteristic of *inflammatory gangrene*, is the changing of the organic solids into a soft, pulsatous, decomposing mass; while that of *dry gangrene* is the changing of the humours and soft parts into dry solids. The smell of rancid bacon, which the part emits in dry gangrene, and the readiness with which *mycodermes* are developed on the surface, would seem to show that the process is one of slow combustion—*eremacausis*, a process at first excited by the oxygen of the blood, and when that ceases to be obtainable, by that of the air. A foot suffering from dry gangrene, which had been amputated, was introduced under a bell containing air, together with some pieces of caustic potassa. In three days the whole of the oxygen had become consumed, and the potass converted into carbonate. The process of *eremacausis* could not be set up in organic matter without prior enfeeblement of the vital affinities of its elements, which are dependent upon the peripheric innervation.

From what has been said, it is evident that the organic changes produced in dry gangrene are incurable, for the coagulated blood cannot resume its organic liquid state, nor can the chemical metamorphosis, effected by means of oxygen, be arrested. But this chemical process does not limit its operations to the gangrened part, since the facility of absorbing oxygen possessed by a part in a state of *eremacausis* is communicated to all matters in contact with it, so that the entire body might become involved in this slow combustion, originating in a part already dead. The indication, therefore, is not to lose time in employing internal remedies, which are always useless, but to at once remove the limb beyond the gangrened part, and then resort to means of rational treatment. Does not the *vis medicatrix* of Nature, which preserves the whole body by a spontaneous separation of a part, teach us this lesson?—*Omedei Annali*, vol. cxxvi, pp. 415—423.

*On the Communication of Syphilis to Animals.* By MM. AUZIAS-TURENNE and DE WELZ.

It is well known that the experiments of Hunter, Turnbull, Cullerier, Ricord, and others, have hitherto shown that syphilis is incommunicable to animals by inoculation. M. Auzias, ill-satisfied with the soundness of the conclusion, has been since 1844 engaged in experimenting upon the ape, and has more than once announced the success of his endeavours, though this has been denied by others on examination of his cases. He has, however, succeeded in May of the present year, in inoculating the ear of an ape with matter of syphilis, and M. De Welz transferred the matter from the ear of the ape to his own arm, and produced what MM. Ricord, Velpeau, Vidal, and others, acknowledged to be a genuine chancre there, and from this he reinoculated himself, destroying the chancres afterwards by means of the Vienna paste. The ulcerations in the ape, after reaching a considerable size, soon diminished and cicatrised; the development of the pustule, as well as the other stages of the sore, being far more rapid in the ape than in man. M. De Welz has published the details of the experiment, and believes that the *one positive fact* he thus announces, countervails an innumerable quantity of negative results. M. Ricord, in one of his 'Letters on Syphilis,' now publishing, demurs to this dogma, and will not admit the value of such fact unless it is susceptible of repetition, produced, as it was indeed, after innumerable failures in the hands of M. Auzias and others. He observes, that every form of quackery, whether animal magnetism, homeopathy, or what not, defends itself by the assertion, that a single positive result overthrows a hundred negative ones; but that, in fact, in the physical and natural sciences, an *isolated fact is valueless unless susceptible of repetition*. At present, he adds, all that can be said of this case is, that virulent pus has been transported from man to the ape, with which another man has afterwards been inoculated; but the ape may have served as the mere vehicle of transplantation. The puncture made in the ape scarcely exhibited signs of irritation or inflammation, and suppurated very little (although again and again imbibed in virulent matter), and healed up with surprising rapidity.



There was not that continued increasing ulcerative process so characteristic of syphilitic sores in man, there was not the specific *status quo* which art has such difficulty to combat, nor was there any induration. The syphilitic pus may, in fact, be looked upon as an irritant, just like an issue-pea, but did not combine with the tissues as in man, and give rise to characteristic appearances. To be of any value the pustules should be broken, the ulcerated surfaces well cleansed, and inoculation be then performed with the pus next formed. We may cleanse the syphilitic ulcer as we like in man, and apply all kinds of medicinal agents, but the virulent secretion is still produced; and until analogous procedures have been adopted with the ape, this simple experiment does not destroy the value of the conclusions founded on the repeated investigations of careful observers; all that is at present ascertained, is that we may deposit and preserve virulent pus on the ape, and employ it afterwards for inoculation, just as we would transplant a tree from one soil into another.

M. Cullerier, in a letter upon the same subject, reminds M. Auzias of the impossibility both of them had found, of creating anything in appearance like a syphilitic sore, unless the part were constantly irritated; and he joins M. Ricord in refusing to allow to the present experiment the value of a conclusive demonstration. He believes that the skin of the ape constituted a mere place of deposit of the syphilitic pus which was so frequently transported to it; and declares that he can never consider the inoculation of animals by syphilis proved, until a suppurating ulcer is produced, which after repeated washing, may be propagated on the animal himself or on man by inoculation.

M. Auzias, in a communication in reply, states, that since he has confined his experiments to the ear of the animal, which is a part it cannot lick, and whereon the changes induced are easily perceptible, his success has been very great, and that he has been repeatedly enabled to develop a chancre there, susceptible of communication from animal to animal *ad infinitum*; and he denies the necessity of irritating the punctured part, when placed there, to induce it to take on this character.—*Gaz. Méd.* No. 29, *L'Union Médicale*, Nos. 88, 91, and 94.

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*Surgical Operations, in reference to the Employment of Chloroform.* By M. BOYER.

M. BOYER considers that surgical operations, in their relation to chloroform, may be thus arranged.

1. Those in which it is a powerful auxiliary, facilitating their execution, as in the reduction of dislocations and strangulated hernia.
2. Those which are exceedingly painful, in which chloroform diminishes suffering, but does not render the operation more easily executable. They are very numerous, and this is the only point of view in which the public regards the advantage of anæsthetic agents.
3. Those in which it may become an obstruction or preventive to their execution. Thus, a patient of M. Boyer, on whom he was operating for lithotrity, became so restless under chloroform, that he could not continue, until the anæsthetic influence was withdrawn.
4. Those in which it may prove dangerous, as in cataract. So, too, in tedious and delicate operations performed in the vicinity of important organs; as, for example, ligature of the carotid, during which any indocility of the patient might lead to the wounding of important veins, and in some of the cutting operations in the mouth.
5. Those of a trivial character, in which chloroform is a superfluity, as the slight advantage obtained from its use does not generally compensate for the disagreeable effects produced by the inhalation, such as the smell, headache, and the kind of intoxication that results.
6. There are certain contra-indications to its use in all cases, as acute or chronic affections of the brain, liability to cerebral congestion, great debility, &c. Its employment must, therefore, not be regarded as a mere matter of course; and surgeons, in order to prevent abuse, may often have to discountenance it.—*Gazette des Hôpitaux*, No. 51.

## MIDWIFERY, &amp;c.

*On the Statistics of Ovariectomy.* By Dr. ATLEE.

In the present paper, Dr. Atlee relates two additional cases, in which the large peritoneal section was resorted to; in one of which an unilocular ovarian cyst, weighing 40 lbs., was successfully removed; while in the other the tumour was found to be uterine, and its removal impracticable. Our object in noticing the paper, however, is to record Dr. Atlee's opinion of the propriety of tapping in ovarian dropsy, and the favorable view which the large number of statistical data he has accumulated enables him to take of the operation for removing the ovarian tumour.

He denies the accuracy of Mr. Safford Lee's statement of the danger of *tapping*, and especially (according to Dr. Meigs) the first tapping. According to Mr. Lee's own tables, of 123 cases treated in the ordinary way, 63 died in two years, and 90 within four years—leaving only 33, or not quite 1 in 4. Thus 51 per cent. died in two years, 73 per cent. in four years, and only 27 per cent. survived this period. If we examine the table of the mortality after a first tapping, in the 40 cases in which the whole duration of the disease is stated, it is found that 15 (or 39 per cent.) died in two years, 11 within four years (65 per cent.), while 14 (35 per cent.) lived beyond that time, that is nearly 30 per cent. in favour of tapping. Both Mr. Lee and Dr. Meigs employ the loose phraseology, that the women died *after* the tapping, as must necessarily be the case; but the true question is, how far has the tapping been the cause of death. The experience that Dr. Atlee and his brother have had of this operation since 1823, and the numerous inquiries he has made of surgeons in large practice, convince him that death or even serious symptoms never result from tapping, that life is usually prolonged, not curtailed, by resorting to it, while in several cases even permanent recovery has resulted.

*Statistics of Ovariectomy.*—Dr. Atlee has just cause for finding fault with Mr. Safford Lee in this matter, for it seems the statistics he published in his work, and which have been so frequently quoted, were taken, without acknowledgment, from a paper prepared, with great labour, by Dr. Atlee, and published in the 'American Journal' for 1845. Mr. Lee has since apologized for an omission, which, occurring in a work supposed to be so peculiarly composed of an author's own materials as a prize essay, was a very culpable one. That table referred to 101 cases, but Mr. Atlee has since extended it so as to embrace the cases which have since occurred—the entire number now amounting to 179. We will state some of the results which flow from the analysis of this large body of facts.

Of the 179 cases, 120 *recovered*, and 59 *died*, viz. :

Operation.		Recovered.	Died.
Minor Section.....	28	20	8
Major Section.....	133	87	46
Not stated .....	18	13	5

In 34 instances the operation was *unfinished*, the patient recovering in 24, and dying in 10 of the cases; and in 6 instances *no tumour* was found, the patient recovering in 4, dying in 2, viz. :

Operation.		Recovered.	Died.
Minor Section.....	9	5	4
Major Section...	24	17	7
Not stated .....	7	6	1

In 17 instances other *important diseases co-existed*. In 4 the operation was unfinished, and all the patients recovered; of the 13 in which it was completed, only 1 recovered. In 14 cases the major operation, and in 2 the minor, were

resorted to. In 62 cases, *adhesions* are stated to have been present, recovery occurring in 36, death in 26.

The *cause of death* is only stated in 38 of the 59 fatal cases. In 12 it arose from hæmorrhage, in 14 from peritonitis, in 3 from gangrene, in 3 from exhaustion, and in 2 from shock. One person is stated to have died from each of the following causes: inflammation of mucous membrane of the intestines, pneumonia, ileus and phlebitis, and from an accident during convalescence.

In considering the *ratio of mortality*, it is to be observed, that of the 17 cases stated above as complicated with other important diseases, 7 were obviously improper cases for the operation, and 8 instead of 4 ought to have been left unfinished after the section was begun. Abstracting the 7 cases, we have 172 legitimate ones, and, rating the 4 others at the mortality of unfinished operations, we have 123 recoveries and 49 deaths, or 1 in  $3\frac{1}{2}$ , or 28 $\frac{1}{2}$  per cent. Moreover, in one case death occurred on the seventieth day, in 2 after six weeks, and in another from a fall—all of which should be considered as recoveries from the operation, which would give us 45 deaths to 127 recoveries, or 26 $\frac{2}{7}$  per cent. The rate of mortality has diminished since 1845, when it was 1 death in  $2\frac{1}{2}$  cases, or 37·62 per cent. In the subsequent 78 cases there was 1 death in  $3\frac{1}{4}$  cases, or 26·92 per cent.; *i. e.* a diminution of nearly 40 per cent. There has been also a diminution of unfinished operations, which have, too, been mostly of an exploratory character. No new case has occurred, in which the abdomen has been opened and no tumour found.

The above statistics, compared with those of other operations, are favorable to gastrotomy. Thus, while the mortality arising from operations on the larger arteries, as stated by Norris, amounts to 33·45 per cent., that from the whole number of ovariectomy cases is but 32·96; and if the more recent cases be alone counted, the arterial operations furnish a higher mortality than they do, by 25 per cent. According to Malgaigne, the mortality of all the 852 amputations performed in the Paris hospitals, 1836-41, was 38·97 per cent.; of amputation of the thigh alone, 62·68 per cent.; of the leg, 55·21 per cent.; and of the arm, 49 per cent.;—shewing a vast preponderance in favour of the recent ovarian operations.—*American Journal of Medical Sciences*, N.S., No. 38, pp. 318-336.

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*On a Stethoscopic Sign of the Detachment of the Placenta.* By M. CAILLAULT.

M. CAILLAULT states, that repeated observation has shown him, that, at the moment of the detachment of the placenta from the uterus, a peculiar sound is produced, feeble at first, increasing in intensity in proportion as the uterine contraction becomes more energetic, and then becoming less audible, until it disappears. He says it consists of a small cracking sound, very frequently repeated, and which may be roughly compared with the noise produced by running the nails over a straw-bottomed chair.—*L'Union Médicale*, No. 81.

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*On the Cæsarean Section.* By Professor CHRESTIEN.

PROFESSOR CHRESTIEN expresses great regret, that death has deprived the Cæsarean operation of one of its ablest champions in the person of Capuron; and still maintains the doctrine of its preferability to the destruction of the child in narrow pelvis. He states, that since 1839, there have been thirty-three such operations performed, with the effect of saving the lives of twenty-six mothers and twenty children. The notes he furnishes of these various cases are too brief to admit of critical examination; but as he supplies the references to most of them, it may prove useful to transcribe these; for general as has become the preference of embryotomy to the section, the question cannot yet be considered by any means as decided.

Cases are described by Lestiboudois and Toché in the *Journ. de Connaiss. Méd.*, Jan. 1839; and others are found in the 12th and 14th year of that journal, and in the vol. for 1848, p. 17; Hæbeke, *Ann. de la Soc. de Méd. de Gand*, 1839. Godefroy

and Ling, in *Gazette Médicale*, 1840, p. 444; 1845, p. 583; Stracke, Berndt, and Schnakenberg, in the 8th, 9th, and 14th vols. of *Neue Zeitschrift für Geburtshunde*; Kilian and Merrem in *Annales d'Obstétrique*, tom. ii and iii; Monin, *Journ. de Méd. de Lyon*, Feb. 1843; Pitre-Aubinais, *Journ. de la Société de la Loire Inf.*, 1844; Mestenhauer *Oesterr. Med. Wochens.*, 1844; Lebleu, Bach, Steinbrenner, and Runsemüller, in *Gaz. Méd. de Strasbourg*, 1845 and 1846; Guisard, *Bullet. de l'Académie*, April 1849.—*Bulletin de Thérapeutique*, tom. xxxviii, p. 506.

*A Case of Rupture of the Varicose Veins of the Labium Pudendi.* By Dr. THOMPSON.

THIS patient was in the ninth month of her pregnancy, and, while engaged in household duties on the 5th January, felt a large tumour, which she thought must be the child's head, bearing down. Placing her hand below, she felt a tearing sensation, and she became deluged in blood, so that the reporter believes she lost considerably more than a gallon. Upon examination, he found a laceration on the left of the median line, extending from within an inch of the anterior commissure of the labia to the upper part of the *mons veneris*, about three inches in length; the soft parts on that side being greatly distended by the effusion of blood into the cellular tissue. The patient being in the recumbent posture, the hemorrhage ceased, the varicose veins of the extremity having become completely emptied, and the heart scarcely pulsating. As the bleeding had stopped, the case was left to nature. After the first week, camphor liniment was employed to disperse the large swelling produced by the extravasated blood, which entirely disappeared before the end of the second, so that the edges of the wound, which were at first three inches apart, were now in close contact. On the 20th January the fœtus was suddenly and easily expelled, and the case did well.—*New York Journ. of Med.*, N. S., vol. iv., p. 317.

*A Case of Lactation in a Male.* By Dr. C. HORNER, Philadelphia.

THIS case occurred in the person of an athletic New York blacksmith, æt. 22. His attention was drawn to his left breast about the 10th of February, in consequence of its size, and this had continued to augment for three weeks, when he arrived at Philadelphia; and, in the course of another three weeks' residence there, it had become as large as that of a female when nursing, being, however, accompanied by very little pain. Professor Mütter saw him, and found the mammary gland largely developed, and filled with lacteal secretion, which in no wise differed from that of a nursing female. He could assign no reason for it; his health was good, and the other mamma natural. A soap plaster, with compression, was ordered; and, after persisting in the use of this means for six weeks, the gland returned to its natural size, and became in every respect like the other.—*Phil. Med. Exam.*, N. S., vol. vi, p. 455.

*On the Eclampsia of Children.* By Dr. OZANAM.

DR. OZANAM terminates an elaborate paper upon this subject, with the following conclusions:—1. Eclampsia is quite a distinct disease from all other convulsive affections, and especially sympathetic convulsions. 2. It is characterised by chronic convulsions, coming on suddenly, in a greater or less number of paroxysms, from two to ten minutes in duration, followed by coma without stertor, and a tonic contraction of the limbs. 3. A burning heat of skin, and distension of the abdomen, almost always accompany eclampsia, and serve to distinguish it. 4. When in a child, who seems well, the pulse becomes suddenly, and without apparent cause, very rapid, an attack of eclampsia is imminent, especially if the belly be distended and the skin hot. 5. A persistence of frequency of the pulse after the subsidence of a paroxysm, indicates its quick repetition, while, when the pulse becomes natural, it is a sign of complete cessation. 6. A coma of several hours' duration, supervening on a paroxysm, announces the existence of serous effusions in the ventricles.

7. The respiration in this affection is doubly pathognomonic. During the *convulsive* period it is difficult, rapid, and accompanied at the moment of expiration by a short, interrupted cry; while, during the *coma*, it is slow and deep, and terminates with prolonged and plaintive sighs. 8. The hemiplegia which supervenes indicates the occurrence of a cerebral or meningeal hemorrhage. 9. The intra-arachnoid hemorrhages are the most common, and are followed, in young children, with a dilatation of the side of the head (capable of mensuration) opposite to that on which the hemiplegia occurs. 10. The serous cyst surrounding the blood has frequently been mistaken for the arachnoid, or dura mater. 11. The hemiplegia affects movement rather than sensibility, the limbs oftener than the face, and diminishes much with age, though it is rare for it to cease entirely. 12. It may be limited to a single limb, or to certain muscles, as the extensors of the fingers, or the feet, the upper eyelid, &c. The paralysis of the extensors of the foot induces the predominance of the flexors, the curvature of the limb, and consecutive club-foot. 13. An arrest of development takes place on the paralysed side, especially as regards the leg rather than the arm. 14. As a consequence of eclampsia also supervene the more or less persistent contractions of the muscles, which frequently give rise to torticollis, club-foot, spinal distortion, as first shown by Guérin. 15. The existence of these club feet prior to birth, their cure, and their return after the eclampsia, lead to the belief that eclampsia may attack the child *in utero*. 16. It is the same with meningeal hemorrhages observed in the fœtus without any traces of external violence. 17. Eclampsia differs from *epilepsy* because, (1) the convulsions are at first tonic, and terminate by a tonic contraction—being the reverse of what occurs in epilepsy; (2) the duration of the paroxysm is longer, and the *aura* is absent; (3) the peculiarity of the respiration, and absence of stertor; (4) the frequency of pulse, the meningeal hemorrhages, the invariable cessation of the convulsive attacks at the end of some years, or, at the latest, about the period of puberty. 18. Eclampsia differs from *symptomatic* or *sympathetic convulsions*, because, (1) the spasmodic movements are much more marked on one side than the other, and generally implicate the entire half of the body; (2) the paroxysm is never transformed into, or alternated with, another form of convulsion, as tremor; (3) eclampsia induces repeated paroxysms, and not continuous convulsions; (4) the peculiar characters of the respiration; (5) eclampsia arrests or modifies diseases in the course of which it happens to occur, and has its own course arrested by the supervention of a new disease.—*Archives Générales*, tom. xxiii, p. 177.

*On Fungous Tumour of the Rectum in Children, attended with Bloody Discharges.*

By M. LECLAYSE.

M. MARTIN has already directed attention to the affection as it occurs in the adult, producing discharges which are mistaken for those from hæmorrhoids. The first case occurred in a child *æt.* 5, about whom the author was consulted in consequence of hemorrhages which occurred during a prolapsus ani, and which arose from an excrescence that he at first mistook for hæmorrhoids. Examining it more closely, he found it was a spongy vegetation, not unlike a portion of the placenta, which protruded from beyond the sphincter when the child went to stool, and was quite insensible to the touch. As the hæmorrhage had been considerable, the fungus was touched with the nitrate of silver, whenever it protruded; and owing to its softness, four or five applications, at intervals of several hours, sufficed for its destruction. In a second case, a girl *æt.* 8, had become much reduced by the quantity of blood she had lost during several weeks; and a fungous tumour, about the size of an almond, was easily removed in the same way. A third case occurred in an infant six months old, in whom efforts at stool protruded a tumour the size of a pea, which bled. The author believing it to be the germ of the fungous tumour, also treated it with caustic.

M. Leclayse believes that this affection is often mistaken for hæmorrhoids; and especially when the bleedings are said to be due to internal piles. The caustic

could not be applied very high up, but as the bleeding has only occurred on the protrusion of the tumour, this has been easily reached, the application being successful even when the base of the tumour could not be attained.—*Rev. Médico-Chirurgicale*, tom. vii, p. 346.

## MATERIA MEDICA AND THERAPEUTICS.

### *On Cod-liver Oil in Phthisis.* By M. DUCLOS.

M. DUCLOS thus sums up the results of his experience with this substance: 1. The presence of fever is what we must chiefly attend to, relying more on this remedy when it is absent, and less when it is present. 2. The remedy frequently arrests the progress of the disease when only in the first stage. 3. It rarely arrests it when in the second stage, although it may retard it. 4. The third stage is not favorably influenced by the oil. 5. The oil should be administered for a considerable time; and, if a good effect results, it should be suspended awhile, to be again resumed. Thus, it may be given for two months, and then suspended for a fortnight, resumed for a month, and re-suspended for a fortnight again, so as gradually to reduce the length of the intervals during which it is given. 6. The clear, slightly smelling, nearly tasteless oil, is less efficacious than the brown, thick, strong oil.—*Bull. de Thérapeutique*, xxxviii, p. 490.

### *On Linseed Oil in Hæmorrhoids.* By M. VAN RYN.

M. VAN RYN believes, that, in general, surgical treatment is too hastily resorted to in this affection, and he wishes to bring under the notice of the profession a remedy he has found of great efficacy during twenty-five years. It consists in the administration of two ounces of fresh linseed oil morning and evening; and so rapid is the amendment generally, that the remedy is seldom continued longer than a week. Sometimes the stools are somewhat increased in quantity, but neither vomiting nor any other ill effect is produced. The only precaution the while, is the abstinence from alcoholic drinks and too stimulating a diet.—*L'Union Médicale*, No. 69.

### *On the Effects of a Bread and Milk Diet on a supposed Malignant Tumour of Eight Years standing.* By Dr. BOWDITCH.

THIS case occurred in the person of Dr. Twitchell, one of the most celebrated medical practitioners in New England. Carcinoma had prevailed in his family, and during his studentship he had suffered first from dyspepsia, and then from severe asthma, which entirely left him after he had abandoned the use of meat, and confined himself to a vegetable diet on account of an acne with which he was troubled. After nine years of such abstinence, he gradually resumed the moderate use of meat. The local disease in question commenced about eight or ten years since, as a small hard tumour at the inner angle of the eye, which slowly but gradually enlarged so as to become very conspicuous. The greater part was excised, but the wound did not heal, and another operation, followed by the use of nitrate of silver, was performed. Eventually the ulcer which resulted assumed a decidedly malignant aspect, and the general opinion of the eminent men he consulted at the meeting of the Medical Association at Philadelphia, in May, 1847, was that the disease was of a very serious character. A variety of local applications having been tried, Dr. Twitchell, starting from the theory that malignant disease arose from the taking too much carbon into the system, resolved to limit himself to a bread and milk diet, and to this he henceforth strictly adhered. He took from four to six ounces of cream, or the richest milk, and as much white or brown bread, three times daily. The immediate results were a cessation of the pain, a diminution of the discharge.

and an arrest of the spread of the ulcer—this eventually diminishing, and then altogether disappearing. When Dr. Bowditch saw him, August, 1849, no difference could be discerned in the angles of the two eyes, unless by a person already aware of the former existence of the ulcer, who would then perceive a small soft cicatrix. A perfect cure of what all thought to be a malignant disease of ten years' duration had thus been accomplished by this diet, commenced when the patient was 68 years of age. Under its influence too, Dr. Twitchell found himself less irritable than heretofore, while, on account of a tendency to corpulence under its use, he was at one time obliged to reduce the quantity. His strength continued as great, though having a large practice to attend to; his digestion good, and his respiration more free. To the eye he had the appearance of a hale, robust man, in perfect health.—*Ann. Journ. Med. Sc.*, N. S., No. 39, p. 269.

[By an obituary notice in the same journal, we perceive that Dr. Twitchell died in May, 1850; but, as that notice is contributed by another hand, there is no account of his health since Dr. Bowditch's communication in November, 1849, nor is the cause of his death stated.]

#### *Vegetable and Animal Parasites of the Teeth.* By Dr. BOWDITCH.

DR. BOWDITCH has recently made microscopic observations upon forty-nine persons taken from different classes of the community, and none of them having any disease of the mouth. In all, except two, he found animal or vegetable parasites between the teeth, or at their junction with the gums. Such parasites have hitherto been attributed to a state of disease, but Dr. Bowditch regards them as entirely the result of want of cleanliness. The only means of avoiding them is the thoroughly brushing the teeth after each meal, and even this will not suffice if there be decayed or false teeth. The two individuals who did not exhibit the parasites, cleaned their teeth four times a day, and frequently "threaded" them. Dr. Bowditch has found that soap, soda, ammonia, and the "chlorine tooth wash," speedily destroys them, but that infusions of cinchona or myrrh do not affect them. Even strong infusion of tobacco does not affect them, and they are indeed found most abundantly in smokers and chewers.—*Amer. Journ. Med. Sc.*, N.S., No. 38, p. 363.

[As we fear that few persons will be found willing to effect the radical cure of this evil by cleaning and threading (a very questionable procedure) their teeth four times a day, it is desirable to be made aware of the best means of mitigating it. M. Foy, a French dentist, after strongly animadverting upon the employment of pulverulent dentifrices of any description, states he has used the following liquids, one with great advantage during forty years:—*Tr. Kino, Tr. Catechu*, aa a teaspoonful, to be added to cold or tepid water, and used every morning. A more complex and more aromatic one is the following, which he also strongly recommends. In a litre (1½ pint) of alcohol, at 36°, macerate essent. oil of mint; powder of cloves and canella, of each 2 drachms; green anise, 8 drachms; powder of catechu and of kino, of each 16 drachms; camphor, 1 drachm; and musk, ½ grain.]

#### *On Liquor Ammoniac.* By M. TEISSIER.

M. TEISSIER considers that this substance has fallen into most unmerited neglect in modern times, and thus sums up his view of its utility: 1. It may be administered profitably in diseases produced by the emanations of tobacco. 2. It is very useful in combating the accidents, even the remote ones, which ensue on the prolonged abuse of alcoholic drinks, and especially those characterised by disturbance of the nervous system. It is a mistake to regard ammonia as serviceable only in temporary intoxication. It may be of great utility in consequent permanent lesions, as e. g. in amblyopia. 3. Its powers are not merely stimulant and sudorific; for these would not explain its success in various diseases, as pertussis, some of the neuroses, repercussion of the exanthemata, envenomed stings, and poisonings. 4. It possesses greater antidotal powers than are ascribed to it; so that it may properly be regarded as an *alexipharmic*. Its antidotal powers explain its success, not only in

many cases of narcotic and narcotico-acrid poisons, but also in various diseases, in which there are injurious principles to neutralise or eliminate, such as reperculated exanthemata, malignant fevers, chronic rheumatism, poisoned wounds, and the pains preceding dysmenorrhœa. 5. The doses usually recommended in works on materia medica are too large. Ten or fifteen drops *per diem* suffice, and large doses engender hemorrhages and cachectic debility.—*Bulletin de Théraputique*, tom. xxxix, p. 64.

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*On Cauterizing the Ear in Sciatica.*

M. LUCCIANA states this is a common practice in Corsica, the operation being usually performed by farriers, who apply a red hot iron to the helix of the ear of the same side as that affected with the sciatica. The cure usually takes place in about a week, but in some cases instantly; but it is very rare for the application to fail, and if it does so, a second succeeds. Several such cases are referred to, and guaranteed by M. Lucciana, who states, that when a cure was not at once effected, considerable relief always followed, and, especially, a good night's rest was obtained. Some of these cases, too, were of old standing, and others continued for some time under observation after treatment.

M. Malgaigne tried this remedy upon a patient who suffered excessive pain, and required the use of crutches. It acted upon him as if by enchantment, and he was at once able to walk, but was detained in the hospital a few days in order to see if the improvement continued, which it did. In two out-patients, in whom the disease was uncomplicated and well marked, the relief was very striking, so that they returned home utterly astonished. Since that period it has been tried at St. Louis in sixteen or eighteen cases. In about one third, a complete cure resulted; in another third the pain was at once relieved, but returned in from two to twenty-four hours; while in the remainder, not even temporary benefit accrued. Thus it is not always an efficacious means, but it is one of the most powerful we possess, and when it does succeed, its effects are surprisingly rapid.—*Rev. Médico-Chirurgicale*, tom. vii, pp. 358 and 365; viii, p. 53.

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*On the Application of Alum in Acute Tonsillitis.* By M. MERTENS.

ALUM has been frequently recommended as a good application in acute tonsillitis, and M. Mertens suggests a plan for its employment, in young children, far more efficacious than insufflation. Having previously moistened the pulp of the index finger, and covered it with alum, he carries this down to the inner surface of the tonsil and there applies it. He places the ring and middle finger between the lips of the child; and when it opens its mouth, advances them to the base of the tongue, and is thus enabled easily to reach the tonsil with the index finger.

The same plan may be resorted to by adults themselves, in chronic tonsillitis for example; and they should be instructed first to pass the finger down to the tonsil, and observe, by the saliva which adheres to it, what part comes in contact with the tonsil, and then to cover that with the alum; and it is surprising with what facility the tonsil, and the tonsil alone, can then be touched.—*Gazette Médicale*, No. 23.

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*On the Action of Bromide of Potassium.* By M. HUETTE.

M. HUETTE has been induced, by reason of the analogy in composition which this substance offers to iodide of potassium, and by the recommendation by a few practitioners of its therapeutical employment, to try a series of experiments with it. These have resulted in complete disappointment; but during his investigation he discovered two effects producible by the bromide, which, if confirmed on farther trials, may admit of useful application. One of these is the power it possesses, even when given in small doses, of inducing a state of *insensibility of the palate and pharynx*, which, commencing on the second day, continues during the whole course of



treatment. It is so complete, that the finger may be carried to the base of the tongue, touch the amygdalæ and posterior nares, or tickle the uvula, without inducing any effort at vomiting or deglutition whatever. This local anæsthesia seems deserving of investigation, as being preferable to that derivable from chloroform, when tedious and delicate operations about the mouth and throat are in question. Still other investigations are required; for, on the one hand, the insensibility might not subsist under the action of a cutting instrument, and on the other perhaps the glottis itself is involved in a like insensibility, and would not indicate the passage of blood into the trachea.

The bromide possesses also remarkable power in inducing *torpidity of the genital organs*. A patient tormented by a vivid imagination, and subject to frequent consequent pollutions, found himself quite freed from his infirmity after having taken 15 grains *per diem* for three days; while some patients to whom the drug was administered reproached the author with this effect, which however passes off in a few days after the discontinuance of the medicine. The medicine thus seems indicated in *chordee*, in relieving which camphor and opium so often fail, as also in certain forms of spermatorrhœa.—*Gazette Médicale*, No. 23.

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*On the use of Iron in Anæmia from Starvation.* By M. BRICHETEAU.

THE employment of iron in chlorosis has perhaps been carried to an excess, since it only acts as a palliative, and other indications have been too much lost sight of. In anæmia, properly so called, essential anæmia, arising from prolonged grief, deficient food, &c. an excess of another kind has been fallen into, viz. the endeavouring to force too large a quantity of nutritive substances into the system. These are not supported, their imperfect digestion fatigues the stomach, and an entire disgust for food may thus be generated, and the patient's cure prevented. A gilder had for some time suffered from domestic affliction which preyed deep upon him, when the February revolution destroyed his means of living, and left him destitute. He lived upon small quantities of potatoe and water, and *meanwhile* became the subject of mercurial tremor. This last left him, but he suffered from dyspnœa, palpitation, and œdema, while his skin became as pallid as that of a corpse. On his admission into La Charité plentiful supplies of roast meat were ordered him, as starvation had reduced him to his present state. These the patient at first took with avidity, but only for a short time; a diminished appetite soon being followed by disgust for food, so that he left the hospital worse than he entered it. Coming under M. Bricheteau's care some time after at the Necker, he presented a most marked specimen of advanced anæmia. M. Bricheteau resolved to pursue the course with him that Hoffinguer had found so advantageous during an epidemic among the miners at Schemnitz, and prescribed ferruginous medicines, with as good diet as the patient could take. A complete renovation was rapidly effected, so that in a month the patient had recovered his spirits, strength, and good looks.—*Bulletin de Thérapeutique*, vol. xxxix, p. 34.

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*New Test for Quinine.*

M. VOGEL, jun., has discovered a new test for quinine. To a solution of quinine, in alcohol or water, some drops of liquid chlorine are first added, and then a *concentrated* solution of ferro-cyanide of potassium, when the liquid assumes a beautiful bright red colour. If the solution of the ferro-cyanide has not been enough concentrated, the same effect is produced by adding some drops of ammonia. For examination of quinine in a dry state, a small quantity of the body is placed in a watch-glass, and, while it is stirred with a glass rod, a few drops of chlorine are first added, and then the ferro-cyanide. A beautiful red is produced, changing presently to a green.—*Bull. de Thérapeutique*, vol. xxxix, p. 70.

*On the Therapeutical Employment of Coffee and Caffeine.* By MM. VANDEN-CORPUT and HANNON.

M. VANDEN-CORPUT has recently published an article upon the febrifuge power of coffee, and especially its antineuralgic action, on which account it is now very much employed by the Belgian practitioners. Numerous therapeutical applications of this substance were made long since. Nebelius and Baglivi gave it in cephalalgia, Dufour prescribed it in phthisis and migraine. Willis employed it in narcotic poisoning; and Grindel and Dorpat as a febrifuge. Musgrave, Pringle, Monin, Percival, Laennec, and a great many others, have spoken of it approvingly in essential asthma. In Dutch Batavia it is used in strong infusion, with lemon-juice, in pernicious fevers; and the practice passing thence to Holland, has led to its being preferred there to quinine. Pouqueville declares it is infallible in the intermittents of the Morea; and Martin-Solon approves of its use in the adynamic form of typhoid. Dr. Guyot has recently strongly recommended it in pertussis. Besides medicinal properties, properly so called, it possesses the important one of disguising the taste of various substances, especially quinine, sulph. magnesia, and senna; and if its antiperiodic virtues really exist, it will probably favour the action of quinine instead of impairing it, as has been feared by some. It possesses the power, too, of developing the action of *haschisch*, contradictory as this may seem to its generally acknowledged antinarcotic properties.

With *Caffeine*, prepared according to Liebig's process, M. Vanden-Corput produces various compounds. Thus the *citrate*, which is very soluble in water, is formed by saturating pure caffeine in a solution of citric acid, and evaporating; or it may be procured by exhausting crude coffee with a very weak solution of citric acid, shaking the liquor with an equal volume of ether, decanting it, and leaving it to crystallize after concentration. Twelve grains are added to 450 grains of sugar, and the mixture given in doses of 12 grains. The *lactate* may be prepared by dissolving caffeine in dilute lactic acid and evaporating, or by treating the infusion of green coffee with lactate of lime, filtering and evaporating. It may be given in sugar. The *malate* may be prepared in an analogous manner, and administered as a syrup by dissolving 4 parts of the malate in 30 of orange-flower water, and adding to it 250 of simple syrup. Caffeine may also be given with *hydrochloric acid*, as in the following formula:—Caffeine 7 grains, distilled water 1350 grains, strong hydrochloric acid 2 drops, syrup 225 grains. Dose, a table-spoonful.

M. Hannon speaks on the highest terms of the employment of the *citrate of caffeine* in *idiopathic migraine*. Ten grains are first made into as many pills, one of which is given every hour for some time before the paroxysm. The dose is gradually increased until relief is obtained; and in one case even half a drachm at a time was given. The dose must, indeed, be large, in proportion to the obstinacy of the case and the length of time between the paroxysms. Large doses are also required in old, feeble, or cachectic patients; and in old cases the medicine must be long continued. It is desirable, when possible, to commence the medicine the evening before the expected paroxysm, when the entire quantity may be divided into several doses; but if it has been delayed until the commencement of the paroxysm, the whole quantity must then be given at once. The expected paroxysm may thus be entirely arrested or merely diminished in severity; but in all cases, save where the disease is sympathetic, it eventually yields.

While upon the subject of *migraine*, we may mention a still pleasanter remedy than coffee, suggested by M. Tavnignot, viz., the making several *deep inspirations* in rapid succession. We must observe, however, that M. Tavnignot does not understand by the term *migraine*, simple neuralgia of the head, which many writers so designate; but the condition when this is accompanied by a state of physical and moral prostration, during which the blackest ideas assails the patient,—the “blue-devils,” in fact, to which the English were once thought on the continent to be especially liable. During an attack of this, which from former experience he was led to believe would continue for twenty-four hours, he was induced by the hope that this condition of the nervous centres might result from a stasis of the blood in

the sinuses of the brain, or from imperfect hæmatois, to take several deep and rapid inspirations; and after a few efforts of this kind, he found himself completely relieved, and able to resume his occupations. Other persons, similarly affected, have been in like manner relieved; but those who have tried the plan in simple neuralgia, have been disappointed.—*Bulletin de Thérapeutique*, vol. xxxviii, p. 498, and *Rev. Médico-Chirurgicale*, vol. vii, p. 350.

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## INTELLIGENCE.

### EXECUTION OF PROFESSOR WEBSTER.

THE execution of Professor Webster took place at Boston on the 30th of August. We are informed, that some time previously he had made a *second* confession, in which he acknowledged the premeditation of the murder of Dr. Parkman; but we have not been able to obtain a copy of the document. The report of the last awful scene, however, contains the statement, that he "admitted the justice of his sentence;" and this may be considered as tantamount to a confession of premeditation, since he had previously protested against the punishment, as having been based on a mere legal presumption. (p. 453.)

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### *Letter from Dr. Jacob, of Dublin, to the Editor of the 'British and Foreign Medical-Chirurgical Review.'*

[We give insertion to the following letter, in order to afford Dr. Jacob an opportunity of setting himself right with our readers, if he can; the acknowledged eminence of Dr. Jacob as an ophthalmologist, and the tone and temper of his letter, having induced us to depart from our ordinary practice in his favour.]

"SIR,—If it comes within your editorial rules, will you have the goodness to allow me to offer a few explanations as to some points raised in a review of my book on 'The Inflammations of the Eye-ball' in your last Number. I do not make the unreasonable request to be permitted to publish what is called a 'reclamation' in your pages; I only ask for an opportunity of suggesting an inquiry as to some anatomical and physiological doctrines questioned in my treatise.

"(1.) The reviewer very fairly remarks upon an observation of mine, questioning the existence of coagulable lymph in all cases of adhesion of serous membranes, that 'this doctrine of adhesion is quite at variance with that which is universally received; namely, that in all cases where two naturally separate tissues are united by inflammation, a material susceptible of organisation is effused, and becomes the connecting medium.' I am, of course, aware that this doctrine of adhesion is at variance with that which is universally received; but still I venture to propound it, because I have long believed that the doctrine of coagulable lymph being essential to adhesion is not so unquestionable as many suppose; and because I believe that the agency of coagulable lymph in producing opacities has been overrated. I do not, of course, deny that adhesions generally take place by the organisation of coagulable lymph, or that opacities of transparent parts often arise from effusion of this material; but I do deny that evidence is forthcoming to prove that adhesion or opacity never occur without it. I was led to the conclusion that adhesion thus takes place between inflamed serous membranes, from repeated observations of such in the bodies of persons who had died from peritonitis; and this conclusion was confirmed by observations made in cases of iritis, where the inflamed surfaces are seen under the most favorable circumstances. The truth is, that were it not for preconceived theory, we should find little difficulty in admitting the possibility at least of adhesion without lymph. Serous membranes do not adhere in their natural or ordinary state; but, when inflamed, they cease to be serous membranes, and become surfaces apt for adhesion. In recent adhesions of the margin of the

pupil, no lymph can be seen with the aid of a lens of the shortest focus available in making such observations. I repeat, that I do not deny that coagulable lymph is often effused in inflammation of the eye; but I do deny that it is in all cases, or even in many cases, the bond of union in adherent pupil.

"(2.) From some confusion, I suppose, in my method of expression, the reviewer assumes that I defer the use of mercury until the period of adhesion, effusion, and loss of transparency arrives; but I do not. This period so rapidly follows the period of mere increased vascularity and turgescence, that no time intervenes for a pause in the treatment. As soon as the contents of the intestines are removed, and often without removing them, I commence with mercury, if I consider mercury essential; and even in doubtful cases I administer it at once, by way of foundation for a full course, if such be found necessary, or to be dropped if found unnecessary.

"(3.) The correctness of the view I venture to entertain respecting the contiguity of the margin of the pupil to the capsule of the lens, is called in question. It seems I referred to my argument in favour of it, as if it was to be found in my paper in the 'Medico-Chirurgical Transactions;' but it is in my article on the anatomy of the eye in the 'Cyclopædia of Anatomy' that it occurs. Of the anatomical fact I entertain no doubt. In some eyes, at least, if not in most, when the lens is of full size and correct curvature, it touches the margin of the contracted pupil. In advanced life, when the lens becomes more flat and of longer focus, it does not; and perhaps it does not in the eyes of long-sighted persons, who probably have lenses of the same description.

"(4.) With respect to the distribution of the ciliary nerves to the iris, I am entirely at issue with the reviewer. He hints, in reference to my observation that 'there is not, perhaps, in the whole body, a part which receives so great a quantity of nerves in proportion to its size,' as the iris, that I have been led astray by Zinn's coarse plate; but he is mistaken. I could show him one or two very beautiful drawings of the ciliary nerves and *orbiculus ciliaris* done from my own dissections very carefully made, as well as the magnified copy of the same which I have used in my lectures for many years. What I say of the iris is, I believe, strictly correct. It is an organ endowed with a conspicuous power of motion, and it is exquisitely sensible, as the touch of the needle in operation proves. It must, therefore, have nerves of sensation and motion, visible or invisible, and these nerves must be derived from the ciliary nerves distributed in the *orbiculus ciliaris*. Whether any of these nerves are lost in this *orbiculus ciliaris*, I do not venture to decide; but that the fine subdivisions of them can be traced with a fine needle up to the margin of the iris, I am certain. As to the notion entertained by some anatomists of the present day, of the muscularity of this structure, the *orbiculus ciliaris*, I must, with all due respect for the opinions of others, express my emphatic dissent. An anatomical fact of so much importance, and involving such important considerations respecting the adaptation of the eye to distance, must be established on much better evidence than any yet advanced.

"(5.) The opinion I entertain respecting the anatomical nature of the membrane of the aqueous humour, your reviewer says, 'cannot be entertained.' It cannot, he says, be considered a serous membrane, because 'the several surfaces forming the parietes of the aqueous chamber are anatomically dissimilar.' Yet the surfaces covered by the peritonæum are dissimilar, being muscular, tendinous, hepatic, splenic, and so on. He also says, it cannot be a serous membrane, because part of it does not present a 'tesselated epithelium,' but I cannot accept this microscopic test as at all conclusive. It is after all, perhaps, only a dispute about names. If the chamber of the aqueous humour be not lined throughout with a membrane of uniform nature, whether we call it serous membrane or not, it presents a remarkable exception to a general rule; and, if the margin of the pupil adheres to the capsule of the lens after a very brief inflammation, then does polished cartilage, of which the capsule is composed, adhere to serous membrane with which the back of the iris is covered.

"(6.) The reviewer objects to my saying that the membrane of Demours or Descemet, the elastic layer of the cornea, has not received the attention it deserves;

but let him look into the current books on anatomy and on diseases of the eye, and he will find it is generally confounded with the membrane of the aqueous humour. Even now its office is not acknowledged as a provision to preserve the correct lenticular curvature of the cornea, as the curvature of the lens is preserved by its capsule.

"(7.) The *questio vexata* of the vascularity of the cornea and other structures I cannot handle here; but I would have it understood that it is not from ignorance of the doctrines latterly inculcated respecting this matter, that I am sceptical. I have paid attention to Mr. Toynbee's valuable researches on the subject, and have profited by them; but gentlemen of the microscopic school must not expect implicit acquiescence in their views from those who require more unquestionable demonstration.

"(8.) Why should the application of a soap as a stimulant to dispel opacity of the cornea be called 'barbarous?' It is an exceedingly nice and a singularly efficient method; and I can tell those who undervalue it, that the action of alkalies on living structure is worthy of consideration. I am sure that a touch of soap is not so barbarous as the blowing of calomel and sugar into the eye, still practised by some.

"The reviewer of this little book of mine need not fear that I will regard his criticism as dictated by an unfriendly spirit. I am not such a novice, or so unreasonable, as to expect all the world to acquiesce in my views. If I have written in an authoritative way, I have some right to do so. I have now devoted more years to the study of these subjects than I care to mention, and during all this time have diligently laboured to come at the truth. Perhaps I have displayed more warmth than necessary in urging my objections to certain doctrines, but perhaps the state of the case requires it.

"I remain, Sir, your obedient servant,

"ARTHUR JACOB."

*Remarks.*—1. That adhesion may occur without inflammation, between two surfaces naturally disposed to it (as the lips of an incised wound), we believe to be a well-established fact; but that adhesion can take place between two inflamed surfaces naturally separate, without a uniting medium, we venture to think impossible, in spite of Dr. Jacob's assertion. We have never separated recent adhesions of the peritoneum or any other serous membrane, without finding a layer of plastic lymph between them, however thin this may be. If lymph be *not* the uniting medium, what is?

2. Dr. Jacob has only himself to thank for this criticism. At p. 35 of his book, he enumerates mercury as one of the means to be employed in the *second* stage of iritis; but he does not mention it among the remedies to be had recourse to in the *first*. We are glad to find that Dr. Jacob's authority is really on the side of what we regard as sound practice.

3. If the lens is *ever* in contact with the margin of the pupil, in certain cases in which it presents an unusual curvature, and presses forwards in an unwonted degree, this must surely be an exceptional phenomenon, and not a general rule. We have never been able to discern the least indication of such a condition, and should think it likely to impede the freedom and rapidity of the pupil's contraction, when the eye is suddenly exposed to intense light. The only argument advanced by Dr. Jacob, in the memoir to which he now refers, is the existence of adhesions consequent upon iritis. This fact obviously affords no proof of his position.

4. Dr. Jacob states that he has followed the ciliary nerves to the *orbiculus ciliaris* surrounding the iris, and made drawings from his own dissections. But did he follow enough *into the iris*, to be able to say that it receives a greater quantity of nerves in proportion to its size, than probably any other part of the body? Or does he believe this upon physiological evidence? Neither Weber nor Eble could trace the ciliary nerves farther than into the orbiculus ciliaris. Arnold traced a few into the iris. Bowman believes that they are prolonged into the iris in considerable numbers; but, at the same time, he notes the difficulties attendant upon their dissection, from their losing their most recognisable character, viz. their double

dark contour. Did Dr. Jacob trace the nerves from the *orbiculus ciliaris* into the iris, with or without the aid of the microscope? From the low value which he sets on that aid to vision, we presume the latter. How, then, did he manage to see things, which other expert anatomists have found it so difficult to discern? We should like him to make known his method of dissecting the iris, so as to bring its extraordinary supply of nerves distinctly into view. We suspect that he has been misled by the admitted distribution of a large number of nerves to the *orbiculus ciliaris*, and by his want of belief in the muscularity of this structure. Of course, if he does not value the evidence supplied by the microscope on this point, his "emphatic dissent" will only have force with those who are equally sceptical. Mr. Bowman speaks on this point with the fullest confidence (Lectures on the Eye, p. 53); and further informs us that of the ciliary nerves a large proportion terminates in this muscle.

5. Dr. Jacob has strangely misinterpreted his reviewer's remark upon the difference of the anatomical elements lining the chamber of the aqueous humour. What was said was, that whilst the posterior surface of the cornea is covered with an epithelium, and might be regarded in the light of a serous membrane, the front of the iris and of the crystalline lens, having no such investment, furnish "surfaces anatomically dissimilar." Are we to admit a serous membrane in the eye, having no epithelium, thus modifying the universally understood meaning of the term; or are we to believe, on theoretical grounds, in the existence of a membrane, of which no one anatomical element can be demonstrated? On this point, we stand by the high authority of Mr. Bowman, who says (Op. cit. p. 22), "It seems, therefore, incorrect to speak of the chambers of the eye as lined by a serous membrane, or of the aqueous humour as contained within a proper capsule; and I suppose that practitioners must abandon the *name* at least, of that affection which is now generally termed *aquo-capsulitis*, even if they continue to regard it as a distinct disease." It is a little remarkable that the continuity of the synovial membrane over the articular cartilage, which was so long taken upon trust, and referred to by Dr. Jacob (p. 189) as analogical evidence in favour of the continuity of a serous membrane around the anterior chamber, should now be disproved. Dr. Leidy, of whose researches we gave a short account on a former occasion (Vol. IV, p. 277), distinctly states that neither epithelium nor basement-membrane exists on the free surface of the articular cartilages of the adult, within the *circulus articuli vasculosus*; and although in the fœtal state it can be readily traced over the whole surface of the cartilage, it appears subsequently to retreat with the retiring of the surface-vessels towards the circumference, just in the same way that the *membrana pupillaris* disappears.

6. This is a point scarcely worth further discussion.

7. The vascularity of the healthy cornea has long since ceased to be a *questio vexata* with modern anatomists. If microscopic examination is not to be admitted as having decided it in the negative, how *is* it to be decided?

8. The brushing of the cornea with soap-suds was called "barbarous" by the reviewer, not because he thought that Dr. Jacob when thus employed would look like a *savage*, but like a *barber*. The printer, not understanding the joke, did not make it sufficiently apparent by dividing the word—barbar-ous. We quite agree with Dr. Jacob in the reprobation of the calomel and sugar insufflation, which could only be employed by a barbarian.

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